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SUNSPOTS AND THEIR TERRESTRIAL EFFECTS

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THE cyclic fluctuation in the appearance of spots on the Sun was first pointed out about 100 years ago by Schwabe, an apothecary of Dessau, who for many years observed the Sun on every day that it was visible and kept a record of all the spots that he saw with his telescope. The cycle is irregular in its duration; though the average duration is about 11 years, the interval measured from minimum to minimum or from maximum to maximum has been as short as 8 years and as long as 17 years. Successive maxima may be markedly unequal, some being much higher than others. From about 1645 to 1715 there was a long period of comparative quiescence. Such an effect may be produced by interference between two periodic effects that are nearly equal in amplitude but differ slightly in length of period. Attempts have been made to analyse the fluctuations in sunspot activity into a number of regular periodic terms. By assuming a sufficient number of periods a remarkably close agreement between the observed and the computed activity for 150 years or more has been obtained. But when the attempt is made to use these periodic terms for the prediction of the future course of sunspot activity, the predictions are invariably found to deviate considerably from the subsequently observed activity. The inference is that the representation by the combination of a series of regular periodic terms does not correspond to any physical reality. The cycle of activity is essentially irregular.

At the present time the Sun is somewhere near a maximum of activity. The progression of the cycle since the last maximum in 1928 is shown in the following table, which gives the means for each year of the daily area covered by spots, expressed in units of one-millionth of the Sun's visible hemisphere.

Year.	Mean Area.	Year.	Mean Area.
1928	1390	1933	88
1929	1242	1934	119
1930	516	1935	624
1931	275	1936	1141
1932	163	1937	1750*

* Approximate estimate.

It may be remarked that the total area of the Earth's surface expressed in the same units is about 172.

The rapid rise in activity from the minimum in 1933 will be noted. It is usual for the rise from minimum to maximum to be more rapid than the decline from maximum to the next minimum.

The year 1937 was one of unusually great activity, probably greater than in any year since 1870–71. One symptom of this great activity was the large number of spots that were visible to the naked eye. A spot near the centre of the disk of the Sun, with an area greater than 500-millionths of the Sun's hemisphere (or, in other words, with an area greater than three times the total surface area of the Earth), is generally visible to the naked eye. During 1937 there were seventeen large spots which reached more than double this area. Two spots, which crossed the central meridian of the Sun on 1937, July 28 and October 4 respectively, reached a maximum area of 3000 units and were the largest spots observed since January 1926, when the largest spot ever observed at Greenwich, having an area of 3700 units, appeared (Plate I). Another large spot, with an area of 3200 units, crossed the central meridian on January 18 this year, a week before the brilliant display of the aurora borealis.

It may be remarked that more than 200 years elapsed between the first telescopic observations of sunspots by Galileo and the discovery of the sunspot cycle by Schwabe. Yet the existence of such a cycle might have been discovered at any time merely from naked-eye observations. Such observations would show a series of peaks near the times of sunspot maxima, with flat intervals around the times of sunspot minima, when no naked eye spots are to be seen. The cycle remained undiscovered for so long not because instrumental equipment was inadequate, but because such observations as were made were not systematic.

It has long been known that there is a close correlation between the sunspot cycle and certain terrestrial magnetic effects. Two different ways in which this correlation is shown will be mentioned. At any one place the direction in which the compass needle points is slowly changing. There is, in the first place, a slow secular change in direction. In the year 1580, the compass needle in London

pointed about $11\frac{1}{4}°$ east of true north; the direction moved progressively westward, until in 1819 it pointed about $24\frac{1}{2}°$ west of north. Since then it has been moving eastwards again and at present it points about 11° west of north. Superposed on this slow or secular change there is a continual daily swing backwards and forwards. At sunrise, the north end of the magnet points slightly east of its mean position; by noon it points approximately in its mean position; then it gradually moves to the west towards sunset, returning again during the night, to regain its mean position about midnight. This diurnal movement of the compass needle is recorded continuously at fixed magnetic observatories. It is found that the average daily swing is not constant but fluctuates in close correlation with sunspot activity, being about twice as great when sunspots are most numerous as it is when they are few.

The second correlation is between sunspot activity and the frequency of magnetic storms. When a magnetic storm occurs, there are large and rapid variations in the direction in which the compass needle points. The needle may rapidly swing from one or two degrees on one side of its mean position to a corresponding amount on the other side. These magnetic storms are usually accompanied by considerable interference with telegraphic and telephonic communications and may upset electrical signalling systems on railways: the latter are accordingly designed so that such interference sets the signals at danger, and does not give rise to any risk of a collision. The frequency of magnetic storms fluctuates in close agreement with the frequency of appearance of sunspots.

As might be expected, the recent increase in sunspot activity has been accompanied by a considerable increase in terrestrial magnetic activity. If we consider only storms in which the range of movement of the needle was greater than half a degree, twenty-six of such storms were recorded in 1937 as compared with nine in 1936 and eight in 1935. The three largest disturbances, on April 25, 26 and 27, commenced within a few days of the passage of a large sunspot across the central meridian of the Sun.

Is the passage of a large spot across the central meridian invariably, or usually, associated with a magnetic storm? An investigation of the direct connection between the two phenomena was made a few years ago at Greenwich. The magnetic storms recorded at Greenwich during the period April 1874 to December 1927 were classified into seven grades of magnitude, from minor storms to very great storms. The average area of spots visible on the sun at the time of each storm was tabulated, (*a*) for the central region

of the disk and (*b*) for the regions near the Sun's limb. The results so found are exhibited in the following table:

Grade of Storm.	Central Region.		Regions near Limbs.		Number of Storms.
	Average Area.	Relative Values.	Average Area.	Relative Values.	
Quiet days	244	1·0	167	1·0	—
1	260	1·1	181	1·1	42
2	282	1·2	165	1·0	122
3	318	1·3	211	1·3	91
4	390	1·6	302	1·8	52
5	462	1·9	189	1·1	36
Great storms	691	2·8	251	1·5	43
Very great storms . .	1116	4·6	181	1·1	17

For comparison, figures are also given for 264 magnetically quiet days, the days used being the two days of least magnetic disturbance in each month from 1914 to 1924 inclusive, *i.e.* over one complete solar cycle.

This table shows that with increase in the magnitude of the storm there is a tendency for spots of larger and larger size to be found near the centre of the disk at the time of the commencement of the storm, but that there is no similar tendency for the regions near the limbs. It indicates a general relationship between the occurrence of magnetic storms and the state of the central region of the Sun at the times of occurrence of the storms.

The spots must not be regarded, nevertheless, as necessarily the seat of origin of the storms. Though the central transit of a very large spot is usually followed by a magnetic storm, it sometimes happens that such a spot does not give rise to any noticeable magnetic disturbance. Conversely, very great magnetic storms sometimes occur when there are no large spots on the Sun. The storm that was associated with the brilliant auroral display of January 25 last was one of the greatest that has been recorded at Greenwich, but it was not associated with any sunspot. The still greater storm that occurred on April 16 last, on the other hand, closely followed the passage of a large spot across the central meridian of the Sun's disk (Plate I).

To investigate still further the portion of the Sun's disk within which spot activity appeared to be related to magnetic disturbance, the disk was divided into zones, each extending over 26° of heliographic longitude, east and west of the central meridian, and the spots present within the several zones at the times of commencement of a magnetic storm were listed. The increase in spot area with

increase of severity of the storm was noticeable only in the three zones of longitude 0°–26° E., 0°–26° W. and 27°–53° W. Thus it appears that the source of the disturbance is situated, at the moment when the storm begins, between two days east and four days west of the Sun's central meridian. We may suppose that a disturbed area on the Sun is most favourably situated for influencing the Earth when it is on the central meridian; it follows that, as there is a tendency for a storm to commence on the average about a day after the spot crosses the central meridian, the disturbing agency —whatever its nature may be—takes something like one day to travel from the Sun to the Earth.

The period of rotation of the Sun with respect to the Earth, corresponding to the mean latitude of the sunspot zones, is about 27 days. Long-lived spots will make successive transits across the central meridian of the Sun at intervals of about 27 days, and a tendency for magnetic storms to recur after this interval is therefore to be expected. An investigation was made for the 403 storms listed in the above table. It was found that for the storms of grades 1 and 2 there was a marked tendency for other storms to follow after intervals of 27 and 54 days, but there was no significant tendency for a storm to follow after intervals of 81 or 108 days. For the storms of grade 3 there was a marked tendency for another storm to follow after an interval of 27 days, but there was no tendency for further storms after intervals of 54 or 81 days. For storms of grades 4 and 5 there was only a slight tendency for a recurrence after an interval of 27 days, and it appeared probable that this tendency was not real but merely a spurious tendency resulting from the data being insufficient to smooth out the chance of two independent storms occurring with an interval of about 27 days. For the great and very great storms, there was no indication at all of any recurrence tendency. It thus appears that the 27-day recurrence tendency is mainly a property of storms of very moderate range.

How are these results to be explained? What is the nature of the relationship between the daily movement of the compass needle, or the greater and more violent movements that occur during magnetic storms, and the sunspot activity on the Sun?

The investigation of the way in which radio waves of short wave-length travel from one place on the Earth to another proved that there existed an electrically conducting layer at a height of about 100 miles or so above the surface of the Earth. This conducting layer imprisons the waves and prevents them travelling away into outside space. When the waves reach this layer they are

reflected back towards the surface of the earth. More detailed investigation has shown that there are two or more layers, but we need not concern ourselves here with this refinement. The electrical conductivity of the layer is produced by ionization of the atmosphere in the layer, the ionizing agent being the ultra-violet light from the Sun.

It is believed that a system of electric currents circulates in the ionized layer. The way in which these currents are produced has not yet been established with certainty. One theory, which is perhaps the most probable, is that the currents are produced by convective motions of the atmosphère across the permanent magnetic field of the earth. The circulation of these currents gives rise to magnetic effects, and these must produce movements of the compass needle.

Though the details of the mechanism are obscure, the general effects are clear. The ionization will be most intense over the region of the Earth that has the Sun more or less overhead; in consequence, the compass needle is drawn somewhat towards the Sun at sunrise. The amount of its movement is naturally determined by the strength of the magnetic field produced by the current system in relation to the strength of the magnetic field of the Earth. At sunset the effect is in the opposite direction from what it is at sunrise. During the night, the ionized layer overhead gradually dissociates and the needle gradually returns to its normal position. The diurnal movement of the compass needle is therefore a consequence of the alternate lightening and darkening of the upper regions of the Earth's atmosphere as the Earth turns on its axis.

We have mentioned that the average diurnal movement increases with increase of sunspot activity, being about twice as great at sunspot maximum as at sunspot minimum. The cause of this must be a variation of the solar ionizing agency, which is more intense at maximum than at minimum activity. This variation is much greater than the variation in the longer wave-length radiation that reaches the surface. The extreme ultra-violet radiation from the Sun is entirely absorbed in the atmosphere of the Earth and never reaches the surface. We are thus forced to the very interesting conclusion that the intensity of the short-wave radiation of the Sun varies much more through the sunspot cycle than does that of the radiation of the longer wave-lengths that penetrate to the surface of the Earth.

The more intense and spasmodic disturbances, which are called magnetic storms, cannot be produced by short wave-length radiation from the Sun. We have seen that storms tend to occur when there

are spots on the central region of the Sun and to be more severe the larger the spots. If they were produced by an increase in the emission of ultra-violet radiation, a storm might occur at any time when a large spot was visible on the disk, and might last as long as the spot remained visible. But we find that a storm only occurs when the spot is in the central area of the disk. This suggests that a magnetic storm is produced by a stream of electrically charged particles, shot out from the Sun in the form of a jet, entering the upper regions of the Earth's atmosphere.

Such a jet will not necessarily be emitted in the direction normal to the Sun's surface, though on the average the direction will be normal. The investigation of the association between sunspots and magnetic storms showed that a storm might commence two days before the spot reached the central meridian, whereas, if the jet were emitted in the direction normal to the surface, the commencement of the storm would necessarily follow the central meridian passage by a time interval equal to the time taken by the corpuscles to travel from the Sun to the Earth. The average lag between the commencement of the storm and the central meridian passage of a spot being about one day, the mean speed of the particles must be of the order of 1000 miles a second. The fact that a storm sometimes precedes the central passage of a spot shows that the jet is sometimes sent out in a direction considerably inclined to the normal to the surface.

Confirmation of the corpuscular origin of magnetic storms can be obtained from the comparative study of the disturbances recorded at magnetic observatories widely distributed over the Earth. It is found that the intensity of the disturbance increases from low to high latitudes and that it is related to magnetic latitude, not to geographical latitude. The north magnetic pole is situated at about latitude 70° 40′ N., longitude 96°·5 W. The disturbance increases up to a distance of about 23° from the magnetic pole. As the magnetic pole is approached the intensity of the disturbance decreases somewhat, though still remaining considerable.

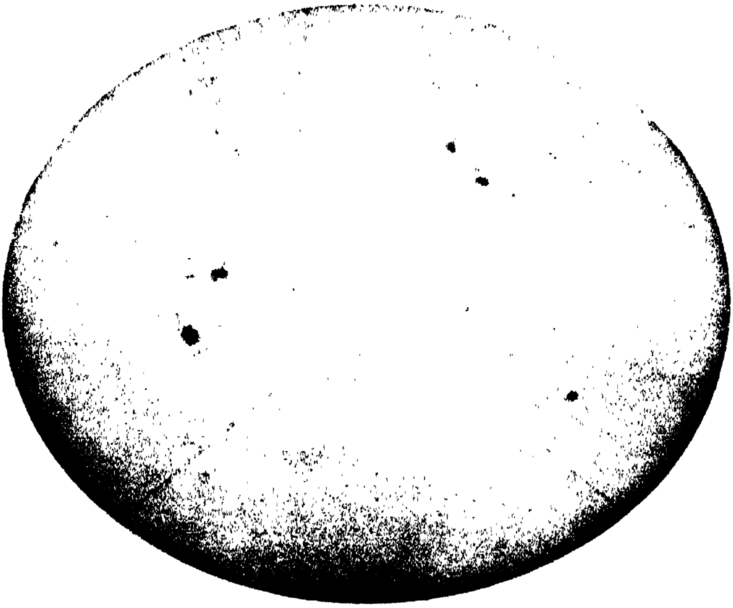
The disturbance is greatest precisely in the regions where the frequency of appearance of the polar lights or aurora attains its maximum, *i.e.* along a small circle of angular radius 23°, with its centre at the north magnetic pole. (A similar relation doubtless holds for the southern hemisphere, but the relevant data are very scanty.) It is significant that the frequency of appearance of auroræ fluctuates with the sunspot cycle, auroræ being most numerous when sunspots are most numerous, and that brilliant displays of auroræ are usually accompanied by strong magnetic disturbance.

The unusually fine display of aurora that occurred on January 25 last will be remembered by many readers; this display was accompanied by the most intense magnetic storm recorded at Greenwich since 1909. A comparison of the observations of the aurora with the magnetic records showed that the large movements of the magnet coincided exactly with the times when the auroral light was most intense. The more violent storm that occurred on April 16 was not accompanied by an auroral display in this country. But the large movements of the magnet occurred during daylight hours. In North America, where these movements came shortly before sunrise, a brilliant display of aurora was seen.

A close relationship therefore exists between magnetic storms and the polar lights. The aurora is believed to be caused by streams of electrically charged particles entering the upper regions of the Earth's atmosphere. Because the particles are electrically charged, their motion is influenced by the magnetic field of the Earth, and instead of the particles being uniformly distributed in the upper atmosphere of the Earth, they are drawn towards the polar regions. On entering the atmosphere the particles collide with molecules, which may either be excited or ionized. When an excited atom returns to its normal state, or when an electron, ejected when a molecule was ionized, recombines with an ion, radiation is emitted. It is in these ways that the auroral light is produced. The height of an aurora can be determined by photographing it from two stations several miles apart, using the stars shown on the photographs as reference points. It is found that the aurora is usually produced at a height of at least 60 miles above the Earth's surface.

The full explanation of all the features shown by auroræ and by magnetic storms has not yet been possible. There is evidence that strong electric currents flow around the auroral zone, at a height of 60 or more miles, and that their intensity is of the order of one million amperes. It is not surprising that with such large currents moving in the upper atmosphere the associated magnetic effects may be considerable.

A striking feature of intense magnetic storms has not yet been mentioned. They usually commence with remarkable suddenness, the needle giving a sudden kick. The time of commencement being so well defined, an accurate comparison of the times of commencement at different points of the Earth's surface is possible. The extreme observed differences in the times of commencement are usually not more than about thirty seconds; as this includes the errors of the clocks controlling the timing of the magnetic records and the errors of reading the time from the sheet, we may say that



[Greenwich Observatory.

Sun. 1938, April 14.

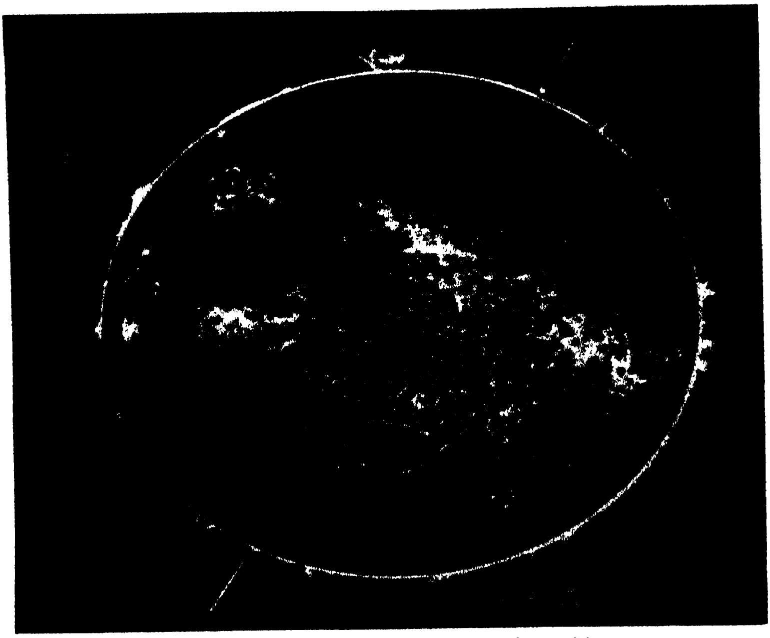
The magnetic storm on April 16 was associated with the spot group in the upper portion of the photograph.

[Greenwich Observatory.

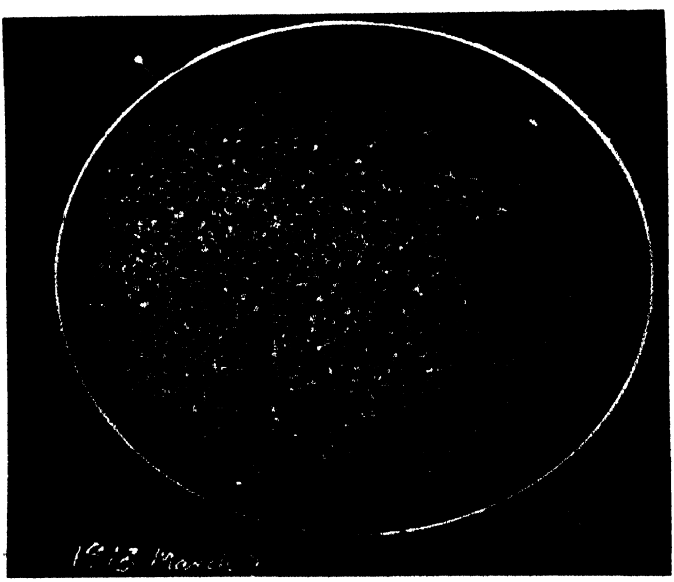
Large Sunspot. 1926, Jan. 20.

This is the largest sunspot ever observed at Greenwich.

PLATE II



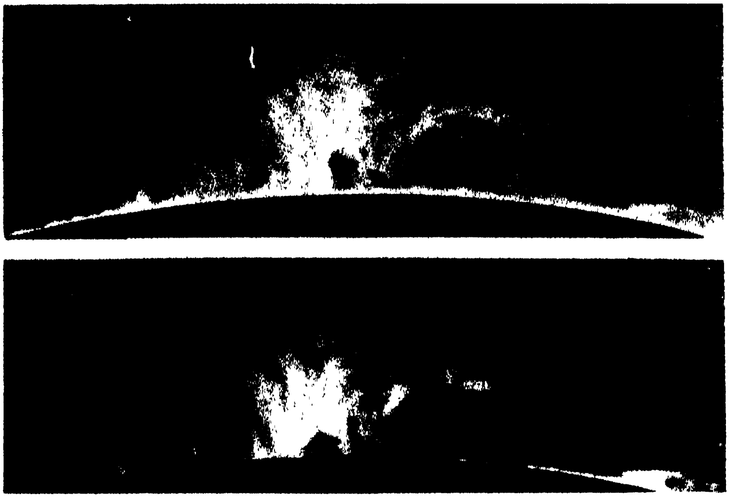
Sun in Calcium Light. 1918, Jan. 11.
(Spot Maximum.)



[*Kodaikanal Observatory*

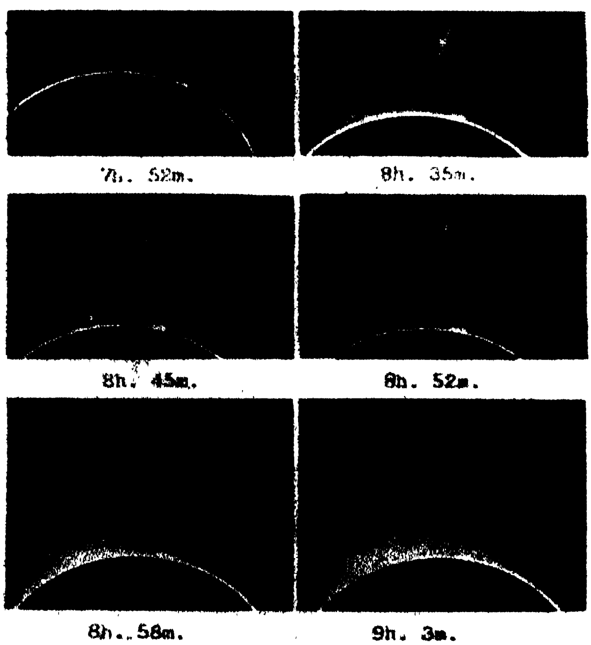
Sun in Calcium Light. 1913, March 7.
(Spot Minimum.)

Compare the spots and prominences at maximum with their absence at minimum. The ends of the axis of rotation of the Sun are indicated in each photograph.



[*Yerkes Observatory.*

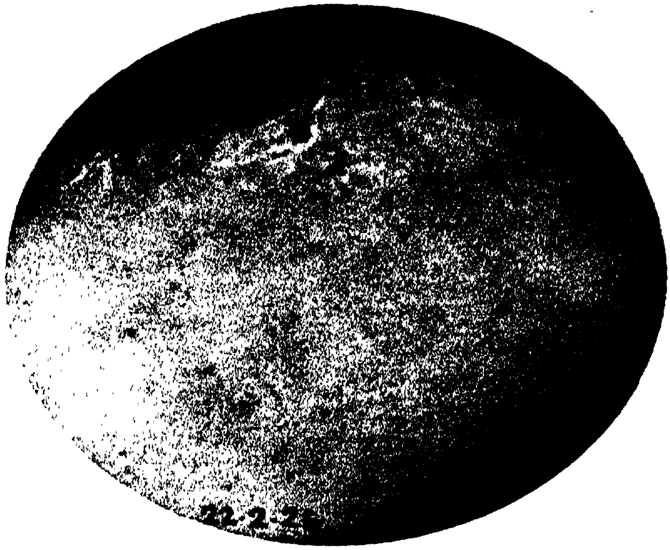
Solar Prominence. 1910, Oct. 10.



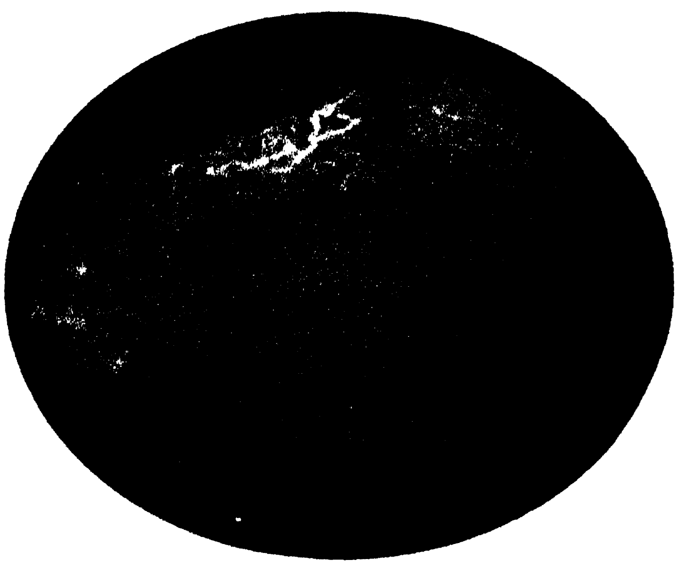
[*Kodaikanal Observatory.*

Eruptive Prominence. 1928, Nov. 19.

PLATE IV



Bright Eruption. 1926, Feb. 22.
8h. 8m.



[*Kodaikanal Observatory.*

Bright Eruption. 1926, Feb. 22.
9h. 4m.

Note the increased brightness in lower photograph.

a magnetic storm begins practically instantaneously at all points of the Earth's surface. This suggests that the boundary of the stream of particles is sharply defined.

Where do these streams of charged particles originate on the Sun? There is not a direct correspondence between sunspots and magnetic storms. A large storm can occur when no spots are to be seen, and, conversely, a large spot may cross the central meridian without a magnetic storm occurring. There is, nevertheless, a relationship, for, as we have seen, there is a very strongly marked tendency not only for storms to be associated with definite spots but also for the more intense storms to be associated with the larger spots. We must also bear in mind the general tendency for storms to recur after successive intervals of 27 days when the same portion of the Sun is again presented to the Earth.

The necessary conclusions seem to be (i) that there are disturbed regions on the Sun that are effective in producing magnetic disturbances on the Earth; (ii) that these regions are often, though not invariably, found in the neighbourhood of sunspots; (iii) that these regions rotate with the Sun; (iv) that the disturbing effect may persist for several rotation periods.

The existence of definite disturbed regions on the Sun, associated with magnetic disturbances on the Earth, is well illustrated by recent magnetic disturbances. Of 35 storms recorded at Abinger since the beginning of 1937, 24 have occurred when the longitude of the central meridian of the Sun has been between 60° and 180°. The seven greatest storms have all occurred when the longitude of the central meridian was between 120° and 180°. During the last 15 months or so, therefore, magnetic disturbances have been predominantly related to a definite region of the Sun. The distribution of large sunspots in solar longitude during the same period has been, on the other hand, more or less of a random distribution.

The sunspot cycle is but one symptom of a cyclic sequence of activity which the Sun undergoes. Another symptom of this activity is found in the prominences. When these occur at the edge of the visible disk they appear as great jets of incandescent matter, extending outwards from the Sun for thousands of miles; they can be seen in projection against the bright disk in photographs of the Sun taken in hydrogen light, when they appear as long black filaments. The prominences wax and wane in number in close correlation with the spots (see Plate II).

The corona or "glory," which extends around the Sun to a distance of a couple of million miles, is in some way influenced by the general solar activity. Near sunspot minimum, it shows large

streamers in the equatorial directions but at the two poles of the axis of rotation of the Sun a number of short tufts appear. Near sunspot maximum, there are large streamers in all directions and the polar tufts are no longer seen.

The variation in frequency of appearance of prominences and the changes in shape of the corona, occurring in close correlation with the variation in numbers of sunspots, indicates that these phenomena are all in some way related to a sequence of changes on or within the Sun, whose nature and interpretation is not at present understood. We may speak of a cycle of activity on the Sun; we do not know the physical nature of this activity, but the sunspot cycle and the variation in frequency of appearance of prominences are two of the ways in which it is revealed to us. We have seen, moreover, that we must suppose that there is a considerable variation during this cycle of the emission of ultra-violet light, the emission being greater at maximum activity than at minimum activity.

The prominences give us, at times, direct evidence of matter in the process of being violently ejected from the Sun. A prominence may exist in a more or less stable condition for some months and then undergo a violent eruption, the prominence being rapidly dissipated in a few hours. At such times, jets of gaseous matter can be seen moving away from the surface of the Sun with speeds very much higher than the velocity of escape. It is evident that such matter cannot fall back to the surface of the Sun but that it must escape into space. If such a jet of matter encounters the Earth's atmosphere, the presumption is that a bright aurora will be seen and that a magnetic storm will occur (Plate III).

We have no direct evidence of the dissipation of a prominence in this way except when the prominence is at the edge of the disk. It is not impossible that methods may in time be found for seeing such happenings when they occur in the central region of the disk. If that should be possible, we may be able to predict the appearance of a bright aurora or the occurrence of a large magnetic storm.

Sunspots and prominences are often associated, but not necessarily so. Sunspots only occur in a belt of latitude extending some 35° either side of the Sun's equator, whereas prominences may appear in any latitude. We should like to know whether the streams of corpuscles, which cause auroræ and magnetic storms, are always produced by the dissipation of prominences. At present it seems that there can be areas on the Sun which may be the source of intense disturbance, but which are not associated in any way

with sunspots and do not give us any direct evidence of corpuscular emission.

We must refer again to one point of interest that emerged from the study of magnetic storms. We saw that the recurrence tendency was most clearly shown by storms of moderate intensity, but that the great storms and the very great storms do not recur. Two possible explanations may be given. We can suppose that the corpuscular streams may be emitted in the form of a concentrated narrow jet, which would give rise to a great magnetic disturbance, or in the form of a wide-angled jet of low intensity, which would produce only a moderate disturbance. With a very narrow jet it would be very much a matter of chance whether the earth encountered it or not, and it might be supposed that the chances are high that, after a complete rotation, the emission of the jet might be in a slightly different direction so that the Earth would be likely to miss it. With the wide-angled jet, on the other hand, the Earth would be unlikely to miss it at its return. If this explanation were correct, we should expect that the very great storms would be of shorter duration than the storms of low intensity, because the Earth would pass through the narrow jet in a shorter time than it would pass through the wide-angled jet. There does not seem to be any evidence of the more intense storms having a shorter duration. The second possibility is that the emissions that produce the largest magnetic storms are short-lived, and never survive for the length of a complete rotation of the Sun ; the emissions that provide the smaller storms, being of moderate intensity, may persist for a complete rotation or, possibly, for several rotations. If, as seems fairly probable, this is the correct explanation, then we may be justified in attributing the very great storms to the dissipative break-up of a prominence, which, as we have seen, occurs with great suddenness ; the smaller storms would then be due to more continuous emissions, possibly from prominences and possibly from other disturbed areas.

We have here a further indication that sunspots are not directly responsible for magnetic storms. For the largest storms tend to be associated with the largest spots ; the largest spots have a strongly marked tendency to persist for several solar rotations, whereas the largest storms show little or no recurrence tendency.

Solar activity is shown by another phenomenon, to which we have not yet referred, which is known as a bright eruption. It is only within recent years that these bright eruptions have been studied in detail. The reason is that they can very rarely be seen in an ordinary telescope or in a photograph of the Sun taken with

integrated light. The first bright eruption ever recorded was observed by Carrington on September 1, 1859, when a small portion of the disk, near a sunspot, was seen to become intensely bright. The phenomenon was of short duration and ended almost as suddenly as it began. It must have been of a very exceptional character, for the observation has never been repeated. The eruptions are best recorded, like the prominences, in photographs in the light of hydrogen or calcium vapour. The spectrohelioscope, designed by Dr. Hale in 1936, enables the Sun to be observed visually in the light of hydrogen and has made possible the visual study of these eruptions. There is a scheme for co-operation amongst observatories that are equipped with spectrohelioscopes, arranged under the auspices of the International Astronomical Union, by which the Sun is kept under fairly continuous observation, provided that weather conditions permit. It has only been during the last few years, when solar activity has been rapidly on the increase, that the bright eruptions have been systematically studied.

The frequency with which these eruptions occur correlates closely with the frequency of sunspots. In fact, the eruptions are nearly always associated with spots. They have certain well-defined characteristics. Appearing very suddenly, they have a rapid rise to maximum intensity, which is usually reached in from 5 to 10 minutes. They then die away less quickly and the eruption has usually ended within 40 or 45 minutes of its commencement. The duration of the eruptions of less intensity is even shorter. Sometimes, two or more bright eruptions will occur practically simultaneously in different groups of sunspots, and this appears to happen more frequently than can be explained by mere chance. The area covered by the eruption ranges from small patches, equal in size to that of a small sunspot, to large patches equal in size to that of big sunspots (of the order of several times the area of the Earth's surface). Closely associated with the bright patches, matter can often be seen moving rapidly outwards from the Sun, with velocities of the order of 250 miles a second, usually followed by inward motion, as though a cloud of gas had been shot outwards with a velocity lower than the velocity of escape, which somewhat later falls back to the Sun's surface (Plate IV).

The nature and cause of these eruptions is not yet understood. Besides their intrinsic interest, they have a special interest in that they are closely associated with sudden fadings in short-wave radio transmission. Short-wave radio signals (*i.e.* of wave-length in a range of about 15 to 40 metres) will be coming in with normal audibility when the signals may commence suddenly to fade and in

the course of a few seconds become completely inaudible. There is sometimes, however, an increase of noise field (known as " fizzlies ") just prior to the fading. The drop in signal strength is sensibly simultaneous on all routes affected but is only observed on those waves that traverse a daylight path. Thus, for instance, if fading should occur near midday at a given station, all routes would be effected ; if, however, it should occur near sunset, the fading would be observed on signals coming from the west and not on those coming from the east ; conversely, if it occurred near sunrise, it would be observed on signals coming from the east and not on those coming from the west. The highest frequency signals appear, in general, to be affected to a lesser degree and for a shorter time. The excessive attenuation may last for from 10 to 45 minutes, and is followed by a period of gradual recovery, which may extend over one hour or more.

Complete records of the times of radio fadings at various stations are available ; unfortunately, continuous observation of the Sun is impossible and therefore a complete correlation between bright eruptions and radio fadings cannot be made. The study of these phenomena has shown the importance of negative information ; suppose a radio fading has occurred at a time when the Sun is due, according to the international scheme of co-operation, to be under observation at a certain observatory. If the appearance of a bright eruption was not reported by that observatory, it is important to know if the weather conditions were such that the Sun was actually being observed at the time when the radio fading occurred. It is therefore essential, for the proper statistical discussion of the correlation between the two phenomena, that each observatory making these observations should provide a complete record of the times during which the Sun was actually being observed.

Nevertheless, despite the incompleteness of the solar observations, sufficient material is already available to establish, beyond the possibility of doubt, the close relationship between the solar eruptions and the radio fading. The most reliable data are provided by those eruptions whose commencement was actually observed. Considering these only, the number of radio fadings which commenced within a few minutes of the observed commencement of an eruption is far larger than can be explained by chance coincidence. On the average, a radio fading commences about 7 minutes after a bright eruption is seen to commence. The eruptions that are associated with fadings are not limited to the central part of the disk ; an eruption on any part of the visible hemisphere of the Sun may give rise to a fading. The conclusion is that the fadings

are not produced by a corpuscular emission from the Sun, as in the case of magnetic storms, but are caused by something travelling from the Sun with the speed of light. It may be noted that not every eruption that is observed gives rise to a fading ; the eruptions vary considerably in intensity and the observers classify them according to their intensity on an approximate scale of 1 (for the smallest and least intense eruptions), 2, 3 (for the largest and most intense eruptions). It is noticeable that the cases where the association between the eruption and the fading is definitely established include a high percentage of the most intense eruptions (intensity 3).

It is possible that the fadings are produced by a considerable increase in the emission of ultra-violet light from the area in which the eruption occurs. The radio ceiling, which normally reflects the radio waves back to Earth, is considerably disturbed and there is no longer a regular reflecting surface. The surprising feature is that the increased emission of ultra-violet light from an area that is so small compared with the area of the visible hemisphere of the Sun is sufficient to produce the complete and rapid fading that is observed.

We therefore find in unmistakable fashion terrestrial evidence of the Sun's cycle of activity shown in the polar lights, magnetic storms, the daily movement of the compass needle and radio transmission. We have been led to infer, moreover, that the emission of ultra-violet light from the Sun must be greater at maximum activity than at minimum. This suggests the possibility of the weather being influenced by the cyclic changes occurring on the Sun. The question is often asked whether sunspots influence the weather. If a big group of spots appears on the Sun and coincides with some unusual weather phenomenon, there are never wanting people who will assert that the sunspots are responsible. It makes no difference what the particular freak of the weather may be—a great storm or a spell of drought, intense heat or great cold—the sunspots are sure to be blamed.

A great deal of time has been devoted by many people to the investigation of possible correlations between sunspot activity and various weather phenomena—rainfall, cloudiness, temperature, wind, etc. No clearly marked correlations have been established. This is, perhaps, not surprising. Weather is a phenomenon of great complexity ; the distribution of land and water and the local topography play a great part in producing the weather that we, on the surface of the Earth and at the bottom of an extensive atmosphere, experience. If we could obtain regular observations

at a great height in the atmosphere, where surface topography ceases to produce any effect, we might find well-marked correlations with the Sun's cycle of activity.

As the attempts to correlate sunspot activity with various weather phenomena have not given any very definite results, it is a matter for some surprise that a clearly marked correlation is shown in the spacing of the annual rings of certain trees. Professor A. E. Douglass, of the University of Arizona, has found the sunspot cycle to be well shown in the annual rings of the hard underwoods and the giant sequoias of California and of the Arizona pines. He found that there were wide variations between the spacings of the rings of trees taken from forests in different regions, but that the trees from one area showed the same general spacing pattern. The growth is most rapid, and the rings are most widely spaced, at times of sunspot maximum. By starting with the outermost rings of trees just cut down and working backwards, the time spacing pattern was joined on to that of trees cut down hundreds of years ago. These were similarly joined on to timbers from still older trees and ultimately the record was carried back, in the case of the sequoias, for more than 3000 years. By this method, Professor Douglass has succeeded in dating many old Hopi and other Indian ruins. The accuracy of his conclusions received a striking confirmation. Between about 1645 and 1715 he found that there was very little indication of the 11-year cycle. About the same time Maunder, who was then in charge of the solar department at the Royal Observatory, Greenwich, was investigating old records of sunspots and found that there was a great dearth of sunspots during this particular period. He wrote to Douglass to tell him of this and to remark that, if there were any real correlation between the spacing of tree rings and the sunspot cycle, evidence of a cyclic variation in spacing should be lacking during these years. Maunder did not know that Douglass was already puzzling over this very fact, which he had already found.

The spacing of the tree rings is presumably dependent upon a combination of a number of different meteorological factors: the amount of rainfall, the temperature, the amounts of sunshine and cloudiness, and the intensity of the ultra-violet radiation received from the Sun are no doubt all contributory factors. The tree-ring has the advantage of showing the integrated effect of some particular combination of these factors over a period of a year, or at least over the portion of the year when growth is taking place. In tree-rings we have therefore some definite indications that meteorological conditions are not entirely independent of the sun-

spot cycle. The failure to find conclusive evidence of correlation in the case of individual phenomena, such as rainfall or temperature, may be due to the fact that our records do not extend over a sufficient number of sunspot cycles for the many other phenomena that contribute to produce the weather to be averaged out. We may compare a parallel instance; the Moon has a small influence on the height of the barometer, but the effect is so small in relation to the variations in height that are produced by other causes that the analysis of the records of barometric height for a single year would not reveal it. By dealing with a large number of years, however, these other and greater effects can be averaged out and the lunar effect clearly shown. So, perhaps, we may hope that in the course of time the manner in which the varying activity of the Sun affects rainfall, temperature, the amount of sunshine and cloudiness and other weather phenomena may be clearly revealed.

THE ANTIQUITY OF RECENT MAN

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It seems probable that present-day man is, as the knowing young woman remarked about her newly acquired possession, "a very old antique." That man as we are familiar with him in the form of ourselves and one another has existed for long periods of time and may have omitted all gradations and steps in his origin and sprung full-fledged from the loins of some hairy ape or monkey is perhaps a startling notion. Indeed, the idea has always seemed absurd to most anatomists and others. Always when thinking about the origin of man they have supposed that there was a sort of lower shelf represented by the late-lamented Mok and his expectant bride, Moina, the splendid gorillas of our Zoo, and an upper shelf holding us and all the fine people of the world of the present day. Between these two there would be a large series of gradations, many collaterals, and others—perhaps unfortunate bad starts, soon to be extinct—but on the whole all grading into the recent type of man, and depicting for us the road evolution has followed in changing over from the monkey-ape level to the modern human level.

It may be long before we can speak with much confidence of our knowledge of the actual events that have occurred in the anatomical remodelling of our monkey ancestor into man. We are certain that such an event has occurred. Because of the great similarity in the anatomy and the close chemical resemblances exhibited between man, and especially the chimpanzee and gorilla, it has been inferred that the point of origin in time and its level in the biological scale must be somewhere close to the stock which produced these same chimpanzees and gorillas.

The suggestion contained in the opening remarks of this article has perhaps already aroused the suspicion in the reader's mind that these assumptions are not well founded, and that perhaps the emergence of man from his low relations was not condemned to the inevitability of gradualness. To some extent such suspicions

may prove not unreasonable, because certain discoveries compel us to believe that recent man must have existed when other types which we looked upon as links between man and the great apes were also stalking the earth. Obviously people living contemporaneously cannot be ancestral to one another. It is therefore proposed that we briefly consider the two hypotheses, *viz.* that man has slowly emerged from the primate stock, gradually shedding his brutal aspect, enlarging his brain, and taking on the humanity of *Homo sapiens* ; or that man started abruptly, that in the ancestral line there was a period of great instability and change in the hereditary factors, and by mutation many new types were evolved. Some, if brain size is the principal criterion, would be classed as low and others as high. The high ones would contain big-brained men who retain in the face and jaws some resemblance to the more beast-like features, and other big-brained men who assumed at once the more refined features of recent man.

The notion of a gradual emergence of man seems the more reasonable, and has exerted even a seductive influence upon the minds of anatomists, few of them being able to contemplate any other view consistently or for long. This is shown very clearly by their behaviour whenever a new fossil has been discovered. The discoverer has been unable to resist the temptation of asserting that his fossil, if ape-like, presented all sorts of human characters, and, if human and clearly modern in character, that it possessed all sorts of simian characters more or less hidden and elucidated only by minute examination. It is not merely the scientific predilection for stating every hypothesis possible and then trying to find the right one that has prompted us to put bluntly the two possible modes by which man might have emerged from the Primate stem. There is some justification for examining these hypotheses because there have been discoveries of human bones, which do not differ from modern man, at geological levels which make them as ancient, if not more ancient, than other fossils which differ from present-day man and resemble more than he does the existing great apes.

In making a brief survey of these discoveries we must bear in mind that both ape and human fossil bones are extremely rare. Fossilisation can never overtake more than the most minute percentage of living things, and some may obviously never be fossilised at all. Arboreal primates and man from their very way of life can offer but the fewest opportunities for mineralisation of their bones after death. The chances of fossilisation being extremely remote, fossilisation when it occurs will not reveal whether it happened early or late in the history of the type. We could only

infer that if we had an abundance of examples. In the making of maps to show the relationship believed to exist among fossil apes and men, and recent apes and men, the anatomist has prolonged or diminished the duration of the existence of a type accordingly as he has deemed it more primitive or more complex than another found at the same level. If two fossil men are found at the same geological level, and one has a large brain and the other a small brain, invariably it will be shown on a genealogical map that the man with small brain emerged much earlier from the common stock than one with the big brain. This, of course, is an anatomical inference drawn so because of certain preconceptions that the evolutionary process must proceed by gradations. It is not founded upon any knowledge got from palæontological evidence.

Fossil Types

A brief review of the salient facts about fossil man might help to make the problem more comprehensible. In 1894 Dr. Eugen Dubois discovered in Java a fossilised skull cap, a fossilised femur, and some teeth. Of these the important find is the skull cap. The femur, which is indubitably human, probably belongs with the skull cap, but about this there cannot be absolute certainty. There has been much discussion about the antiquity and geological level of these finds. Following the conclusion of Abel, we may accept them as belonging to the Lower Pleistocene. The skull is extraordinarily flat, so that the creature is almost without a forehead. The brow ridges overhang in a brutal sort of way the cavities for the eyes. Behind the brows there is a very obvious constriction or waist in the skull. The skull is ape-like and recalls in many ways the form of the acrobatic gibbon. That it cannot be any sort of gigantic gibbon is shown by the fact that its cranial capacity, which is about 900 c.c., is twice that of the largest brain of any anthropoid recent or fossil. On the other hand, the average cranial capacity of a modern man in Europe is somewhere around 1400 c.c. Though *Pithecanthropus erectus*, the name given to this very remarkable fossil, has doubled the anthropoid, he has by no means reached the human level in brain capacity. As already remarked, if the femur found near this skull belongs to it, then there can be no doubt that Pithecanthropus was a human being. Pithecanthropus may be regarded as a primitive man who shows us the way we may have travelled from the anthropoids.

Sinanthropus pekinensis was discovered in China by the late Davidson Black in 1929. A great many specimens of this type have been found, and an extensive literature has already grown

up describing and discussing them. Unlike Pithecanthropus, whom Sinanthropus resembles so closely, not only the skull but much of the face is also known. The nose, for instance, is flat like that of a negro. The skull has a fuller forehead than Pithecanthropus, but the same postorbital constriction. The jaw, in the arrangement of the dental arcade, in the absence of the chin, and the possession of small incisor teeth, is congruous with the humanness of the skull. The cranial capacity, which is about 1000 to 1100 c.c., exceeds that of Pithecanthropus, but there can be no doubt that these two forms, *Pithecanthropus erectus* and *Sinanthropus pekinensis*, are so closely related that living together in the Lower Pleistocene they form one type, and as some anatomists have remarked, they give undoubted evidence of the origin of man from some indubitable member of the primates.

Homo heidelbergensis came to light in 1908, and is generally known to us as the Heidelberg jaw, because this is all that we have of him. It belongs to the lower or Middle Pleistocene, and its possessor probably lived in the second interglacial epoch. The jaw is distinct from all other known jaws, though the shape of the dental arcade and the teeth, especially the arrangement of the molar cusps, declare that it is essentially human. The jaw is large and massive, the ascending ramus being unusually impressive. The chin is completely absent, and where the two halves fuse together, the resemblance to the anthropoid jaw is especially close. This specimen is unique.

Homo neandertalensis has been known since 1858. He belongs to the Upper Pleistocene, and is more recent than the types which have just been mentioned. He has been found in Palestine, Europe and Africa (*Homo rhodesiensis*). By some he has been claimed as the direct ancestor of modern man. His anatomy, and there are many examples now to attest the description, has proved on the whole very uniform, though there is an interesting exception to this discovered by Sir Arthur Keith amongst the fossils from Palestine. The man had an oblique forehead and a flat skull, but the cranial capacity was high. On the average this was about 1300 c.c., but in one case—the La-Chapelle man—it reached 1600 c.c. Many of the features of this type are recalled by the Australian aborigines of to-day, and though on the whole the gap between Australian aborigines and, say, the European type, is less than the gap between them and the Neanderthal type, yet the similarities have been commented upon by everybody including the great Thomas Henry Huxley. In having no chin he differs from all recent men, and his short occipital bone was correlated with a thick neck. His

slightly bent knees, as indicated by the thick extremities of the bones that enter into the joint, indicate a shuffling kind of thick-set short man with a big brain. If the increase of the bulk of the brain is the real story of man's evolution, then obviously this big-brained type had gone a very long way to realising the greatest accomplishment of the human race. For when the apparatus had been devised which gave man his incomparable intelligence, what followed needed only time for its accomplishment. We include along with this Neanderthal type the South African specimen already mentioned as *Homo rhodesiensis*. He has some differences from Neanderthal man and some resemblances with Pithecanthropus, but in general he belongs with these flat-headed, beetle-browed, chinless men.

These three, Pithecanthropus, Sinanthropus and Neanderthal, form a series which agree in certain special anatomical features which have been mentioned in the descriptions given above. They pass from a cranial capacity of, say, 900 c.c., and then achieve about 1100, and finally reach and even exceed that of modern man. Human anatomists are more hesitant, but palæontologists, who are more expectant of finding these sequences, feel no doubt that this succession in the emergence of man has happened so, and these are the precursors of modern man, representing the stages through which he passed.

There is nothing intrinsically improbable in this view. Our biological knowledge is not sufficient to assert that it could not happen. The difficulty in feeling content with this view arises because in sharp contrast to these fossil types others have been discovered which are in no way different from modern man, and which are as old and even older than those just described. It has already been mentioned that age is determined from the geological level at which the fossil is found. The fossil might have been an example from the group to which it belongs at a phase when that group was quite young or when it was quite old. If we regard the probability of fossilisation as greatest when the group was most numerous we can, in the absence of any other available consideration, assume the time at which the fossil was found represents its age relative to any other like fossil.

The gradual emergence of man from the anthropoids is attested then, it is alleged, by a series of heavy-browed, flat-headed, chinless men, but with brain size increasing until it equals or exceeds that of modern man. The other aspect of the matter is illustrated by a series of fossils which have been found in various parts of the world, but curiously with quite uncommon frequency

in England. A series of very ancient fossils has been found which attests the fact that the modern Englishman, so far as his anatomy goes, extends backwards into the past to a time when in other countries man was distinguished with difficulty from the ape.

These finds in England happily are supported by others elsewhere, such as those of Dr. Leakey in Africa and those of Sir Arthur Keith amongst the Palestinian fossils. In the skeletons from Palestine which resemble in most particulars the Neanderthal type, Sir Arthur has made the interesting discovery that when they do vary, they vary in the direction of modern man. For instance, amongst these are fossils which do not possess the beetling brows. These observations reveal the genetic make-up of the phylum from which modern man has come. If the germ-plasm already contains the characters which, when assembled together, give the modern man his especial features, then, of course, the problem becomes one of finding out the possibility of assembling these and omitting or suppressing those which do not enter into his make-up. The most famous of these is Piltdown Man, *Eoanthropus dawsoni*, described by Sir Arthur Smith Woodward in 1913. The age of this fossil has been fixed as Early Pleistocene or even earlier. For this reason it has been called the dawn man, the first inhabitant of England, and perhaps not only the first of our kind of man, but the first man in the world, in fact the old Adam. The skull fragments when fitted together differ from modern man only in one particular, namely, the thickness of the bones. They are approximately twice as thick as the same bones in a modern skull. The cranial capacity is high, giving a brain volume of at least 1300 c.c. Near these skull fragments there was found a fossilised jaw. This jaw resembles most closely that of a chimpanzee and looks incongruous with the skull. Because in general it has seemed to palæontologists unlikely that a fossil anthropoid would be found so far west in Europe at this time, and because it has seemed to everybody so improbable that a fossil man and a fossil anthropoid should lie together in the same fossil bed, everyone has felt it incumbent to try to fit this jaw to the skull. If we can leave the jaw on one side it is possible to assert that a modern man with a thick skull turned up in the Early Pleistocene in England. A second similar fossil without any trace of a jaw has now been found. Its date is known apparently beyond all peradventure, for the skull was photographed as it lay embedded in its stratum. It is as old or older than the Piltdown fragments. This second skull, the Swanscombe, was found by Mr. A. T. Marston in 1937, and the pieces in all respects except for thickness belong anatomically to a modern skull. When the foundations of Lloyds

Bank, in Leadenhall Street in the City of London, were dug out, a skull was found there by Mr. W. R. Dawson and handed over to the late Sir Grafton Elliot Smith for study. Dr. Matthew Young has just recently published a full and detailed description of this specimen. This skull is of an antiquity only slightly less than that of the Piltdown fragments, and is asserted by its describer to be a modern skull in all its anatomical features. In addition to these three, another similar skull of similar date was found some few years ago at Bury St. Edmunds. There are possibly others, but their dating is too uncertain to justify any reference to them at this moment. Thus there seems to have been from these several fossils a human population in this island which was of the same date in time as Pithecanthropus and Sinanthropus, and earlier than the Neanderthal man of Europe. It was a form which, in the anatomy of the skull at any rate, differed in no way from man of the present moment except that perhaps some of them had rather thicker skulls than usual. We do not really know what to make of the Piltdown jaw, but the cranial capacity of the early Englishmen was well up to present-day level, and the beetling brows were absent. Clearly if these things are so, the view previously advanced that Pithecanthropus, Sinanthropus and Neanderthal man form a series leading from the ape up to modern man, need not be true; and the problem is presented that modern man has an antiquity as great as any of these fossils. Why should modern man not be directly descended from a lower primate stock?

Antiquity and Genetics

This discovery, that recent man has a vast antiquity, in fact greater than any other extinct variety, most anatomists have always tried to get round or minimise by making all hominid fossils carry pithecoid features which are absent in the present-day man. It seems to imply the happening of the incredible that modern man assumed his present form immediately upon emerging from the primate stock. His upright form, his gait, his chin, his big brain, his small face, his long legs are sudden acquisitions. We must, before we dismiss this notion as absurd, examine from an anatomical standpoint the nature of the differences between men and the anthropoids.

Clearly the extent and the nature of the differences between man and the anthropoid must bear on the probability of a quick derivation. The leaps and mutations involved must not be wildly outside the limits permitted by our existing knowledge of heredity and genetics. If we could show that many of the so-called human

features are already to be found in some degree amongst members of the primates, a possibility arises that the assemblage we call human could be separated from all others and so give us man without or with a minimum of gradual evolvement.

Related to the very beginning of the primate stock, presumably existing in its present form already in the Eocene, is the ghostly Tarsius spectrum. Venerated and feared by the natives of Borneo as though intuitively they guessed its significance for the coming of man, it interests us because it differs from all other primitive members of the primate series in the large number of anthropoid characters that it bears. There is no point in enumerating them for the sake of this article, but they have excited the interest and speculation of anatomists for nearly half a century, and one distinguished anatomist plunged boldly and claimed this Tarsius as a living example, perhaps the nearest of all living progenitors, of the stock from which man came. We need this ancient animal, with its mosaic of reptilian, primitive mammalian, monkey, anthropoid, and even human characters, that takes us right back to the Eocene and enables us to assert that so-called human characters are present in the primate germ-plasm from its beginning.

We used the word mosaic deliberately for two reasons, to describe certain type animals and to recall Mendelian unit inheritance. Mendelian inheritance implies a particularistic inheritance. The make-up of the mature individual in inheritance is resolved into a series of unit characters, each of which is carried by a gene as though it were a packet. These genes are strung like beads along the chromosomes, strings of nucleo-proteid which can be studied under the microscope, and whose behaviour establishes very firmly the Mendelian ideas about heredity. This makes the germ-plasm represent a mosaic of characters, the harmony of the whole arising out of the way in which the parts fit together, and it is this special aspect that we need for our purposes in this discussion. We can leave out other aspects of Mendelian inheritance such as linkage, crossing over, multiple action of the genes, delayed action of the genes. We need only to determine which of the genes or characters we require are already present in the primitive stock, or what may have arisen by mutation, the only way we so far know by which the gene alters its contribution to the heritable make-up. The probability that some particular assemblage of genes might be handed on and not some other possible assemblage, can be approached mathematically. The chance that any particular assemblage when handed on will survive in its recipient and be perpetuated by descendants can also be considered mathematically and biologically. All we wish to do

at the moment is to see if these genes are present in the lower primate make-up, and to what extent they would have to be changed to become man as we know him.

There is one aspect of this gene theory that we must just mention briefly. In order that the character which the gene stands for may be realised in development, interaction between the gene and the surrounding cell material must take place. This can be delayed, hurried, prolonged, and the like ; that is to say, differences between one form and another may be qualitative or quantitative. The former would need some modification of the gene itself, the latter needs but more or less of the same forces normally at work.

Primate Anatomy

We can now approach this aspect of our problem by considering the differences between the members of the anthropoid stem and ourselves. There are three striking series of facts to be mentioned at the outset. First, that Tarsius is probably an indication of the number of human-anthropoid characters that were already present as far back as the Eocene in the Primate stock. Secondly, that such fossil apes as have been discovered, and we refer especially to *Australopithecus africanus*, in so far as they differ from the existing anthropoids, lean more towards the human characters, and that when fossil men vary, especially, at any rate, those of Palestine, they too come to express modern human characters.

When we consider such a matter as the human infant proceeding on all fours, it goes upon the palms of the hands and indeed walks like a monkey and not like an anthropoid ape. Herein at once we are struck by the fact that the ape is doing something different, something more specialised. In fact, the muscles of the forearm have become shortened so that the ape must perforce march upon his knuckles. Man is nearer to the generalised primate stock, so that when he resumes a quadrupedal gait he does it in the most ancient way. In the absence of certain external sexual changes man is more like the gibbon and the orang, and in the ability to grow long hair he shares an accomplishment with the orang, although in most respects he is more like the chimpanzee and gorilla than the other anthropoids. Not only did man avoid the bodily changes of brachiation—a special arboreal mode of progression—but also in a great many other directions he refrained from pushing to the extreme certain trends. That man is fundamentally more closely related to the existing great apes than to any other living primate is common ground, but account must be taken of the very important anatomical observations made by Straus and also by Schultz,

working in the department of anatomy at Johns Hopkins. Straus has especially studied the pelvis, the musculature of the extremities including the hand and the foot, and the form and arrangement of the abdominal viscera. In all these he comes to the same conclusion—that man stands nearer to the primitive monkey stem than do the anthropoids. Straus' studies have led him to the conclusion that man must have emerged from the primate stock when its level of evolution had reached somewhere between that of an old-world monkey and a gibbon.

Schultz has especially devoted himself to giving a quantitative value to all those characters which man and the ape share. The skeleton, the proportions of the limbs, hands and feet, the duration of the growth period, times of closure of sutures and the like, lend themselves to quantitative investigation. Thus we can consider not only qualitative differences between man and the apes, but also quantitative differences. The results of these investigations have led Schultz to the general conclusion that the Asiatic and the African anthropoids are groups closely related amongst themselves, and more closely related to one another than they are to man. Thus it is established by these quantitative investigations that the divergence of man from anthropoid ape is greater in degree than the divergence amongst themselves. By a different method of investigation we thus come to the same conclusion as Straus, namely, that man has come from the primate stem earlier than the anthropoids. Le Gros Clark, who has especially considered the basal primate anatomy, comes to a similar conclusion. If this is so, the vast number of likenesses in man and the anthropoids are family resemblances starting from hereditary units derived from the common primate pool and developed along parallel lines.

If man has avoided many of the anthropoid specialisations, it is logical to expect that he might retain many of the early primitive primate characters which the great apes have modified or lost. These aspects Wood Jones has especially emphasised, and they have led him to conclude that man has emerged from the primate stock at a very early stage in its evolution. Man, for instance, has small canine teeth and a chin. In all apes the canines are great teeth, but the chin is absent. The bones of the side of the skull and front part of the floor of the skull preserve the primitive arrangement in man, while they are differently constituted in the anthropoids. The humerus of man shows very occasionally an entepicondylar foramen, a feature which occurs only in the very basal primate stock. These are characters tucked away in the recesses of the body, of no apparent survival value, and not exposed to the

rigours of natural selection. They live on because their genes have enjoyed unbroken transmission. Those who have these characters could not be derived from those who have lost them. A recrudescence of them by a subsequent mutation seems incredible.

Man, of course, has a great number of distinctive characters. The most striking and obviously the most important is the enormous development of the brain. The important observations on this matter have been gathered together by Sir Arthur Keith. Man's brain is more than treble the size of the largest existing anthropoid, though all primates have relatively large brains, and the anthropoids, and especially the fossil ape—*Australopithecus africanus*—exhibit a well-marked tendency to brain enlargement. In both man and the anthropoids the brain grows abundantly during the time of the milk dentition, but while the increase is 30 per cent. in the gorilla, it is 200 per cent. in man. The greater size of the brain in man is arrived at not so much by a longer time of growth, but by the greater velocity at which it proceeds.

This specialisation of the brain, which is one not of change in the organisation or differentiation, but of the rate of growth, has of necessity led to many changes in man's body. It allies man with the apes in so far as they share it ; it separates man from them by the far greater degree to which it has been carried in him. A common trend has been inherited, but man by his earlier emergence from the primate stock has avoided either the inhibitory, or acquired the augmentory factors which have led to the largest cerebral volume.

Conclusion

By two lines of evidence we have proceeded to justify the theme of this essay. By a brief review of the discoveries of fossil man we have been led to believe that man of a type indistinguishable from modern man lived in the Early Pleistocene, and that his contemporaries were men who shared more closely than he did many of the pithecoid trends, and some of them were much smaller brained than he was. By brief reference to the work of Le Gros Clark, Schultz, Straus, and others, we are on anatomical grounds led to believe that man emerged from the primate stock at the primitive monkey level before the emergence of the anthropoid apes. If this is so, it is conceivable that one type of man might have evolved without being encumbered by overhanging brows, and without a flat head, and have proceeded straight along an evolutionary line that ended in the production of the modern type. How direct and brief this course might have been we can only surmise. Palæontology suggests gradations, genetics makes it possible that it might have been

saltatory with few and large gradations. The finding of more fossils alone can decide this problem.

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JETS MUSICALLY INCLINED

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"We must look on all jets as musically inclined."

JOHN LECONTE, M.D., *Discoverer of Sensitive Flames.*

". . . answer to Hi! or any loud cry
Such as Fry me! or Fritter my wig!"

"*The Hunting of the Snark.*"

". . . organ pipes are of such delicate constitution that they take umbrage at a touch."

Opinion of Walter and Thomas Lewis, organ builders of Bristol ("*Modern Organ Building.*" London: William Reeves, 1923).

JETS of fluid have always attracted the interest of natural philosophers. They were the favourite study of Leonardo da Vinci, forming the subject of a great number of his drawings and notes, and although he was chiefly concerned with water jets, he also studied jets in air and described ways in which they might be made visible.[1] The illustration in Plate I, Fig. 4, is one of Leonardo's sketches of the vortices produced by a water jet flowing into water.

That great physicist, physician and Egyptologist, Dr. Thomas Young, also addressed himself to them; so, again, did John Tyndall, who produced many pretty experiments, demonstrations and dogmatic conclusions; and Lord Rayleigh, lecturing on Tyndall's scientific discoveries, confessed that "one that I have been especially interested in myself, is the subject of sensitive flames" [1]. Indeed, he had spent thirty-five years, off and on, in experiment and calculation, and had published a dozen communications varying from short notices of observations to long mathematical papers, of which even Lord Kelvin was constrained to remark that he found some of them "very difficult reading, in every page, and in some only difficult" [2].

In fact, despite more than a century of experiment and theory,

[1] I am indebted to Sir Kenneth Clark, Director of the National Gallery, for the authority for these statements.

the immense complexity of the behaviour of jets has defied all attempts at explanation. And yet, at first sight, nothing would seem much simpler than forcing a fluid to debouch from a hole into a space containing the same fluid at rest: complications due to surface tension and buoyancy which arise when a jet issues into a medium different from itself (such as water fountains in air) are absent. According to the kinetic theory, a gas consists of a large number of particles moving in all directions with very high velocities, and suffering a large number of collisions, and superimposed on these velocities is the velocity of the jet which, relatively speaking, is a mere drift. Nevertheless, the phenomena exhibited by jets of this sort present problems which are far beyond our present mathematical powers.

Thomas Young [3] commences his communication to the Royal Society on the subject of sound by remarking that "the further I have proceeded, the more widely the prospect of what lay before me has been extended," and concluded that his investigations would "occupy the leisure hours of some years, or perhaps of a life." The second of his experiments relates to his drawing reproduced in Fig. 3 (Plate I), which he describes as follows:

"2. Of the direction and velocity of a Stream of Air.

"One circumstance was observed in these experiments, which it is extremely difficult to explain, and which yet leads to very important consequences: it may be made distinctly perceptible to the eye, by forcing a current of smoke very gently through a fine tube. When the velocity is as small as possible, the stream proceeds for many inches without any observable dilation; it then immediately diverges at a considerable angle into a cone, Fig. 24; and, at the point of divergency, there is an audible and even visible vibration. . . . When the pressure is increased, the apex of the cone approaches nearer to the orifice of the tube, Figs. 25, 26; but no degree of pressure seems materially to alter its divergency. . . . At first sight the form of the current bears some resemblance to the vena contracta of a jet of water; but Venturi has observed, that in water an increase of pressure increases, instead of diminishing, the distance of the contracted section from the orifice. Is it not possible, that the faculty with which some spiders are said to project their fine threads to a great distance, may depend on the small degree of velocity with which they are thrown out, so that, like a minute current, meeting with little interruption from the neighbouring air, they easily continue their course for a considerable time?"

This curious feature of jets, the nearly constant angle of divergence whatever the velocity, has remained substantially unaccounted

for until the present day. It appears that this sudden spreading-out of the jet from a certain point was first shown in a drawing by a scientist. Artists, acute as their vision is in most respects, have failed to notice it. Botticelli's depiction of a jet in his "Birth of Venus" (reproduced in Fig. 2, Plate I) is, for instance, a quite impossible form.

Interest in the behaviour of jets was re-aroused when, as a consequence of the adoption of coal-gas for the illumination of houses, a most unexpected discovery was made in somewhat unusual circumstances. One John Leconte, M.D., wrote to Silliman's *American Journal* for January 1858 as follows [4]:

"A short time after reading Prof. John Tyndall's excellent article 'On the Sounds produced by the Combustion of Gases in Tubes,' I happened to be one of a party of eight persons assembled after tea for the purpose of enjoying a private musical entertainment. Three instruments were employed in the performance of several of the grand trios of Beethoven, namely, the piano, the violin, and the violoncello. Two '*fish-tail*' gas burners projected from the brick wall near the piano. Both of them burnt with remarkable steadiness, the windows being closed and the air of the room being very calm. Nevertheless it was evident that *one* of them was under a pressure nearly sufficient to make it *flare*.

"Soon after the music commenced, I observed that the flame of the last-mentioned burner exhibited pulsations in height which were exactly synchronous with the audible beats. The phænomenon was very striking to everyone in the room, and especially so when the strong notes of the violoncello came in. It was exceedingly interesting to observe how perfectly even the trills of this instrument were reflected on the sheet of flame. *A deaf man might have seen the harmony*. As the evening advanced, and the diminished consumption of gas in the city *increased the pressure*, the phænomenon became more conspicuous. The jumping of the flame gradually increased, became somewhat irregular, and finally it began to flare continuously, emitting the characteristic sound indicating the escape of a greater amount of gas than could be properly consumed. I then ascertained by experiment, that the phænomenon *did not* take place unless the discharge of gas was so regulated that the flame approximated to the condition of *flaring*. I likewise determined by experiment, that the effects *were not* produced by jarring or shaking the floor and walls of the room by means of repeated concussions. Hence it is obvious that the pulsations of the flame *were not* owing to *indirect* vibrations propagated through the medium of the walls of the room to the burning apparatus, but must have been

produced by the *direct* influence of aërial sonorous pulses on the burning jet."

Leconte went on to assert that it was evident (although it was not), that the phenomena he observed were perfectly analogous to those investigated by Count Schaffgotsch and Tyndall [5] with flames which could be made to sing in tubes, the only difference being that in his case there was no tube. He then quoted the explanation of singing flames given by Faraday, who considered that the pulsations producing the "singing" were due to successive explosions caused by periodic combination of the oxygen of the atmosphere with the issuing jet. Later, however, after reading Plateau's *Theory of the Modifications experienced by Jets of Liquid issuing from circular orifices when exposed to the influence of Vibratory Motions*, he concluded that this is only a particular case of the effects of sound on all kinds of fluid jets.

According to Plateau, all the phenomena he observed in his famous experiments on the disruption of liquid columns, were due to conflict between vibrations and "forces of figure." Leconte then tried to argue in favour of molecular cohesion in gases so as to produce "forces of figure." If this be admitted, then he maintained that gaseous jets are subject to the same laws as liquid jets and we must then "look on all jets as *musically inclined*."

He concluded by suggesting that the singing sound produced by free burning jets is also due to periodic explosions, but he forbore "enlarging on this very interesting subject, inasmuch as the accomplished physicist (John Tyndall) . . . has promised to examine it in future. In the hands of so sagacious a philosopher, we may anticipate a most searching investigation of the phænomena in all their relations."

The results of the "most searching investigation," anticipated by Leconte, were given by Tyndall in a lecture at the Royal Institution on "Sounding and Sensitive Flames" [5]. He commenced by remarking that the fish-tail flames in some of the metropolitan railway carriages would jump in synchronism with certain tremors of the train. He then showed that flames ordinarily insensitive, such as candle flames and the batswing flame, can be made to exhibit changes when a whistle is blown, provided that they are first disturbed by a jet of air from a blowpipe. But, curiously enough, Tyndall never seems to have suspected that the blowpipe jet was the sensitive agent and that the flames only acted as indicators of its disturbance.

A number of different flames were made to show their peculiar ways of turning from "apathy to sensitiveness"; some long flames

became short, forked, and brilliant, while some short flames became long and smoky, others roared, and his most sensitive jet was much affected on having a passage from Spencer read to it:

> " Her ivory forehead full of bounty brave,
> Like a broad table did itself dispread," etc.

It picked out certain sounds, noticing some " by the slightest nod, to others it bows more distinctly, and to some its obeisance is very profound, while to many sounds it turns an entirely deaf ear."

Later, by means of passing the gas over ammonia and then hydrochloric acid so as to produce a smoke of sal ammoniac, Tyndall showed that similar effects are obtained with unlit gases. He used coal-gas, hydrogen, carbon dioxide and air. In each case the pitch effective was found to be much lower. Some of Tyndall's drawings of his smoke jets are shown in Fig. 6, Plate I.

In concluding, Tyndall, after agreeing with Leconte that the flames must be on the point of flaring, said that they stood " on the brink of a precipice. The proper sound pushes it over. It shortens when the whistle sounds, exactly as it did when the pressure was in excess. The action reminds one of the story of the Swiss muleteers who are said to tie up their bells at certain places lest the tinkle should bring an avalanche down. The snow must be very delicately poised before this could occur. I believe it never did occur, but our flame illustrates the principle. We bring it to the verge of falling, and the sonorous pulses precipitate what was already imminent. This is the simple philosophy of all these sensitive flames."

The thoughts of the Switzerland that he loved so well thus engendered, seems to have prevented Tyndall from noticing that his explanation consisted in saying that a process whose origin he didn't explain at all (turbulence due to excess velocity) was imminent. This was hardly satisfying and even his assistant Barrett ventured to criticise it [6], without, however, getting very much further. Barrett thought that the main action of the sound was to throw the pipe leading the gas to the jet into vibration and so " the flow of gas is thereby driven from the sides and urged more towards the centre of the tube." This necessitated an increase in velocity and it was known that burning jets flared when the velocity was above a certain limit.

Lord Rayleigh started by examining the effect of obstructions in the tubing such as stopcocks and taps and agreed: " It may be that, as Prof. Barrett suggests, the mischief is due to the irregular

flow and consequent ricochetting of the current of gas from side to side of the pipe." Nevertheless, with pipes without obstructions and shielded from the sound vibrations, the jet is still sensitive; in fact, the sensitivity is increased. Rayleigh then put the flames in stationary waves between a source and a reflector of sound and observed that they were most disturbed at points where the *motion* of the air was greatest, and not at the points where the pressure change was greatest, and consequently were affected at places where the ear would not be and *vice versa*.

Later, Rayleigh examined jets of phosphorus-smoke by intermittent vision and noticed that sound-waves produced "a serpentine motion of the jet previous to rupture," but he had great difficulty in getting sufficient illumination for his stroboscopic experiments and he repeated them with jets of water coloured with potassium permanganate flowing into water. Even so, with the best arrangement at his disposal, he found the appearances difficult to interpret and reproduce in a drawing. However, Mrs. Sidgwick, who was working with him, was not deterred from producing the sketch shown in Fig. 8, Plate I. Rayleigh kept on returning to the subject and his collected papers are interspersed with notes of experiments and queries, these, no doubt, being included in order, as he said, "to mitigate the general severity."

In the "general severity" must certainly be included Lord Rayleigh's mathematical attempts to throw light on the phenomenon of sensitivity; in fact, as we have seen, such an accomplished mathematician as Lord Kelvin could only view some of them with dismay. Rayleigh, adopting a method originally due to Kelvin, examined what would happen if the boundary of the jet contained

PLATE I

FIG. 1.—Effect of a jet upon the surface of the sea (and Odysseus) as seen in the fifth century B.C.
[*Greek comic vase: from Gardner's "Greek vases in the Ashmolean Museum." Clarendon Press.*]

FIG. 2.—A jet as painted by Sandro Botticelli in 1485.
[*"Birth of Venus." Uffizi. Anderson Photo.*]

FIG. 3.—Smoke jets as they appeared to Dr. Thomas Young, F.R.S., in 1800.
[*Philosophical Transactions of the Royal Society*, **112**, 1800.]

FIG. 4.—Vortices in a jet of water drawn by Leonardo da Vinci.
[*Windsor Collection. Copyright of H.M. the King.*]

FIG. 5.—The ideal jet of the mathematicians.

FIG. 6.—Smoke jets as seen by John Tyndall, F.R.S.
[*By courtesy of the Editors of the "Philosophical Magazine."*]

FIG. 7.—A jet in the real world.
[*By courtesy of the Physical Society of London.*]

FIG. 8.—A water jet as observed by Mrs. Sidgwick (and Lord Rayleigh).
[*By courtesy of the Editors of the "Philosophical Magazine."*]

FIG. 1.

FIG. 2.

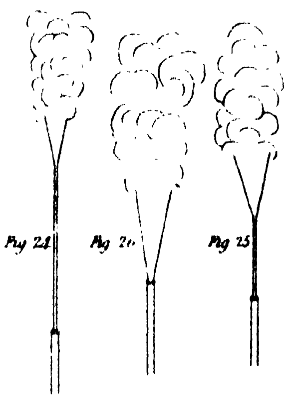


FIG. 3.

FIG. 4.

FIG. 5.

FIG. 6.

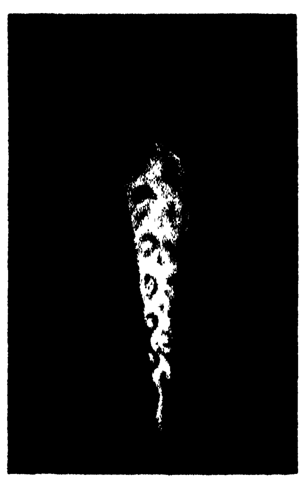


FIG. 7.

FIG. 8.

undulations (such as are shown in Fig. 13, Plate II). He was looking for a word to describe this case in which the sinuosities are symmetrical about the axis of the jet, when, "during a biological discussion I found that there is a recognised, if not very pleasant, word '*varicose*.'" In such a case of "varicosity" the formula he obtained indicated that the undulations should increase in size rapidly and disrupt the jet, and this would happen the more readily the higher the frequency of the sound. Unfortunately this prediction is not borne out in practice: jets vary in the frequency to which they are most sensitive and their response to different frequencies is very complex [7]. Rayleigh then attempted to allow for the viscosity of the fluid and produced several more long papers, but at the end of thirty-five years, he was no nearer explaining the "simple philosophy" of sensitive flames.

Many other investigators added to the accumulation of observations, some maintaining that the most sensitive jets were those which issued from a perfectly smooth circular metal orifice of perfect symmetry [8], while others recommended orifices of glass of irregular shape [9].

Let us cease now for a moment from considering the passive musical inclinations of jets which have caused such confusion and fruitless speculation, and study another and more active feature of their musical propensities. When suitably stimulated they will emit beautiful sequences of tones and these, in their turn, serve to provoke oscillation of the air in resonating cavities such as the flute and the organ pipe, so that a great volume and variation of sound is produced. It is in this way that the music of nearly all wind instruments originates.

If a jet of air is blown, say, from the mouth, against an obstacle such as a sharp edge, a note is produced, called an edge-tone, whose frequency rises with increased blowing pressure, and there may also be sudden jumps to notes of higher frequency. This was, no doubt, a prehistoric discovery, but the fact that if the edge blown against is the rim of a pipe, the pipe may be made to sound, is mythologically attributed to Pan. There seems to be some doubt as to whether the first pipes were reeds or hollow bird's bones, but the attribution of the discovery to a shepherd is certainly to be welcomed since it helps to explain what shepherds do to while away the long hours of their vigil. For it is curious that an occupation so conducive to contemplation should, in the whole history of the world, have produced so little. Perhaps the close association with sheep, creatures that have, at any rate in their maturity, long been symbolic

of a lack of originality and spirituality, may react unfavourably on their human accompanist. At any rate, it would be a happy thing if we could count the music of wind instruments as a gift of shepherds to civilisation, and suppose that it was one of them who first blew across the top of a broken reed and noticed that the note produced varied with the distance of the joint from the open end. Thus by cutting the reeds at different distances from the joint, or closed end, a series of pipes was obtained from which music could be called forth.

The pipes in the fourth century B.C. consisted of a set of ten with their open ends in a horizontal line so that the lips could easily be transferred from one to another, and their lower ends forming, in consequence, a series of steps. This was the syrinx or pan pipe, and a representation of Pan blowing such an instrument, taken from a Greek vase of the fourth century B.C., is shown in Fig. 9, Plate II.[1] The fact that increase of blowing pressure causes the edge tone to jump suddenly in frequency was made use of in order to produce the octaves. The syrinx enjoyed great popularity and was much used in the Middle Ages, but as early as the second century B.C. there evolved from it one of the most remarkable of all musical instruments—the organ. How this took place we shall probably never know, but there is little doubt that it also began, if not as a broken reed, certainly as a mutilated one.

Let us look at Fig. 10, Plate II : the reed which Pan actually used was the one into which the nymph Syrinx was transformed when fleeing from his embraces ; the one shown in Fig. 10 is, however, merely diagrammatical. The breath, indicated by dotted lines, is blown horizontally across the top and impinges on the sharp inner edge producing vibration of the air in the pipe in a manner which we shall presently be at pains to explain. The joint forms a natural solid bottom to the pipe. The vibration of the air in the pipe, in the simplest case, is a maximum near the lip and a minimum in the neighbourhood of the joint.

Fig. 11, Plate II, shows how the reed could be altered so as to be blown in a different and more reliable manner. A slit is cut through the joint and a hole is made in the side of the reed with a sharp inner edge, in such a way that, if air is blown in as shown, a jet is formed on passing through the slit, which impinges on the sharp edge as indicated by the dotted lines. This again sets the air in the upper part of the reed in vibration. Fig. 12, Plate II, shows how closely a modern organ pipe resembles the earliest reed.

[1] I am greatly indebted to Prof. Bernard Ashmole and Prof. Randolph Schwabe for help in choosing some of the illustrations.

Now with a jet in a fixed position with respect to the sharp edge and no longer dependent on adjustment of the human lips, it is possible to evoke the tones mechanically, and an account in Greek exists of an organ worked by water pressure compressing the air, called a *hydraulus*, which belonged to Hero of Alexandria. The use of some form of bellows for compressing the air supply came later and was known in Roman times, when the organ was associated with the theatre, gladiatorial combats, and pagan amusements generally. It was consequently under the ban of the Church, and it is a curious reflection to consider that an instrument formerly banned should have become the supreme medium for the musical expression of Gothic religious feeling. The organ did not enter churches until about the tenth century, and it had then reached a considerable degree of perfection, Venice being particularly famous for its builders. It became the fashion for emperors to give them away as presents: Charlemagne, for instance, had to find room for one which was presented to him by the Byzantine emperor in 812. The bellows were of hide and the pipes of bronze, and its tone was said to be as loud as thunder and as sweet as the lyre and psaltery.

In Fig. 15, Plate II, we see King David playing an early type of organ in which wooden slides act as valves to admit the air to the different pipes. The air pressure is maintained by an assistant using a pair of bellows. St. Cecilia, the patron saint of musicians, is often shown with an angel working the bellows for her. A more uniform and copious supply than is obtainable by either of these methods is now attained by the use of mechanical bellows.

The organ continued to evolve, a great deal of attention and skill being lavished on it, especially in England, until we have at last an instrument possessing "that mysterious quality of other-worldliness that brings all heaven before one's eyes."

That the sound is produced by the oscillation of the air in the pipe has long been recognised, and in the larger pipes of the organ this can be felt by the hand; but when we ask how this oscillation is first set up we touch on a problem, which, like that of the sensitivity of flames, has worried natural philosophers for over a century.

The first attempt to explain the initiation of tones in an organ pipe [1] was made by the great German physicist Wilhelm Weber and his brother Ernst [10]. They suggested that the first puff of air entering the pipe starts a pressure wave which sets the air in the pipe into oscillation and the resultant compression and rarefaction at the mouth cause alternate compression and expansion

[1] The fullest account of this subject available in a text-book is to be found in Prof. A. T. Jones's *Sound*. Chapman & Hall. London, 1937.

in the air jet itself, causing it to give a regular series of puffs which maintain the vibrations.

Then Sondhauss [11] in Germany noticed in 1854 that tones are evoked when a jet is blown against an edge even in the absence of any pipe. He likened the phenomenon to a toy called the "Waldteufel," which had a great sale in bazaars in Berlin and Breslau about Christmas-time. This consisted of a cardboard tube, one end being closed with a disc having a horse hair attached to it. A loop was made in the free end of the horse hair and a pencil or similar object was rotated in it, the hair being kept taut. This procedure jerked the disc into vibration, and resonance in the tube helped to produce a loud noise so that the toy was a favourite amongst children. Sondhauss proceeded to point out solemnly that if, instead of twisting the pencil, the fingers were wetted and stroked along the horse hair, a longitudinal vibration was set up in it causing a tone whose frequency depended on the distance between the disc and the fingers; and as the frequency of edge tones also depends on the distance between the orifice and the edge, he suggested that there might be some mechanical analogy. This, however, is somewhat obscure and cannot really account for the facts.

Von Helmholtz [12] who did so much to put sound and music on a scientific basis, thought at first, that the impact of the jet on the lip caused a noise from which the pipe selected its own frequency. Later he suggested that vortices were formed in the jet when it struck the edge; these mixed with the oscillating air in the pipe and, according to whether it was streaming in or out of the mouth, reinforced its inward or outward velocity, and thus sustained the oscillation. When the air in the pipe is oscillating it is clear that if its amplitude were sufficient it would blow the jet out of the tube when it was moving towards the mouth and suck it in when moving away from it, and this is the famous "air-reed" theory which maintains that the jet of air behaves like a solid reed (like those in the mouth-organ, for instance). Similar but slightly different views were held by Herman Smith in England [13], by Sonreck, an organ builder of Cologne [14], and by one Heinrich Schneebeli [15], and although it is to a large extent true in the case of an organ pipe in which the air is already oscillating vigorously, it does not explain how this vibration is ever initiated or how tones are produced without any pipe at all.

A number of other researches and theories on the action of the organ pipe, made by means of introducing smoke into the air stream, were published by Van Tricht [16], by Hensen [17], and by Friedrich [18], and so great was the confusion that the Bataafsch

Genootschap der Proefondervindelijke Wijsbegeerte (Netherlands Society of Experimental Philosophy) chose the initiation of organ tones as the subject for a prize essay in 1858, and this was won together with a gold medal by Van Schaik [19]. He put talcum powder in the wind chest and observed the motion of the jet stroboscopically (*i.e.* by looking through slots in a rotating disc): this showed that the air jet does oscillate from side to side, first going into the pipe and then outside. However, this threw no light on the *initiation* of the tone, and Weerth [20], and then Wachsmuth [21], in Rostock, thought it might be simpler to go back to Sondhauss' original experiments with a wedge alone without any pipe. They showed that in this case, if the distance of the wedge from the orifice is constant (as it is in an organ pipe), the tone rises in frequency with increased blowing pressure together with occasional sudden jumps in frequency, and again that a similar effect occurs if the pressure is kept constant and the distance of the wedge decreased. Wachsmuth managed to get a series of photographs of a jet, to which ether vapour had been added, illuminated instantaneously by electric sparks.

His photographs were shown at a meeting of German scientists held in Kassel in September 1903, and a long discussion followed. Heydweiller (Münster) thought that there must be some asymmetry somewhere, since if the stream were accurately bisected by the wedge there was no reason why it should ever start to go to one side rather than the other. Wachsmuth replied (quite correctly) that any disturbance or sound waves would upset the symmetry. Riecke (Göttingen) thought there must be an analogy with a garden hose shaken at one end, while Rubens (Charlottenburg) and Pringsheim (Berlin) thought that they would be able to explain everything if the series of photographs could be shown cinematographically. Wien (Würzburg) said the number of oscillations per second of the jet must be equal to the frequency of the tone, and Drude (Giessen) observed that there were two motions to distinguish, the oscillation of the air from side to side and the translatory movement of the whole.

The session ended inconclusively and nine years elapsed before the problem came up again at another meeting of German scientists at Münster in September 1912, when König read a paper [22]. He put forward a theory similar to that originally adumbrated by Lord Rayleigh, *viz.* that the sudden stoppage of the jet on first encountering the wedge would send a compression wave travelling outwards in every direction with the velocity of sound and that this would produce a disturbance at the orifice, which, travelling

upwards with the jet speed, would cause another stoppage. But this did not give the correct value for the frequency, and anyway, it did not account for the jumps in tone. In the discussion which followed Kaufmann (Bonn) pointed out that the vortices could occur in a jet without its encountering an obstacle, and that perhaps the action of the wedge was to throw them first right and then left, and so cause sound. König replied that that was essentially Wachsmuth's original theory but it would not make the note depend on the distance of the wedge. Then Heydweiller repeated Helmholtz's original theory that the air stream behaved simply like a stiff reed, in which case the frequency of the tones would depend on its length. This was too much for König, who replied testily, " If it's really as simple as all that, then I can't for the life of me see why so many people have broken their heads over it ! " Heydweiller hastened to add, " Yes, of course, it would be much nicer, and from the point of view of exposition much preferable, if your simple and beautiful explanation might prove correct." The lecturer being thus placated, the discussion continued, and although considerable illumination was thrown on the various mentalities of scientists, little, if any, fell on the problem of edge-tones.

We come next to Schmidtke [23] : he agreed that the frequency of the note was equal to the number of vortices passing a given point in a second, and then attempted to make use of a mathematical theorem by von Kármán, who had calculated that a double row of vortices ought to be unstable except in one configuration, one in which they occur alternately on either side of the row with a certain spacing. The temptation to introduce mathematical considerations is always a strong one since physical theories are always strengthened thereby, but this attempt seems to have been premature. In order to obtain his result von Kármán had to assume (1) his vortices to be equal, (2) the rows of indefinite extent, and that the fluid was (3) without viscosity and (4) incompressible. In the case of air jets the vortices are (1) growing and decaying, and (2) the rows are necessarily not indefinite in extent, and further, the air is both (3) viscous and (4) compressible. It is not surprising, therefore, that Schmidtke's theory does not fit the facts, and anyway, he avoided the main question, which is how the vortices arise.

Then in 1920 Krüger [24] put forward yet another theory : he suggested that the jet had a natural " wavelength," or distance between vortices on the same side, corresponding to the " jet tone "—the hissing noise produced by a jet which is not striking any obstacle. When the distance from the orifice to the edge is equal to this wavelength the vibration ought to be a maximum,

and as the distance increases the amplitude of the vibration should diminish since the edge-tone wavelength becomes more and more out of step with the jet-tone wavelength. But as the wedge distance approaches double this wavelength, there will be a sudden jump back again to the natural wavelength corresponding to an octave jump in tone. Krüger did not verify his hypothesis by resorting to experiment, and as a matter of fact, the exact opposite of what he predicted occurs. Further, he did not explain why a jet should have a natural wavelength.

Benton [25] and Richardson [26] also favoured the Kármán vortex-row theory. Richardson described an experiment with water jets which tends to show that the Rayleigh-König theory of a compression wave travelling backwards from the edge to the orifice, with the velocity of sound, cannot be correct. He placed a second jet, without a wedge, near the first one: this second jet should have been affected by the sound wave from the first, but no disturbance of any kind was observed.

From this brief account of the behaviour of jets it will be seen that the conflicting observations and great variety of theories make it a difficult subject in which to attempt to introduce any semblance of order. And, as regards the physical mechanism, the analogies that have been suggested range over an alarmingly wide field, and the would-be investigator has to bear in mind—

(1) The projecting of threads by spiders (Thomas Young).

(2) The "Waldteufel," a toy sold in bazaars in Berlin and Breslau about Christmas-time (Sondhauss).

(3) The loosening of avalanches by careless muleteers (John Tyndall).

(4) A garden hose shaken at one end (Riecke).

(5) A varicose disturbance (Lord Rayleigh).

Certainly the position with regard to jets seems to be even worse than that of aerial vibrations in general, which Lord Rayleigh summed up as having resulted in a "vast accumulation of isolated facts and measurements which lie as a sort of dead weight on the scientific stomach."

Let us see now if it is possible, by further observation and inference, to mitigate this untoward dyspepsia. Suppose we pass a stream of air through a burning cigarette on its way to an orifice consisting of a narrow slit, and we then illuminate the jet intermittently by passing the light from two arc-lamps through slots in a rapidly rotating disc, and produce at the same time a pure sound whose frequency is equal to the number of interruptions of the light in a second. Now, if we look at the jet end-on, that is to say, in the

direction of its greater thickness, and we cause the sound from the loudspeaker to pass through it in the direction of its lesser thickness, and we choose a fairly wide jet (say 3 mm.), we see the appearance shown in Fig. 13, Plate II. Further, if we run the stroboscopic disc a little slower so that the interruptions of the arc-light now have a frequency a little below that of the sound, we no longer get a stationary appearance but a slow-motion view of the periodic changes which occur in the jet with the frequency of the note used (in this case 126 per sec.), which are, of course, much too rapid for the unaided eye. This stroboscopic device is much used for obtaining a slow-motion view of machinery such as cotton-spinning machinery, and the behaviour of the valves in motor-car engines.

From the slow-motion view we can see that the effect of the air motion in the sound waves is to produce alternate swellings and contractions in the boundary of the jet as it issues from the orifice, and these varicosities (to use Lord Rayleigh's term) travel upwards with a speed which is some fraction of the mean velocity of the jet. Now, if the velocity of the jet is not above a certain critical value, these undulations die away and the boundary becomes straight once more, as it does in Fig. 13. But if the velocity exceeds this value we get the appearance shown in Fig. 14: the undulations increase in amplitude and then a vortex filament breaks away from the top of a crest and curls round. This feature of the motion is more clearly seen in thinner jets so let us turn to Fig. 7, Plate I, where the effect of the same sound on a 1-mm. jet whose velocity is a little above the critical is shown. In this case the vortices occur alternately and we can clearly see how, when the crests and troughs reach a certain amplitude, the friction between the crests and the nearly stationary air outside causes a filament to break away, in much the same way that spray is blown from the crests of waves and produces "white horses" on the sea, when the velocity of the wind relative to the waves is above a certain value. This filament curls round into the following trough, but it only makes about one and a half complete turns before it ceases to rotate and the external air is no longer entrained, the boundary becoming straight once more but inclined to its original direction, so that the jet, when seen with the naked eye, appears to rise to a certain height and then diverge in a wedge shape.

All the parts inside the jet in Fig. 7 which appear black are, of course, due to the external air which has been entrained (which contains no tobacco smoke), and now we can see why *flames* are such good indicators of the action of sound waves on jets. For this entrained air increases the combustion which otherwise could

only take place along the outer boundary of the undisturbed jet, and the gas, being burned more rapidly, does not appear to extend so far from the orifice, since, of course, it is only the *burning* gas which is visible. Consequently a burning jet will appear to shorten or "duck" when it is affected by sound, which is the observation first made by Dr. John Leconte at the American musical tea-party. In our considerations we have gained greatly by using an unignited jet issuing from a *slit*, but the same phenomena can be shown to occur in flames from circular orifices [7]. In circular-sectioned jets the axes round which the vortex filaments curl are not straight, so that we cannot look through the vortices and see the motion so clearly, and this probably accounts for the fact that Lord Rayleigh could make nothing of the appearance pictured by Mrs. Sidgwick (Fig. 8, Plate I).

Another feature of jets which seems to have escaped a century's close observation is that they do not spread from the edges of the orifice but overlap a little owing to diffusion, and it is just in this overlapping region that they are most sensitive to sound. The more diffusible the gas the greater the effect, and in the case of coal-gas it can readily be seen by looking at the jets in a geyser. The omission of this effect is one of the reasons why the ideal jet of the mathematicians shown in Fig. 5, Plate I, is not satisfactory, and its other defect is, of course, the assumption that turbulent mixing with the external air and wedge-shaped spreading commences at the orifice. This assumption cannot be correct unless the jet is already turbulent before issuing from the orifice, for, as we have seen, the undulations must travel a certain distance before friction between them and the surrounding air causes vortex formation and consequent mixing. Both effects, however, become negligibly small when very large jets are considered, such as those from aeroplane wind tunnels, and these are the only ones that have been used for comparison with experiment. It must be confessed that little help has come from the mathematicians, and this has chiefly been due to their tendency to adopt *ad hoc* assumptions rather than wait for further physical research. One is tempted to repeat Dr. Thomas Young's criticism of the celebrated Bernoulli. "M. Daniel Bernoulli has solved several difficult problems relating to the subject, yet some of his assumptions are not only gratuitous, but contrary to matter of fact."

Suppose, now, that we turn once more to Dr. Thomas Young's drawings of jets (Fig. 3, Plate I). Here there was no loudspeaker to produce disturbance and yet the jets diverge beyond a certain point into a cone. Does the jet itself produce sound? If we

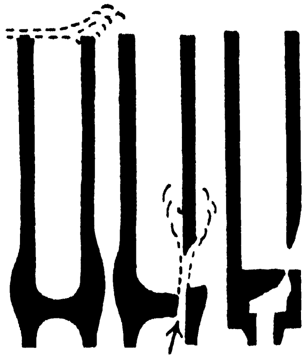
listen carefully through a piece of rubber tubing as the free end is moved about near the jet we find that at the point of divergence, where the vortex filament is spreading out, there is a slight hissing sound which may become easily audible if the velocity of the jet is increased; and with ignited coal-gas there is a loud roar. As the jet is sensitive to these tones it follows that it is self-disturbing, but the tones are not pure and the resulting vortex formation is very irregular and cannot be seen clearly. Nevertheless, this gives a hint towards the solution of our other problem, the production of edge-tones; for may we not suppose that the interposition of a wedge into a jet stabilises the vortex formation in some way, and so causes the emission of nearly pure sound which travels out and reacts on the sensitive part of the jet near the orifice, causing an orderly sequence of fresh vortices? This is a very plausible hypothesis and is essentially that suggested by Rayleigh, Schmidtke and Krüger, but Richardson's experiment with two jets seems to make it untenable.

Once again, then, we must resort to more experiment. Suppose, therefore, that we fix a brass wedge firmly in the path of a smoke-jet and allow the beams of two arc-lights to pass, one down either side of the wedge, and we alter the speed of the stroboscopic disc so that we get a stationary, or nearly stationary, picture. Then we see the following sequence [27]: first of all the jet is merely bisected by the wedge and half of it flows smoothly up one side of the wedge and half up the other, but when the velocity exceeds a certain value (lower than the value required for the development of sound-produced vortices and much lower than that necessary to produce the appearance drawn by Thomas Young) the whole jet appears to oscillate from side to side and presents the form shown in Fig. 16, Plate II. Let us call this Stage I. As the velocity is increased the oscillations become more vigorous and their frequency increases until, suddenly, there is change to the formation shown in Fig. 18 (Stage II) accompanied by a sudden rise in frequency (the wedge-distance in this and the succeeding illustrations was extended in order to make the vortex formation clearer). Augmenting the velocity causes further gradual increase in frequency and vortex growth, and then another jump takes place to Stage III, shown in Fig. 19. A similar sequence follows and another jump to Stage IV (Fig. 20). Beyond this the edge-tones degenerate into toneless hissing.

The changes and jumps in frequency as the velocity of the jet is increased are shown by the thin lines in Fig. 22. AB represents Stage I, and at B the frequency jumps up in the ratio 1 to 2·3; CD

PLATE II

Fig. 9.



Figs. 10, 11, 12.

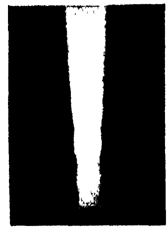


Fig. 13.

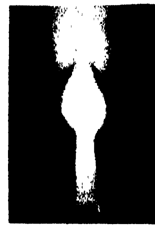


Fig. 14.

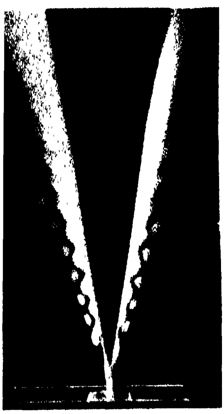


Fig. 16.

Fig. 15.

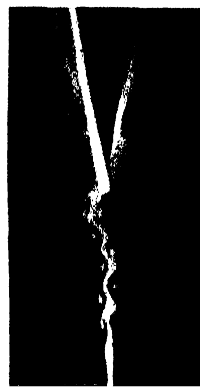


Fig. 17.

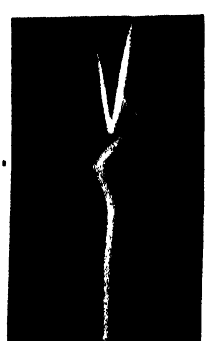


Fig. 18.

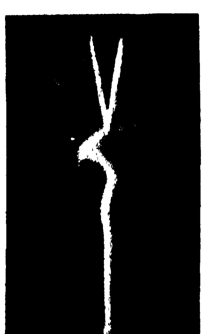


Fig. 19.

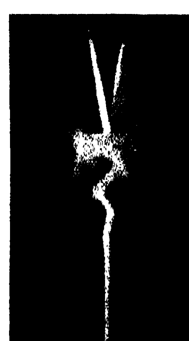


Fig. 20.

represents Stage II and another jump-up occurs at D (ratio 1 to 1·6); the jump from Stage III to Stage IV (ratio 1 to 1·4) is not shown. On decreasing the velocity the jumps down take place at different points E and C. A curious feature, when the mean velocity is above 500 cm. per sec., is that Stage I may occur simultaneously with one of the other stages, so that the jet may produce *two* nearly pure notes at once.

The frequency of the sound is related to the mean velocity of the jet and the height of the wedge by the following formula, which holds for a slit 1 mm. wide:

$$n = 0{\cdot}466\,j\,(\mathrm{U}-40)\,(1/h-0{\cdot}07)$$

where n = frequency in vibrations per second
U = mean velocity in cm. per second
h = distance of wedge from orifice in cm.
and j has the values 1, 2·3, 3·8, and 5·4 corresponding to the four stages.

Now there are two experiments which we can make, in addition to Richardson's, which throw doubt on the theory that sound travels back to the orifice and starts a fresh series of vortices. We can reproduce similar vortex development to that in Stages II, III, and IV, by the loudspeaker alone without any wedge. When this is done it is found that a loud sound is required, whereas the jet may emit no audible edge-tone with the wedge. Evidently, then, sound is not the only cause. Again, we can "grow" sound vortices on top of edge-tone vortices by using the loudspeaker when the wedge is in position as shown in Fig. 17, Plate II. (This is similar to Fig. 18, with sound-produced vortices superimposed.)

PLATE II

FIG. 9.—Pan playing the syrinx in the fourth century B.C.
[Copyright of the British Museum.]

FIG. 10.—Section of a reed showing the method of blowing by the mouth.

FIG. 11.—Section of a reed altered so as to be suitable for mechanical blowing.

FIG. 12.—Section of a modern flue organ pipe.

FIG. 13.—Undulations in the boundary of a smoke jet produced by sound.

FIG. 14.—Development of undulations into vortices with increase in jet velocity.

FIG. 15.—King David playing an organ as seen in 1241.
[Munich: Staatsbibliothek, from "A History of Music in Pictures." J. M. Dent & Sons, Ltd.]

FIG. 16.—Vortex formation produced by a wedge (Stage I).

FIG. 17.—Sound-produced vortices superimposed on edge-tone vortices.

FIG. 18.—Vortex formation produced by a wedge (Stage II).

FIG. 19.—Vortex formation produced by a wedge (Stage III).

FIG. 20.—Vortex formation produced by a wedge (Stage IV).

[Figs. 13, 14, 16, 17, 18, 19, 20, by courtesy of the Physical Society of London.]

It is difficult to see how the larger edge-tone vortices could remain unaffected by the development of the sound-produced vortices if they both originate at the orifice.

It appears, therefore, that some essential aspect of the phenomena must have been neglected. To see what this may be, consider the simplest case of all, that in which a jet impinges on a flat plate as shown in Fig. 21*a*. In this case, as in the case of a wedge, the jet, when its velocity is sufficient, starts to oscillate from side to side and the smoke escapes first on the right and then on the left. Suppose, now, that the jet moves over to the right, as shown in the diagram; this will involve displacement of the surrounding air, and if this is done quickly enough a local rise of pressure will take place at A, which, is dissipating itself, will react on the lower portion of the jet which is very easily deflected, and cause it to be displaced slightly to the left. The efflux then takes place towards the left, causing another local increase of pressure on that side, which drives the jet to the right, and so on. It is not difficult to see, therefore, how a vibration of the jet might be maintained by the reaction of the surrounding air without necessarily assuming that vortices must be formed at the orifice. Jets producing edge-tones are always sensitive to sound, so that we can explain the initial deflection by supposing that it is due to a vortex travelling up the jet caused by sound or other small disturbance.

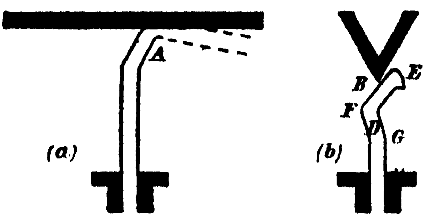


FIG. 21.

If we take the left half of the flat plate away, in Fig. 21*a*, we get the case of a jet blown against an edge, as in the original Pan-pipes, and a little consideration will show that here, again, oscillation can be maintained, even in the absence of any pipe.

To see what occurs when a wedge is used, we can imagine the plate in Fig. 21*a* to be hinged in the centre and that we slowly fold back the sides so that it becomes a more and more pointed wedge. The oscillations of the jet continue although the efflux occurs in a less confined space; this merely increases the frequency slightly.

But now another phenomenon occurs, connected with the sudden jumps in tone: to understand this let us turn to Fig. 21*b*. If the deflection of the portion FE of the jet is of sufficient amplitude and rapidity, it causes the deflection of the portion GF by the process that we have just envisaged: we may now suppose that a point

is reached, as the velocity of the jet increases, at which, from the exposed portion F, a vortex filament spreads out which develops as it travels upwards. When this vortex passes the edge it may cause a different disturbance in the surrounding air, resulting in the distance FE becoming shorter. Now this would correspond to a jump to Stage II and a sudden increase in frequency, since the shorter FE the shorter the interval between the arrival of successive vortices at the edge and so the greater is the frequency of the sudden local increases of pressure which occur.

Similarly the deflection of GF may become great enough to deflect the portion of the jet below it and then a vortex may commence at G. This, on travelling upwards, develops to a greater extent than the vortices formed at F and, in its turn, may cause a different disturbance at the edge, resulting in a smaller value for the distance FE, and so the frequency of the vortices arriving at the edge increases suddenly once more, giving what we have called Stage III. This process can be repeated by further increasing the velocity but after Stage IV no further jump takes place. That the distance FE does become suddenly smaller in each stage just after the jump can easily be observed, but what happens during the jump, takes place too rapidly for the eye to follow, even with the slow-motion device used.

This theory of the mechanism of edge-tone formation is able to account for vortex production which does not necessarily originate at the orifice, but, of course, when sound is produced there will be sound-produced vortices similar to those which we considered when discussing sensitive flames. This explains the ability of jets to give rise to two notes at once : the higher frequency tone (Stage II, III, or IV) is maintained by its sound waves producing fresh vortices at the orifice, while, at the same time, the whole jet, vortices and all, may oscillate from side to side as in Stage I, maintained by the process we have described.

We can now see why, on blowing harder on a musical instrument, we can evoke higher harmonies of the fundamental note of the pipe, and this, no doubt, was made use of by Pan when playing the syrinx, although he probably did not know that the edge-tone does not jump exactly an octave but in the ratio 1 to 2·3, the pipe forcing it to the octave. The greatest complexity in the initiation of its tones is shown by the flue organ pipe, and in view of the great controversy that has raged over it, it is of interest to see whether it can be explained by means of edge-tone theory.

If the air is admitted slowly into an organ pipe (it is then said to be "underblown") a very curious sequence of sounds ensues,

which is illustrated in Fig. 22, where the thick lines represent the audible pipe-tones, and the thin lines represent the edge-tones for the same jet and height of wedge but without any pipe. Lord Rayleigh [28] examined this phenomenon very carefully in a 2-foot open metal pipe and his description, for *decreasing* pressure, is given below together with letters referring to the corresponding features in Fig. 22 which was obtained from a 4½-foot open wooden diapason.

Rayleigh's Description.	Fig. 22.
" About this point the octave of the normal note is heard, after which the normal note itself disappears.	$i + j$ h
The normal note reappears, the octave continuing.	g
The octave goes	f
and then the normal note,	e
after which there is silence.	ed
Octave comes in again,	d
and then the normal note, at a pitch which falls from considerably above to a little below the natural pitch. At the lowest pressures the normal note is unaccompanied by the octave."	cb

In order to explain the mechanism of the production of this sequence, it is more convenient to consider what occurs with increasing velocity. In the pipe used, the construction of the block and lower lip was such that the jet of air is directed outwards and does not encounter the edge until its velocity is 200 cm./sec. Consequently Stage I is unstable and edge-tone oscillation commences at a (Stage II) : when this reaches a frequency of $n = 100$ c./sec., oscillation is set up in the pipe and the edge-tone changes suddenly to $n = 126$ c./sec. at b, and this frequency is feebly emitted by the pipe. The combination of the pressure changes in the pipe, and those in the wake of the vortices entering the pipe (which according to our theory are the cause of free edge-tone production) result in the frequency of pipe and edge-tone rising continually and slowly together to $n = 136$ c./sec. at c (U = 290 cm./sec.). The pressure changes in the pipe are not sufficient to affect the edge-tone any further and the sound ceases. But it happens that Stage III in the free edge-tones at U = 290 cm./sec. has a frequency very close to the octave, and this gives rise, for a short interval, to a very feeble octave note at d.

The construction of the block and lip now causes the jet to pass just *inside* the edge, and silence ensues, together with absence of edge-tone oscillation, until the velocity reaches 380 cm./sec. At this velocity a disturbance passing up the jet and striking the edge would produce a frequency of nearly double the fundamental, and

pressure variation of this frequency being reinforced by resonance, the note is maintained in spite of the fact that the jet does not strike the edge centrally. If the velocity is increased very slowly, however, the fundamental is elicited first at *e*. This presumably is due to the fact that the fundamental frequency, $n = 130$ c./sec., is *less* than half the octave, $n = 264$ c./sec., so that the edge-tone reaches double the fundamental of the pipe before reaching equality with the octave. When the octave sounds at *f*, the pipe takes

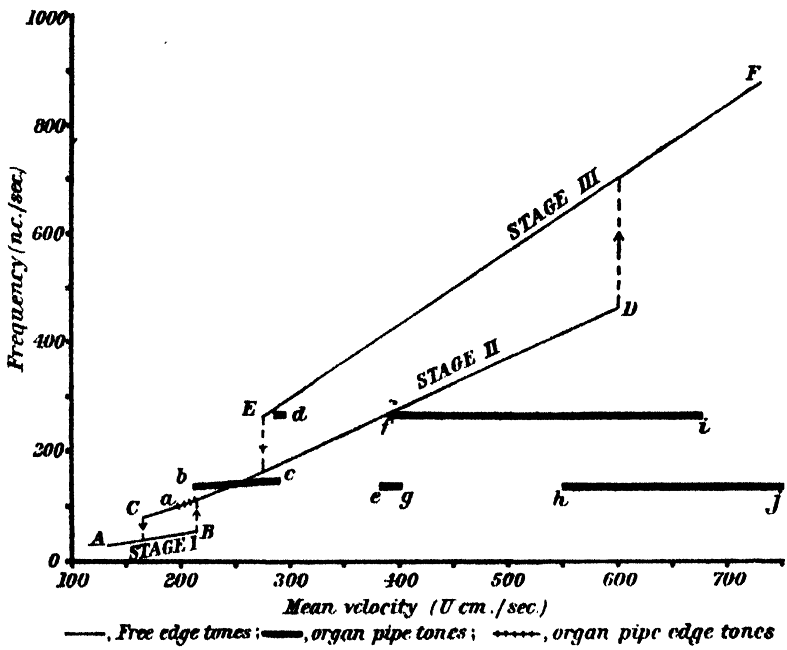


FIG. 22.—Open Wood Diapason. Fundamental, 130 cycles/sec.; Underblown octave, 264 cycles/sec.

complete control and the frequency remains constant (Stage II). When the velocity reaches 550 cm./sec. at *h*, the real fundamental of the pipe begins to sound and the jet oscillates as a whole from side to side (Stage I). The octave note can still be heard but disappears at *i*. The occurrence of Stage I at such a high velocity is quite impossible without a resonator.

It seems, therefore, that edge-tones are responsible for the initiation of the vibrations in musical wind instruments of the organ type, and that even the details of the complex initial phenomena can be accounted for adequately. Also it seems that both Helmholtz's theories are untenable, for the jet, on striking the

edge, does not produce a noise containing many frequencies from which the pipe selects its own frequency, neither does it necessarily start vibrating like a reed in the manner that we have called Stage I.

Such, then, are some of the intricate processes associated with the issuing of a gas through a hole, and it is little to be wondered at that the development of musical wind instruments has proceeded by the method of trial and error, and the knowledge handed down from father to son, and that organ pipes are said, by those who know them best, "to take umbrage at a touch."

Further interesting complications are introduced if the velocity of the jet is above the velocity of sound; or again, if the jet consists of a liquid issuing into a gas, such as a water fountain which breaks up into drops. The simpler case of gas flowing into gas, to which we have here restricted ourselves, has nevertheless stimulated the writing of over a hundred scientific papers. And although it is not yet possible to introduce mathematical rigour into the explanations given above, it is to be hoped that, at least, they merit something better than the dictum of Dr. Thomas Young (who, Lord Rayleigh said, "was hardly ever wrong") applied to a certain unfortunate Dr. Smith with almost Johnsonian devastation—

"Dr. Smith has written a large and obscure volume, which, for every purpose but for the use of an impracticable instrument, leaves the whole subject precisely where it found it."

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WHO DISCOVERED THE TRINIDAD ASPHALT LAKE?

BY PERCY E. SPIELMANN, PH.D., B.SC., F.I.C.

WHEN Sir James Barrie was asked what led him to write one of his literary productions, he answered, "I had a pain in my mind." In the same way I have had a pain in my mind ever since I realised that Sir Walter Ralegh's description of his acquaintance with Trinidad asphalt in no way corresponded with a description of a lake. It was to ease this "pain" that I have examined the matter closely.

It is commonly stated that Sir Walter Ralegh discovered the Trinidad asphalt lake. This immediately leads to the two questions: If he did, why is it not recorded in his writings? If he did not, who did?

The interest in the problem is manifold: it concerns the interpretation of the significance of his records and those of others; as well as a matter of priority, in a setting of Elizabethan vitality and glamour; and it deals with a natural product that was early recognised to be of technical value, which has later developed into being one of the most important materials for road and building construction.

The proper consideration of the subject requires an historical background, however slight, and the following points have been chosen as having the necessary significance.

Trinidad was discovered by Christopher Columbus, at noon on July 31, 1498; but little attention was paid to it by the Spaniards until the close of the sixteenth century. Then they proceeded to establish their domination early in the year 1588 by the almost complete destruction of the Indians. This led to the establishment of a permanent hatred of the Spaniards and an inclination towards friendliness to Sir Walter Ralegh, when he came. His first arrival took place on March 22, 1595, on his first journey in search of El Dorado, and his second in 1617; and he was followed by other Elizabethan and Stuart adventurers.

Repeated attempts were made to settle the British in Trinidad,

the first starting in 1628 with no immediate result, and the last being those of the Earl of Warwick in 1645 (n.s.). It is tempting to think of this Earl of Warwick as being the very Robert Dudley, son of Elizabeth's Leicester, who had actually visited Trinidad two months before Ralegh, but actually Dudley's claims to the earldom had been disallowed, and it had passed to the Rich family. This colonising earl is Robert, second (or twenty-third) Earl of Warwick (1587–1658). Colonel Scott was the chronicler of the undertaking, and he, writing after 1660 about events in the 1640's, refers to the Arawak Indians as being always friendly, as they never forgot the valuable military assistance given to them by the English against the Spaniards. "The commodities of the island, as reported by the English of that time, were rich copper and iron deposits, gums oils, balsams, pitch and sulphur, 'and other symptomes of rich mines' " [20]. There is no means of knowing whether the "pitch" was of lake or land origin: it certainly had no connection with coal tar.

It was not until 1783 that a planter named Saint Laurent realised the value of the island and its political importance, and he succeeded in arousing the interest of the Spanish Government. As a result, the island was thrown open to development and trading, and the population became numerous and mixed; and the island prospered greatly.

On February 16, 1797, a British squadron of four ships of the line arrived under the orders of Admiral Hervey, and landed General Sir Ralph Abercrombie with 4000 men, who easily captured the island from the Spaniards, permitting them to leave with military honours.

What happened to the Asphalt Lake during the "dark ages" of the Spanish occupation between the late sixteenth century and the late eighteenth? Why do we find no record of visits and exploration? Why is there no mention of it until so late a date as 1786?

One reason for this lack of information is that the Spanish records are not easy to obtain and consult. The most available source is Borde's account of the Spanish administration between the years 1498 and 1797 [10]. There is nothing about the lake in it, so that it is very probable that the Spaniards knew nothing about it. A reason for the lack of visitors may be that the island of Trinidad lies to the south of the trade route to the West Indies, so that it is not in the direct line of shipping; even Ralegh and his contemporaries only went there on their way to seek El Dorado up

the Orinoco River. And having got there, why should anyone seek asphalt in an inland forest when unlimited quantities were discoverable immediately on reaching the shore, especially in such a climate? In addition, we may have an interesting example of the unchangeability of human nature. As early as 1786, de Léry [19] wrote: "it is a pity that the lake is still only visited by the Caribs"; and as late as 1915, Pitt-Keithly [17] wrote: "Strange to relate, comparatively few of the people of Trinidad have ever seen the 'pitch lake,' or know more about it than do the residents of this country."

The Trinidad Asphalt Lake is situated near the village and Point of La Brea, about 1200 yards from the sea (in a direct line) and about 130 feet above its level. Let us see the impression made on observers, early in time as regards ourselves though late as compared with the dates of the early visits.

In January 1786, de Léry visited the lake and describes it [19] as follows: "The view is striking. There before one is a plain of pitch on a plateau at the extremity of the Pointe-à-brai, traversed by streams, more or less deep, running in all directions, consisting of excellent drinking water though tasting a little earthy. It is possible to walk upon the surface of the lake if the sun is not too hot; but at ten o'clock the pitch melts and forms a liquid surface which hardens again in the evening.

"The pitch of the lake is superior to that on the strip of land to eastwards ['Bande de l'Ouest']; it is used for the treatment of buildings without admixture of fatty substances, an advantage the others have not got. . . . This coast has only recently been inhabited and by concessionaires. The road from the Port of Spain is a mere track." de Léry also refers to the trade from the village of "brai des étangs," the exact meaning of which is not clear, though the land round about the lake is marshy and there is much "land asphalt" (as distinct from lake asphalt) in the close vicinity.

Three years later (1789) appeared another vivid account of the Lake, by Alexander Anderson [22], of which the following extracts have been chosen. Anderson visited the lake during the rainy season.

The lake "lies . . . where the mangrove swamps are interrupted by the sand-banks and hills . . ." and "is about fifty feet above the level of the sea." "From the sea it appears a mass of black vitrified rocks; but on close examination, it is found a composition of bituminous scoriae, vitrified sand, and earth, cemented together; in some parts beds of cinders only are found. In approaching this

Cape, there is a strong sulphureous smell, sometimes disagreeable. This smell is prevalent in many parts of the ground at a distance of eight or ten miles from it."

"This point of land . . . falls with a gentle declivity to it [the sea] . . . and only separated from the sea by a margin of woods which surrounds it, and prevents a distant prospect of it."

"It is of a circular form, and I suppose about three miles in circumference."

Another "early modern" description of it is that of Doctor Nicholas Nugent [6], in 1816: "We ascended the hill . . . to a plantation, then we procured a negro guide who accompanied us through a wood about three-quarters of a mile. We now perceived a strong sulphurous and pitchy smell, like that of burning coal, and soon after had a view of the lake, which at first sight appeared to be an expanse of still water frequently interrupted by clumps of dwarf trees or islets of rushes and shrubs; but on nearer approach we found it to be in reality an extensive plain of mineral pitch, with frequent crevices and chasms filled with water. . . . The lake . . . contains many islets covered with long grass and shrubs."

Coming a little later is the description by Lavaysse [14, p. 289 *et seq.*], which can be summarised in the following form. The most remarkable of these marshes is the asphaltum lake, which has no connection with the great lagoon as marked on some maps. It is about half a league in length and breadth (these extraordinary dimensions are discussed later). It is situated near the sea and is elevated 80 feet above its level. (This is another queer measurement, as the height to-day is over 130 feet.)

There is no phenomenon which offers more variety and mobility than the surface of the asphaltum lake. Here are seen groups of shrubs; there tufts of wild pine-apples and aloes. Among those shrubs and flowers, swarms of magnificent butterflies and brilliant humming birds seek their food, enlivening the scene which, if it were deprived of animals and vegetables, would present an exact image of Tartarus.

By mixing this asphaltum (blown up by the waves on to the shore) in the proper proportion with tallow and linseed oil, a kind of tar is made which is fit for caulking ships, and which has the inestimable property of preserving them from the corrosion of the sea-worm. Since 1805, the English have employed it very successfully for that purpose. The island produces sufficient to caulk thousands of ships every year.

These are clear and vivid descriptions by intelligent observers of an unusual and (to them) unique sight. They are of value, not

only for themselves, but also to serve as a standard for judging other accounts, which, human nature being of no great variation, might be expected to be of the same general nature.

Now let us have Sir Walter Ralegh's account of his approach to the island on his first visit in 1595:

"On Thursday the 6 Februarie in the year 1595, we departed *England* . . . and directed our course for *Trinidado* . . . wee arriued at *Trinidado* the 22 of March, casting ancour at Point Curiapan which the Spanyards call Punto de Gallo [now called Hicacos or Icacos], which is situate in 8 degrees or there abouts: we abode there 4 or 5 daies, and in all that time we came not to the speach of anie Indian or Spaniard; on the coast we saw a fire, as we sailed from the point *Carao* [now Negra Point] towards Curiapan, but for feare of the Spaniards, none durst come to speak with vs. I my selfe coasted it in my barge close abord the shore and landed in euery Coue, the better to know the Iland, while the ships kept the chanell. From *Curiapan* after a fewe daies we turned vp Northeast to recouer that place which the Spaniardes call *pourto de los Hispanioles* [Port of Spain], and the inhabitants *Conquerabia*, and as before (reuictualing my borge) I left the shippes and kept by the shore, the better to come to speach with some of the inhabitantes, and also to vnderstand the riuers, watring places and portes of the Iland which (as it is rudely done) my purpose is to send your Lordship after a few daies. From *Curiapan* I came to a port & seat of Indians called *Parico*, where we found a fresh-water riuer [now Punta del Cedro, or Cedar Point], but sawe no people. From there I rowed to another porte, called by the naturals *Piche* and by the Spaniardes *Tierra de Brea*: In the way betweene both there were diuers little brookes of fresh water & one salt riuer that had store of oisters vpon the branches of the trees [this is true, as the branches are submerged at high tide; Lavaysse [14, p. 287] also records this, and that the trees are mangrove trees], and were very salt and wel tasted. . . .

"At this point called *Tierra de Brea* or *Piche* there is that abundance of stone pitch, that all the ships of the world may be therewith loden from thence, and wee made triall of it in trimming our ships to be most excellent good, and melteth not with the sunne as the pitch of Norway, and therefore for ships trading the south partes very profitable. . . ."

This is a very different matter. There is here clearly no trace of evidence of his having seen the lake, but plenty that he was acquainted with the shore deposits at La Brea; he is describing a coastal tour of investigation in search of fresh water and of inform-

ation from the natives. Nowhere is there any indication of an inland exploration (and the lake is not visible from the shore), only one expedition being made for punitive purposes from the Port of Spain to San Joseph, a distance of 7–8 miles. The local Indians knew of it, as is shown by their legend of its formation. That nothing was said to Ralegh is understandable : on the one hand the natives were superstitious about it, and on the other the Spaniards were only superficially friendly and were not "giving anything away" even if they knew of it, which is unlikely.

In the few lines of description of the Island by Francis Sparrey [7], who was left there by Ralegh in 1595, there is no mention at all of asphalt.

It might be expected that some other Elizabethan adventurer (more accurately to be described as legalised pirate) had found the lake, but when records are examined there is no reference to it. Robert Dudley, cousin of Ralegh [2], reached the island on February 1, 1595, and left on March 1 (or 12), and mentions nothing about the lake [9]. Abraham Kendall, Master, whose Report is the third of the three describing Dudley's voyage [4], writes : "On 30th January 1595 we saw the Island of Trinidad. . . . In the said bay or harbour is a small stream of good water and easy to get. There is found a certain black bitumen like mineral pitch, good to patch vessels, near to Cape Curipan." The interest of this record is not only that he, too, makes no mention of the lake, but also that he anticipates Ralegh by two months in the discovery of the shore asphalt and its usefulness for ships.

Harcourt made a voyage in 1608, of which he published a description in 1663 [3]. He records : ". . . The twenty five [of September, 1608] we weighed againe, and plied along the shoare towards *Cape Brea*, about three leagues. This *Cape* is so called of the *Pitch* which is there gotten in the earth, whereof there is such abondance, that all places on this side of the world may be stored therewith.

"It is a most excellent pitch for trimming of shippes that passe into these regions and hot Countries, for it melteth not in the Sunne, as other Pitch doth." Another experience similar to Dudley's and Ralegh's, and again no mention of the lake.

Turning to records of foreign travel, there is the account of a Dutch voyage in 1597 [8] containing a general description of the island and of barter with the Spaniards, but asphalt is not mentioned. Apart from this there is nothing more than passing references to the "Pik meer." The aggressive incursions of the Dutch were a constant source of trouble with the Spaniards, and in a description

of a Relief Expedition (1638–39) under Don Diego Lopez, the following is given by Wise [16]: "The Governor himself with Captain Don Diego Ruiz went down the river to Cedros Bay and then on to Punta de Brea where pitch was cut with hatchets and crowbars to serve for patching the pearl fishing canoes at Margarita." Again there is evidence that the shore asphalt was well known and available, whilst that of the lake is not mentioned.

There is one record that gives some indication of the actual nature of the asphalt that Ralegh saw. This is in his account of his second voyage to Trinidad in 1617 [15]. He writes: "The last of December we wayed ancor and turned up north est towards Conquerabo, otherwise called the port of Spayne being new yeers eve, and we came to Ancor at Terra de Bri, short of the Spanish port some 10 leagues. This Terra de Bri is a peece of land of some 2 leagues longe and a league brode, all of ston pitch or bitumen which riseth out of the ground in little springs or fountaynes and so running a little way, it hardneth in the aire, and covereth all the playne, ther are also many springs of water and in and among them fresh water fishe. Here rode at ancor . . ."

The last few words again show that the material was a shore deposit. The area of it shows that it was a "playne" and not a lake, and its nature and mode of formation is similar to the descriptions of Lavaysse and of Bosworth to be given shortly.

There are two further descriptions [12] of the Trinidad asphalt, which make quite clear the nature of the known asphalt being not of lake origin: ". . . from some of the Mountaynes issue out great quantities of Tarre . . . and the very cliffs towards the sea are stored w^{t} a sort of Munjack, not much unlike pitch, to the eye, w^{ch} serves very well for triming ships . . ." Later: "Sir Walter Raleigh . . . came to the point wth in y^{e} grand Bay, called Punta de Brea or point Pitch . . . where the whole clifts are that kinde of Pitch, called in y^{e} West Indies Munjack; Sir Walter Rawleigh at his Extraordinary hazzard and paines surveyed the Island." Here if anywhere we might have expected a reference to the Lake, but there is none; and the statement that Ralegh "surveyed the Island" is inaccurate, unless it refers only to a coastal survey.

Turning to more modern authorities, we find observations and facts that give reasonable support to Ralegh's 1617 account.

Lavaysse [18] describes the coast as being a mixture of argillaceous limestone impregnated with asphalt, with excellent running water in the folds, some 6 feet deep, with plenty of small fish.

Bosworth [13] records: "Along the anticlines, which generally are sharp and very steep, petroleum has accumulated where the

conditions are favourable, and its presence is manifested by escaping gas, seepages of oil and by extensive surface spreads of pitch."

Taking these two together, and remembering that there is a considerable rainfall in Trinidad and that the small fish have a considerable power of migration [11], a fairly close confirmation of Ralegh's description is obtained. It must be remembered that the shore deposits were probably much more impressive in Ralegh's time (Prof. V. C. Illing), more perhaps than the lake itself (if Ralegh had seen it).

There remains to be considered the remarkable discrepancies in the figures given for the dimensions of the Asphalt Lake. Ralegh (assuming for the moment he was referring to the lake) gives 2 leagues by 1 league. In his day, the league had the same length as it has to-day—3 geographical or 3·456 statute miles, so the asphalt he found was about 7 by 3½ miles in area. Comparing this with others, we find :—

Ralegh [15] .	7 miles by 3½ miles
Anderson [22] .	about 1 mile diameter
Lavaysse [14] .	,, 1½ miles ,,
Schomburgk [1]	,, ,, ,, ,,
Owen Rutter [11] . . .	area about 114 acres
Modern map (Cadastral, F.10, N.E.	about 800 to 900 yards diameter, just over 148 acres

Apart from Ralegh's figure, the differences are considerable as between modern and careful observers ; and a possible clue to the reason for this is given in Mr. Owen Rutter's book [11]. He writes (p. 61) : "The asphalt is always moving, though the movement is imperceptible. Even the banks of the lake are affected by these mysterious movements ; from time to time the buildings on them take on a disconcerting list and on my visit part of the factory had recently been renewed for this cause."

This suggests that the asphalt extends for a considerable distance under what appears to be land, and at no great depth below the surface. Wording this in the opposite way, earth and vegetation has apparently spread over the original area of the asphalt. We know that clumps of vegetation grow on the asphalt to-day, so here is a basis for the question : Could the lake have been 1½ miles in diameter in 1845 (Schomburgk) and about 900 yards to-day ? Another possible explanation of the discrepancy is a confusion between the area of the asphalt lake, and the area of its containing basin.

An examination of 11 maps ranging in date from 1797 to 1928 shows that this is not the case, but the results are yet illuminating

and explanatory. The representation of the lake is divided sharply into two types, those made before 1838 (and including one map of no date but giving internal evidence of being published between 1840 and 1861), and those of 1895 and onwards.

Of the earlier type there are six, all showing the lake as an elongated area pointing roughly N.N.E., and drawn as an oval, usually with a heavily indented outline and a long central thin core. It is probably to this that Lavaysse referred (see earlier) as to the inaccurate connection that is shown of the lake with "the great lagoon." Of these, three of the maps were provided with scales and three without. Those with scales showed the central core of the area to be about ¾ to 1½ miles long.

The five maps produced between 1895 and 1928 show the lake as being approximately round, and ranging from about 850 to 1100 yards in diameter (excluding one Road Map in which the position of the lake is evidently diagrammatic). Assuming the lake to be round, which it is not, these diameters could correspond to an acreage of about 126 to 197 : the latest authoritative figure is just over 148 acres. The range of these figures is wide, partly because the mapping and measurement of the diameter of a small object is not very accurate, and partly because the lake is far from being exactly circular. But they do show very clearly that the change in the mapped shape is more likely to be due to the correction of faults of mapping than to the encroachment of soil over the asphalt. This is the more likely as the change took place subsequent to 1860, when the first official Report on the Geology of Trinidad was published [5]. Here the lake is shown as being approximately circular in shape, but being given the very low area of 99 acres.

It is not proposed to go into the other and minor mystery, the difference in statement of the height of the lake above sea level. The earlier writers give it as being about 50 to 80 feet, whilst later and more accurate figures give 138 feet. It is of no interest in our present problem.

In the development of the discussion of this problem, the strict historical sequence of events has been lost sight of, so that a summary of the more important dates will be useful.

Pre-Columbus. The Indians knew of the lake.

1498. Discovery of Trinidad by Christopher Columbus. No mention of asphalt.

1595. February 1 to March 1 (or 12). Visit of Robert Dudley. Abraham Kendall, Master, described Dudley's visit. Describes shore asphalt.

1595. March 22. Arrival of Sir Walter Ralegh. Describes shore asphalt.

1597. Cabilian (Dutch) describes the island. No mention of asphalt.

1608. Arrival of Robert Harcourt. Refers to shore asphalt.

1617. Ralegh's second visit. Refers to shore asphalt.

1638. Don Diego Lopez leads relief expedition. Refers to shore asphalt.
1640's. Colonel Scott describes Earl of Warwick's settlement. Mentions trading in

1783. Spain's interest aroused in the island, which is thrown open to commerce.
1786. Asphalt lake described by de Léry.
1789. Asphalt lake described by Anderson.
1797. Trinidad taken by the British.
1813. Asphalt lake described by Lavaysse.
1816. Asphalt lake described by Doctor Nugent.

From this the following conclusions can be drawn:

(1) Sir Walter Ralegh did not see the Trinidad Asphalt Lake, but described and used shore asphalt.

(2) He was not the first to use it, being anticipated by two months by his cousin, Robert Dudley.

(3) The first description of the lake was by de Léry, in 1786.

The only doubtful point that remains concerns the remarkably late date—1786—of the first published description of the lake. Reasons for this delay have been advanced. It now remains for some chance discovery in a volume of travels or in legal or biographical records to supply an earlier date. As many volumes have been consulted for the purpose of this discussion, the chance exists though it is not great.

NOTES

1. Sir Walter Ralegh wrote of La Brea being "called by the naturals *Piche*." This name is not derived directly from the English "pitch," as the Indians used it before the advent of the English. It has been pointed out [21] that, after Columbus and before Ralegh, trading with the Spaniards and Portuguese must have taken place; and the "naturals" adopted the Portuguese "pixe" rather than the Spanish "pez." Perhaps this preference may be another symptom of the natives' hatred for the Spaniards.

The other Spanish word, which Ralegh and the later map-makers used, is "Brea," meaning "pitch, tar, maltha," and is closely allied to the French "brai."

2. "Pitch" nowadays refers to the product of distillation of the tar that is usually produced by the distillation of coal or wood. The roughly equivalent petroleum product is known as "asphaltic bitumen," and when this is mixed with any mineral matter, such as sand, clay, stone, and the like, the resulting mixture is "asphalt." The Trinidad mixture was known to the early visitors as "pitch" because of the superficial resemblance; to-day the material is known as "Trinidad asphalt," and the term "pitch lake" gives way to "asphalt lake."

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PROGRESS IN FISHERIES RESEARCH

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ANYONE desirous of obtaining a working idea of the progress being made in fisheries research would do well to consult the series of reports issued by the International Council for the Exploration of the Sea, in connection with the Annual Reunion at Copenhagen last July. Much of the information given is, of course, mainly of interest to scientific specialists, but there is plenty to attract and hold the attention of the layman. For example, it may not be generally realised that the transfer of living sea fish from one area of the sea to another has become an established and highly successful method of improving the yield of commercial fisheries. In Denmark, where for many years there has been a yearly transplanting of young plaice from the nursery grounds of the North Sea to the inner Danish waters, about 1,150,000 were transferred in 1936 from the Horns Reef Area to the Belt Sea, the southern Kattegat, the Sound and the western Baltic. Another 1,650,000 were transplanted from the waters off Thyboren to the inner broads of the Limfjord. Smaller quantities of undersized plaice were sent from Denmark to the Swedish Government to be released in the eastern Kattegat, and to Norway for transplanting in the Oslo Fjord. A short report by the Swedish authorities on the results obtained draws attention to one of the disappointments which at present seems to be unavoidable in transplantation schemes, namely, that newly transplanted fish are in danger of being fished out too soon after they are liberated in their new home. Obviously it is a waste of time and money to "bed-out" young fish if they are going to be "plucked" the next day. They must be left to profit by accelerated growth in their more favourable habitat.

There have been improvements in the technique of plaice transplantation, and experiments in transferring fish of larger sizes than hitherto. Thus, in Germany, where plaice have been transferred from the North Sea to the Baltic, the fish are now packed in kelp in boxes and kept moist with running sea-water instead of melting

ice during the five-hour journey to the new grounds. This is done by means of tins with fine holes drilled in the bottom, through which sea water runs continuously on to the boxes over which the tins are mounted. Plaice as large as 35 cm. in length have been successfully moved in this manner, with the object of building up a stock of plaice in the Baltic which would spawn in the first year after transplantation.

The Germans have also experimented with turbot, but find that the fish used for transplantation were too old for the purpose. The results showed, however, that the fish adapted themselves fairly well to the new environment, although they did not grow any better than on their old grounds. It is suggested that the experiment should be repeated with younger fish.

Another piece of very interesting experimental work is that of Norwegian workers in artificially propagating hybrids between the plaice and the flounder. In 1935, at the Biological Station in Trondheim, about two million of these hybrids were liberated in the almost land-locked branch of the Trondheim fjord known as the Borgen fjord. In the same autumn, fry-fishing in this fjord yielded about 2 per cent. of these hybrids. In 1936 another 13 million hybrids were released and the subsequent autumn fry-fishing then showed the much larger percentage of 30 per cent. of hybrids present. The normal occurrence of natural hybrids is very feeble in this locality, being about 1 in 5000 flatfish present.

Scottish experimental work on the lobster has yielded valuable information on this important crustacean. For a number of years, lobsters kept in the building at the Bay of Nigg, under as nearly as natural conditions as possible, have been under continuous observation. The knowledge thus acquired indicates that lobsters after the adult characters and the bottom habit have been assumed, may be divided into five groups or growth periods :

(1) First bottom stage to a size of about 3 inches, during which moulting occurs three to five times, but usually three.

(2) Lobsters measuring 3 inches to about 6 inches. These seem to moult twice per annum.

(3) Lobsters from about 6 inches to between 9 and 10 inches, in which moulting is an annual event.

(4) Lobsters from 9–10 inches to about 13 inches, in which moulting is not more frequent than once in two years, or even longer. The attainment of this stage is probably coincident with first maturity.

(5) Lobsters above 13 inches.

The data on growth rate indicate that the female grows at a

faster rate than the male during the earlier years. Under laboratory conditions, while the male took nine years to reach a size of 9 inches, the female reached the same size in seven years.

Now although activities and experiments of the kind mentioned above are highly instructive and useful, one should not lose sight of the all-important work of actual "stock-taking" at sea on grounds worked by commercial fishermen, and populated by stocks of fish subject to natural fluctuations. Now, as ever, the yield of the fisheries rises and falls from year to year in consequence of unequal brood production under natural conditions. A rich harvest ensues when a rich brood comes to maturity; a bad one when a poor brood has to be fished. A rational fishery will seek to adjust fishing so that the rich brood shall be protected from avoidable waste. Some very illuminating data are given by Dr. Strodtmann regarding the plaice population of an area east of Sassnitz in the coastal waters off Pomerania during the years 1934 to 1936. He shows that a 30-minutes' haul of a shrimp net in 1934 yielded 242 baby plaice, but only 20 in 1935 and 4 in 1936. This was due to the very unequal broods of the three years. In the area between Sassnitz and Kolberg the 1934 brood was extraordinary dense; so much so that fishermen's catches consisted in the main of these young fish, with the result that over 90 per cent. had to be thrown overboard and wasted because of their small size. It is gratifying to learn, therefore, that the minimum size limit has been raised in order to protect the stock from such regrettable waste.

Finally, an equally interesting set of data illustrating the unequal production of broods from year to year is that relating to mackerel off the Atlantic coast of America in New England. Dr. Sette gives comparative figures for the fourteen years 1921 to 1934, inclusive, of the catch of mackerel per unit of catching power, the variation from year to year being a measure of the variation in brood strength. Taking the value for 1923 as 100, the others vary from as high as 150 in 1933 to as low as 0·1 in 1926.

SOME ASPECTS OF TIN RESEARCH

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The wide range of research work on matters in which metallic tin is the most important factor is shown up by the recent publications of the International Tin Research and Development Council, of which Series A, Nos. 46–72, are at present under review. These publications have, for the most part, been previously presented to various societies and constitute convenient reprints which may be obtained free of charge from the Council.

Tinplate is of special importance and No. 66 describes a magnetic and an electromagnetic method for measuring the thickness of tin coatings on steel. The force required to pull a magnet away from a tinplate sheet at a given point depends on the thickness of the tin coating at that point. A magnetic instrument has been constructed for measuring this force and thus indirectly the thickness of the tin at a point; it provides a rapid means of mapping the contours of the whole surface of a tinplate to an accuracy of a 1/100,000th inch.

Matters connected with the porosity of the tin coating have been investigated, and No. 59 reports on the variation in thickness of the tin coatings of tinplate and shows a definite relationship between porosity and thickness of coating. This is of importance in connection with accurate specification for various purposes and enables the value of any process improvements to be accurately assessed. The explanation of the processes of pore formation is dealt with in No. 64, which considers surface tension and viscosity phenomena in tinplate manufacture. During the manufacture, there is a short period, when molten tin and grease films are draining off the sheet, during which there are local effects responsible for the formation of normal and potential pores. When, for any reason, there is a small non-reactive spot on the steel sheet the layer of molten tin tends to bridge it, but when the spot is over a certain size the film of tin breaks and forms a pore. Potential pores are formed when the drops of grease collecting from grease lines

during the drawing of the sheet form cups in the tin coating sufficiently deep to expose the tin-iron compound beneath. No. 52 deals with the electro-deposition and polishing of thin coatings of tin on steel and also on the effect of deformation on the protective value of hot-dipped and electro-deposited tin coatings on steel. In the first part, the Council's patented process of electro-depositing tin on tinplate is investigated, and it is found that the composite tin coating has only one-tenth to one-twentieth the number of pores present in the hot-dipped coating of equal thickness. Tinplate also offers economic advantages over steel sheet as a base for nickel or nickel-chromium plating. In the second part of this report it is pointed out that the fabrication of tinplate involves operations in which the steel base is deformed beyond its elastic limit and the tin coating is generally broken. The average increase in number of discontinuities as a result of deformation has been found for various grades, and this increase is much less in the composite coatings obtained by electro-deposition.

Work has been continued on the fundamental properties of tin and tin-base alloys and the influence of the orientation of two crystals on the mechanical effect of their boundary is reported in No. 68. It is concluded that the boundary itself has no inherent strength and that the evidence obtained is against the existence of an " amorphous layer " or " intercrystalline cement " at the boundary.

In the investigation of tin and its alloys, the microscopical examination of sections is much used, and although the technique of preparing hard metals for this purpose is well established, difficulties arise when it is applied to soft metals such as tin. Improved methods of mounting and special refinements in polishing and etching have been devised, and are reported in No. 47. The systematic study of the effects of alloying on the properties of tin has been accompanied by a corresponding investigation of the constitution of some of the alloys thus produced. In the ternary cadmium-antimony-tin alloys the correlation between structure and properties has been particularly complete and is reported in Nos. 61 and 62; this work makes it possible to select alloys amenable to heat-treatment and to determine the best conditions under which heat treatment can be applied. While this branch of the work has been concerned mainly with the addition of common metals to tin, exploratory work has been carried out on the effects of adding small amounts of rarer metals to tin. No. 50 deals with the alloys of tin with germanium and beryllium respectively, and the results obtained indicate that these alloys do not possess any properties that cannot be attained with equal readiness by using less-expensive elements.

Tin-base bearing metals have received considerable attention and No. 57 deals with the mechanical properties of some white bearing metals and other tin-base alloys at various temperatures. This paper gives data about tensile and Brinell tests on various bearing metals and on alloys made by adding cadmium in varying proportions. Up to 3 per cent. addition of cadmium causes an improvement in strength and hardness but above this amount these advantages are offset by loss of ductility. The extra strength due to the cadmium addition is retained by the alloys when heated, although these alloys, like nearly all the others examined, lost a definite proportion of their tensile strength when raised to a certain temperature. Alloys of tin, copper and cadmium without antimony do not appear to have any particularly useful properties which cannot be obtained more easily in other alloys. Owing to the fact that the temperature of bearings may be much above that of the air when working, it was decided to examine the tensile behaviour of typical tin-base bearing metals at temperatures up to 175° C. and to ascertain the effects of additions of lead and cadmium. No. 58 reports that as the temperature rises the maximum stress and yield point fall fairly uniformly, while the values for elongation and reduction of area increase. Cadmium markedly improves the tensile strength of the alloys when cold, but is of little benefit when they are hot, and the effect of 4 per cent. of lead, although slightly beneficial when cold, is reversed at the higher temperature.

The design and construction of a special interferometer apparatus for the examination of the elastic behaviour of soft metals, such as tin and its alloys, under the necessarily small stresses which can be applied to them, is described in No. 60.

Research work on pewter has been conducted along two main lines—the study of the alloys used at present and the development of new alloys. The effects of cold work and annealing on the hardness of some tin-antimony, tin-antimony-copper and tin-antimony-silver alloys are given in No. 53. Three alloys of tin with antimony in the proportions of 3, 5, and 7 per cent. were investigated without and with the addition of copper or silver in substitution of part of the tin. All the alloys were hardened by moderate cold rolling down to 40–50 per cent. of original thickness, but further rolling softened them, the softening becoming more pronounced with the higher proportions of copper and silver. Annealing caused further softening in most cases, but some of the copper and silver alloys which had been severely worked improved slightly in hardness. Various quenching and ageing treatments were tried but were without permanent effect.

The corrosion of tin in aqueous solutions and in oils has been further studied. It is known that tin is attacked fairly readily in strong acid or alkaline solutions, particularly in the presence of atmospheric oxygen. The greatest interest lies in the behaviour of tin in nearly neutral solutions, which are typical of the conditions in many kinds of canned foods, dairy products, tap water and other substances with which tin usually comes in contact. This matter has been studied with special reference to the behaviour of the oxide film and the influence of different anions and cations. The results are given in No. 63. In this, the attack of solutions of the alkali metal salts of many anions and of the chlorides of a few cations has been studied electrochemically. Repair of the pre-immersion film takes place first owing to anodic oxide formation within its pores, but when the anodic metal at the base of the pores becomes sufficiently polarised, due to deficiency of hydoxyl ion, a sufficient proportion of soluble stannous ion may be formed there to give undermining and breakdown of the film. The consequent increased anodic oxide formation then produces a black spot at the point of breakdown. Black spots are formed on tin by salt solutions which give no precipitate with stannous ions but not by solutions giving stable precipitates since with these undermining cannot occur ; of the anions studied, chlorides give the most attack. Concentrated chloride solutions give more rapid breakdown than dilute. Indirect evidence has been obtained that the rate of oxidation of freshly abraded tin is very rapid for the first few minutes, but becomes relatively very slow after about six hours.

With regard to oils, it is known that bearing metals of different kinds are corroded to a greater or less extent by lubricating oils. To obtain exact information on this subject, the interaction between tin and oils has been studied and compared with that of copper, lead, antimony and other metals which are of interest in connection with the lubrication of bearings. The results are given in No. 51 and were obtained by an optical method based on the increase of light transmission of thin metal films caused by the decrease of thickness due to corrosion. The experiments demonstrated the superior corrosion resistance of tin, which was shown to be due to the formation of a protective film at its surface. Simultaneously, the influence of the different metals on the deterioration of the oils has been studied. It is shown that oxidation of the oil and the formation of sludge are stimulated by copper, but that tin, and to a less extent lead, act to the reverse direction. If the oil is in contact with copper and tin together, the acceleration of oxidation due to copper is compensated by the retarding effect of tin. It appears

that the viscosity, surface tension and acidity of the oil are not influenced by the metal present. Oils stored in open cans for some months before the test were decidedly more corrosive than oil fresh from a sealed can.

The possible methods for protecting or improving the corrosion resistance of tin have received attention and it is known that the protective oxide film may be strengthened by anodic treatment, by methods similar to those employed in the "anodising" of aluminium. Recent results are given in No. 48, which show that a black film may be produced on tin, pewter, tinplate and other tinned ware by anodic treatment in certain aqueous solutions. By stopping-off parts of the tin surface, decorative effects can be obtained. The black films have a pleasing appearance and protect the underlying metal from atmospheric tarnishing; their protective action against chemical reagents has not yet been fully assessed, but is probably not great.

In connection with the uses of compounds of tin, one of the oxides (stannic oxide) has long been used in vitreous enamels to produce a dense, white, porcelain appearance. In recent years there has been a tendency to replace stannic oxide by a number of substitutes that produce a similar effect. This has shown the necessity for examining scientifically the claims of stannic oxide, and No. 65 reports the results of such examination. Experiments on thirty kinds of enamels of different composition show the all-round superiority of stannic oxide. This oxide is outstanding, not only in its opacifying power, but also in its influence on other properties of the enamel, such as bending strength, resistance to thermal shock, resistance to attack by acids, behaviour on overfiring and coating power. Although some of the substitutes are satisfactory in some of these respects, none of them can compare with stannic oxide in all respects. Another important result of this research is to show that the effect of stannic oxide can be enhanced by suitable choice of composition of the enamel.

The analysis of tin and tin alloys has received a certain amount of attention and No. 46 deals with the development of spectrographic methods for determining small quantities of aluminium, cadmium and zinc over the range 0·001 to 1·0 per cent. in tin. Spark spectra are recommended for routine determinations of these impurities.

A rapid method for determining the amount of cadmium in tin is described in No. 55. This method is based on the fact that when tin or tin-rich alloys are heated to 700–730° C. in a vacuum, any cadmium present volatilises, leaving the tin, and the cadmium content is determined by direct loss of weight.

RECENT ADVANCES IN SCIENCE

MATHEMATICS. By J. H. C. Whitehead, M.A., Balliol College, Oxford.

The Theory of Groups : II.—Starting with the definitions and general theorems described in the first article, the theory of groups falls under two main headings. One is the " pure " or " abstract " theory, which is only concerned with the algebraic structure of a group. In other words it is the theory of the multiplication table. The second is the theory of continuous groups which, in addition to the multiplication table, have some geometric structure. For example, a rotation about a fixed point in a plane may be represented by a point on the circle $|z| = 1$ $(z = x + iy)$, and the resultant of two rotations z_1 and z_2 as the product z_1z_2. Thus the group of rotations in a plane has the intrinsic geometric structure of the circle $|z| = 1$.

A pure group, which is sometimes called an abstract or a discrete group, may be defined by a system of " generators " between which there are certain " relations." Any product of the generators determines an element of the group, with the understanding that different products may represent the same element. I will describe some of the theorems and difficulties in the theory of discrete groups, defined in terms of generators and relations, starting with one of the most primitive branches of mathematics, namely, the theory of free groups.

We start, not with abstract elements, but with a set of symbols a_1, a_1^{-1}, a_2, a_2^{-1}, . . ., called *generators*, and finite products of generators, called *words*. It must be understood that the term " word " has its ordinary meaning relative to the " alphabet " consisting of the generators. Thus a_1 and $a_1a_1a_1^{-1}$ are distinct words. However, we include among our words the " empty word," having no letters, which we write as 1. If W_1 and W_2 are any words, the word W_1W_2 may be thought of as the product of W_1 and W_2 and $W_1W_2 = W_1$ or W_2 if $W_2 = 1$ or $W_1 = 1$. Let $W_1 = AB$, where A and B are any words, and let $W_2 = Aa_i^{\varepsilon}a_i^{-\varepsilon}B$, where $\varepsilon = \pm 1$ and $a_i^{+1} = a_i$. The transformation $W_1 \rightarrow W_2$, likewise $W_2 \rightarrow W_1$, is called an elementary transformation, and two

words W_1 and W_n are said to be equivalent if they are related by a chain of words $W_1, W_2, \ldots, W_n$, such that W_i^{+1} is obtained from W_i by an elementary transformation. The relation of equivalence is obviously symmetric and transitive, and it follows that the equivalence classes are mutually exclusive, two words belonging to the same class if, and only if, they are equivalent. If U is any word we may, for the time being, denote the class containing U by [U]. If $U_1 \rightarrow U_2$ is an elementary transformation of a word U_1 it is obvious that $U_1V \rightarrow U_2V$ is an elementary transformation of U_1V, where V is any word. Using the symbol of congruence to denote equivalence between words, it follows inductively that $U^*V \equiv UV$ if $U^* \equiv U$. Similarly $U^*V^* \equiv U^*V$ if $V^* \equiv V$, whence $U^*V^* \equiv UV$ if $U^* \equiv U$ and $V^* \equiv V$.

We are now in a position to define the group. As elements of the group we take these equivalence classes. We have seen that the class [UV] is a single-valued junction of [U] and [V], and we take [UV] to be the product of [U] and [V]. The multiplication so defined satisfies the associative law. For if the word ABC is a representative both of the element [AB][C] and of the element [A][BC], the element [1] clearly satisfies the requisite conditions for the unit element and we take $[a_i^{\epsilon_i} a_j^{\epsilon_j} \ldots a_k^{\epsilon_k}]^{-1}$ $(\epsilon_i, \epsilon_j, \ldots, \epsilon_k = \pm 1)$ to be $[a_k^{-\epsilon_k} \ldots a_j^{-\epsilon_j} a_i^{-\epsilon_i}]$. Then the conditions for a group, given in the first article, are all satisfied and the group so defined is called the *free group, freely generated by the given set of generators.* Except when it is necessary to emphasise the distinction between words and classes of words we may omit the square brackets and refer to the element W when, strictly speaking, we mean the element of which the word W is a representative.

In an account of free groups one would naturally place first the theorem that there is one, and only one, reduced word in any equivalence class, a reduced word being one which contains no consecutive pair of generators of the form $a_i^{\epsilon} a_i^{-\epsilon}$. This theorem provides a criterion for deciding whether or no two words represent the same element. For one has only to reduce each word by cancelling consecutive generators $a_i^{\epsilon} a_i^{-\epsilon}$, and the two words represent the same element if, and only if, the reduced words are identical.

Another theorem is that any sub-group of a free group is a free group (*i.e.* is freely generated by a suitable sub-set of its elements). This was first proved by a fairly heavy algebraic argument. However, a very simple topological proof has since been discovered, depending on the following facts. First, any free group can be represented geometrically as the fundamental group (see an earlier article, Topology I) of a linear graph (1-dimensional complex);

secondly, the fundamental group of any (connected) linear graph is a free group; thirdly, any sub-group of the fundamental group of a complex K, is the fundamental group of a " covering complex " of K, meaning a complex $\bar{K}$ which can be mapped on K by a continuous transformation which is $(1-1)$ in some neighbourhood of every point. The theorem now follows at once from the fact that if K is a linear graph so is $\bar{K}$.

Another class of theorems have to do with the automorphisms of a free group. If in every word we replace the letter a_i by $a_i a_j^{\epsilon}$ and a_i^{-1} by $a_j^{-\epsilon} a_i^{-1}$, for fixed values of i, j and ϵ $(j \neq i)$, it can easily be verified that the resulting transformation of words determines an automorphism of the group. Similarly an automorphism is defined by substituting $(a_j^{\epsilon} a_i)^{\pm 1}$ for $a_i^{\pm 1}$, and also by substituting a_i^{-1} for a_i and a_i for a_i^{-1}, for some fixed i. It has been proved that any automorphism of the group is the resultant of a sequence of such " elementary " automorphisms. By a somewhat complicated geometrical argument a criterion has been established for deciding whether or no there is an automorphism which transforms one of two given sets of elements into the other.

A discrete group in general is defined in much the same way as a free group. We start with a system of generators a_1, a_1^{-1}, a_2, a_2^{-1}, . . ., and relations

$$R_1 = 1, \ R_2 = 1, \ . \ . \ .$$

The elements of the group and the multiplication is defined as before, except that a transformation of the form $UV \rightarrow UR_i^{\epsilon}V$, or its inverse, is now included among the elementary transformations. The group so defined is the factor-group F/F_0, where F is the free group, freely generated by a_1, a_1^{-1}, . . . and F_0 is the smallest invariant sub-group of F which contains all the elements R_1, R_2, ...

Starting from this definition one is immediately confronted with a series of extremely difficult, and hitherto unsolved, problems. For example, one of the first questions which arise is, " How can I decide whether or no two given words represent the same element of the group ? " This is the so-called " word-problem " and, except in certain special cases, the answer is unknown. Again, one cannot tell, in general, whether or no two given groups, even if defined by finite systems of generators and relations, are isomorphic. Indeed, one cannot even tell whether or no a given group contains elements other than the unit element. One seemingly small, but nevertheless important, advance has been made in case there is but a single relation, other than the " trivial relations " $a_1 a_1^{-1} = 1$, $a_2 a_2^{-1} = 1$, ... This is the so-called " Freiheitsatz," which states that, if

the relation is $R = 1$, and R, when expressed as a reduced word, contains the generator a_i or its inverse, then the sub-group generated by the remaining generators is a free group. In other words, no relation between the remaining generators is a consequence of $R = 1$. By a somewhat similar type of argument to the proof of this theorem one can solve the word problem for such groups.

The geometrical structure of a continuous group, G, can be described in various ways, of which one of the simplest is to require that G is a metric space in the topological sense. This means that a real number $\delta(x, y) = \delta(y, x)$, called the distance between x and y, is associated with every pair of elements (x, y), and the function $\delta(x, y)$ satisfies the conditions

(1) $\delta(x, x) = 0$, $\delta(x, y) > 0$ if $x \neq y$,

(2) $\delta(x, y) + \delta(y, z) \geq \delta(x, z)$.

Continuity can be defined in terms of this distance as in the theory of a single real variable. In particular, a function $y = f(x_1, \ldots, x_n)$, where $x_1, \ldots, x_n$ and y are elements in G, is continuous if $\delta(y, y') < \varepsilon$, for a given positive ε, provided $\delta(x_i, x'_i) < \rho$, for some positive ρ, where $y' = f(x'_1, \ldots x'_n)$. With this understood we require the product xy, of two elements in G, to be a continuous function of x and y.

Many theorems concerning continuous groups have to do with the fact that the group G can be represented in two ways as a group of transformations of G into itself. For if a is a fixed, and x a variable element in the group, the equation $y = ax$ determines a transformation T_a^+ of G into itself. Clearly T_a^+ has an inverse, namely, $T_{a^{-1}}^+$. The resultant of T_a^+ followed by T_b^+ is the transformation $z = by = bax$ and is therefore T_{ba}^+. Thus $T_b^+ T_a^+ = T_{ba}^+$, where we write $T_b^+ T_a^+$ for the resultant of T_a^+ followed by T_b^+. Therefore these transformations form a group. Moreover, $T_a^+ \neq T_b^+$, unless $a = b$. For $a = b$ if $ax = bx$. Therefore the correspondence $a \rightarrow T_a^+$ is an isomorphism of the group G on the group of transformations T_a^+. A similar argument shows that the transformations of the form $y = xa$, which we denote by T_a^-, form a group, and that $T_a^- T_b^- = T_{ab}^-$, where we now write $T_a^- T_b^-$ for the resultant of T_a^- followed by T_b^-. Each of these groups is simply transitive, meaning that there is a unique transformation T_a^+ and a unique T_a^-, given by $a = y_0 x_0^{-1}$ and $a = x_0^{-1} y_0$ respectively, which transform a given element x_0 into a given y_0. We call T_a^+ and T_a^- $(+)$ – and $(\pm)$ – *translations*.

With these two groups are associated two kinds of "parallelism." We define a "vector" as an ordered pair of elements

(x_0, x_1), which are analogous to the initial and final points of a vector in Euclidean space (not to the two components of a vector in a plane). Two vectors (x_0, x_1) and (y_0, y_1) are said to be $(+)$ — parallel if they are equivalent under the group of $(+)$ — translations. That is to say, they are equivalent if the $(+)$ — translation $x_0 \rightarrow y_0$ also carries x_1 into y_1; *i.e.* if $y_1 = y_0 x_0^{-1} x_1$; *i.e.* if $y_0^{-1} y_1 = x_0^{-1} x_1$. This condition is satisfied if $x_1 = x_0 a$ and $y_1 = y_0 a$, where a is any element in G, for we then have $x_0^{-1} x_1 = y_0^{-1} y_1 = a$. Thus an alternative construction for $(+)$ — parallelism is to apply T_a^- to every element. Then the vectors (x, xa), for a fixed a and variable x, are all $(+)$ — parallel. For the definition and similar discussion of $(-)$ — parallelism we have only to interchange $+$ and $-$, with the consequent modifications (*e.g.* $y_1 y_0^{-1} = x_1 x_0^{-1}$ as the condition for $(-)$ — parallelism) in what we have just written.

If the group G is commutative (*i.e.* if $xy = yx$ for every x and y) the $(+)$ and the $(-)$—translations and parallelisms coincide, as when G is the group of translations in Euclidean space. The first example to be discussed, in which the group is non-commutative, was the "Clifford parallelism" in (real) elliptic 3-space, which is the space of the group of rotations in Euclidean 3-space. Let us describe the two systems of generators of the absolute (a fixed quadric with a real equation but no real points) as the $(\pm)$ — systems. The generators occur in complex conjugate pairs, since the equation of the absolute is real, and conjugate generators do not intersect, since the absolute has no real points, and so belong to the same system. Therefore there is a unique (real) line through any (real) point P, which meets each of two given conjugate generators in the $(\pm)$—system. Keeping the generators fixed and varying P we have a congruence of lines which we describe as $(\pm)$—parallel to each other. Moreover, any real line meets the absolute in two complex conjugate points and the generators in either system through these points are conjugate. Therefore there are two lines through any point which are respectively $(+)$ and $(-)$—parallel to the given line. The $(\pm)$ — translations are the real projective transformations which leave the absolute and each $(\pm)$—generator on the absolute fixed. Clearly any line is carried by a $(\pm)$ — translation into a $(+)$—parallel line. Moreover, any $(+)$—translation leaves two $(-)$—generators unaltered and, since the translation is a real transformation, these generators are conjugate. Therefore each one of a certain family of $(-)$—parallel lines are unaltered by any $(+)$—translation.

A systematic account of discrete groups is to be found in a book by K. Reidemeister (*Einführung in die kombinatorische Topologie,*

Brunswick, 1932). The theorem that any sub-group of a free group is a free group was first proved by O. Schreier (*Abhand. aus dem Math. Sem., Hamburg*, V, 1927). Various proofs of this theorem, including the one I have outlined, are to be found in Reidemeister's book. The theorem that any automorphism of a free group is generated by elementary automorphisms is due to J. Nielsen (*Math. Annalen*, XCI, 1924), and the test for equivalence of sets of elements under the group of automorphisms was given in a paper of mine (*Annals of Math.*, **37**, 1936). The " Freiheitsatz " and the solution of the word-problem for groups with a single relation are due to W. Magnus (*Journal für die r.u.a. Math.*, **163**, 1930, and *Math. Annalen*, CVI, 1932). The abstract theory of continuous groups, in which a continuous group is taken to be a topological space with a continuous multiplication table, was initiated by O. Schreier (*Hamb. Abh.*, **4**, 1926). The $(\pm)-$ parallelisms were first studied in the space of a lie group, meaning that the geometrical structure of the group is that of an analytic manifold and the product xy is an analytic function of x and y. The group G then has an infinitesimal geometry and the parallelisms were first studied by the methods of tensor analysis. However, it is worth noticing that the definitions and elementary theorems do not depend on these additional assumptions. Indeed, they may be considered as belonging to the algebraic part of the theory, since they do not even depend on the continuity of G, only on the multiplication table. The parallelisms in elliptic space were discovered by W. K. Clifford (*Collected Papers*, Paper XX). There is a good account of them in F. Klein's collected works (Vol. I, Paper XXI).

ASTRONOMY. By A. Hunter, Ph.D., F.R.A.S., Royal Observatory, Greenwich.

The Classification of Stellar Spectra.—The spectra of the stars were first observed more than a century ago by Fraunhofer, whose name is still associated with the characteristic absorption lines in the solar spectrum. Enough data had accumulated by the middle of the nineteenth century for the early and very rough classification of stars by their colours to be replaced by a more precise division into four " types " of spectra. Nobody who has ever invited two people to describe the colour of the same star can doubt that this was a major step forward in the problem of classification. Over 99 per cent. of all known stellar spectra consist of absorption lines or bands crossing a background of continuous emission. Present-day theory attributes the continuum to an incandescent photosphere and the absorption to the atoms and molecules existing in the

relatively cool outer atmosphere. The earlier classification, by colour, dealt in essence with the photosphere, since colour is merely a matter of the relative intensity of the continuum at different wave-lengths. Secchi's classification into four types, however, is based entirely on the absorption lines and bands, and therefore refers to the stellar atmosphere.

Since his time, the examination of thousands of spectrograms, mainly at Harvard, has shown that any division of stellar spectra into watertight compartments is unreal: the great majority can be arranged in the well-known sequence O,B,A,F,G,K,M,N, the number of divisions in the sequence being a matter purely of convenience, and not of physical significance. This Harvard classification, embodied in the latest form of the *Henry Draper Catalogue* (*Harvard Annals*, **91** to **99**, 1918–24), is still in general use. It assigns to any given spectrum its place in the sequence according to the prominence or otherwise of certain selected lines or bands. This work has gone a long way towards removing the distinction noted above between the two kinds of classification, *viz.* by the continuous spectrum and by the absorption lines. Broadly speaking, a similar sequence could be arranged if a colour basis of classification is used. Obviously, though, the recognition of absorption lines is a far easier process than the measurement of continuum intensities at different wave-lengths. This latter does, in fact, involve an experimental technique which is prohibitively laborious for mere classification purposes.

It would be difficult to over-estimate the importance of the Draper classification to astrophysical research. It is a tribute to the prescience of Pickering, Miss Cannon, and the other workers at Harvard that the system has survived unchanged in principle since the end of the last century. Modifications have taken the form mainly of refining the sequence by what is in effect a system of decimal subdivision of the main types. The International Solar Union in 1913 adopted the Harvard system exclusively, and its successor, the International Astronomical Union, in 1922 could suggest only secondary additions, the chief of which recognises the distinction between giant and dwarf members respectively of a single type by prefixing the letters "*g*" or "*d*." Since then other minor peculiarities have been internationally recognised.

Let us turn now to the physical implications of the classification. The fact that a linear sequence suffices for the great majority of stellar spectra suggests that the most important differences between the spectra can be traced to the variation of a single physical parameter. There is no doubt that this is the temperature of the stellar

atmosphere. The other major differences, those between "*g*" and "*d*" spectra, are due to the differences of absolute magnitude, *i.e.* of surface gravity. Minor differences such as are represented by the symbols "*k*" for interstellar lines, "*n*" and "*s*" for nebulous and sharp lines respectively, relate to characteristics which are not particularly relevant to the study of stellar constitution. Broadly, then, the Harvard system represents stellar spectra as controlled by two main physical characteristics, *viz.* temperature and surface gravity.

Until quite recently the adequacy of the Draper Catalogue had not been called into question. The development of large telescopes of great light-gathering power, however, has given impetus to the more detailed study of stellar spectra. High-dispersion spectrograms of the brighter stars are now being subjected to an intensive scrutiny which is bringing to light joints in the armour of the Harvard system. Attempts have been made to refine still further the decimal subdivision of the classes, in some cases even hundredths of a spectral class being distinguished. If it were accurately true that the spectrum of a star could be fully described by specifying two physical parameters, such as temperature and pressure, there seems no reason why such subdivision should not be successful. In fact, though, as Curtiss has shown (*Handbuch d. Astrophysik*, **5**, 100, 1932), it leads to contradictory results at different observatories, or even at the same observatory if different criteria are used for distinguishing between the subdivisions. The idea of a system in which a spectrum can be described by giving numerical values to a limited number of physical attributes holds such attraction, however, that the subject has been examined in detail by O. Struve (*Astrophys. Journ.*, **78**, 73, 1933) and by Russell, Payne Gaposchkin and Menzel (*Astrophys. Journ.*, **81**, 107, 1935). These authors attack the problem from somewhat different standpoints, but though critical of the Harvard system, are all chary of recommending any basic modification of it. Still less do they suggest its abandonment in favour of a more satisfactory classification.

The trouble is this. In classifying stellar spectra as they did, the Harvard observers were actuated solely by practical motives. Each type of spectrum was distinguished from its neighbours in the sequence by the most conspicuous points of difference between them. Now these points of difference are not the same throughout the sequence, nor do they correspond to fundamental physical characteristics. For instance, in A and F stars the criteria used are the intensity ratios of the H and Ca^+ lines, in G stars those of the H and Fe lines, whilst in the later classes the emphasis is laid on band intensities. That these criteria are heterogeneous does

not worry the practical man. He looks, naturally enough, for the most obvious characteristics on the plate in front of him, and classifies his spectra accordingly, though he probably suffers twinges of conscience when he gets a different classification by using subsidiary criteria. But equally naturally, this procedure draws the fire of the theoretical astrophysicist. He would prefer a classification based on the most fundamental physical properties of the stars, the fewer the better. The spectrum of a star, he points out, is determined *au fond* by the physical processes involved in the passage of radiation from its intensely hot interior through the outer layers of its material. Any of the factors determining these processes—the effective temperature, say, or the level of ionisation in the atmosphere—would provide a more logical basis of classification than the multifarious discriminants used at present.

Unfortunately, however, the matter is not so simple as it sounds. Such parameters as these are not directly observable. The spectrum actually obtained is doubtless a complicated function of many of them. To disentangle them would involve appealing to any of several contradictory theories, none of which can yet be regarded as on safe ground. What it comes to is that we cannot have our cake and eat it: the two forms of classification are at present mutually exclusive. If we want to use observable parameters which are convenient in practice, we are forced back to the criteria already employed in the Harvard system, or at least to ones so closely allied to them that they give effectively the same result. Conversely, if we want fundamental parameters, we must put up with their inherent practical disadvantages, and be prepared for inconvenient revision of our classification with expanding knowledge.

It is not to be supposed, however, that the matter is entirely a quarrel between the practical astronomer, fanatically supporting from his observatory an empirical classification of dubious theoretical significance, and the mathematician, equally staunchly insisting from his armchair on an impracticable basis for a new classification. None realises better than the practical man the disadvantages of a system which, for example, takes no account of line profiles, the study of which is of great and increasing importance in astrophysics; or which at different points in the sequence places first on one parameter and then on another the burden of discriminating between neighbouring types. On the other hand, the theorist, though he is perfectly justified in pointing to the evidence we have that two stars with, say, the same effective temperature and the same absolute magnitude will exhibit practically the same spectra, is by no means blind to the defects of a system of classification based on these two

parameters. If the spectra of any group of stars are to be specified merely by attaching the appropriate numerical values to these attributes, we must first be satisfied that the stars have the same constituents in the same proportions in their atmospheres, that turbulence affects them all to the same extent, that they are all rotating at the same speed, that interstellar absorption is the same for all, and so on. Until the effects of these factors and many others on which the final spectra must be supposed to depend are better understood, it would be a false move to depart from the present well-established empirical system, however much its lack of homogeneity may be deplored.

There remains the possibility of refining the Harvard system as it stands. Mere subdivision, as we have seen, has gone as far as it profitably can. Any further improvement must probably come as the result of a guarded appeal to theory—not this time to furnish a new basis for a reconstructed classification, but simply to point out the most likely lines along which to extend the present one. Russell and his collaborators list 13 " prerequisites for a definitive classification." Amongst the more important may be mentioned the following : an exhaustive survey of representative spectra from 3000 Å. to 9000 Å. on high-dispersion spectrograms ; accurate measurements of line profiles, central intensities, and equivalent widths, made in conjunction with theoretical studies of the mechanism of line formation ; and an extended study of the chemical constitution of stellar reversing layers. Such a programme will evidently keep astronomers busy for years, but when the information does become available, there will undoubtedly exist grounds for reviewing the situation again. Stellar spectra are intimately related to so many other characteristics of the stars that an accurate system of classification is rightly regarded as indispensable.

It should perhaps be emphasised here that there is no question of modifying the Harvard system where statistical work on low-dispersion spectrograms is concerned. Here the Draper classification seems likely to remain supreme. It is only for the most detailed work that its adequacy is doubted. Even then its critics admit that modifications must be made with caution and as a result of knowledge which we are still in process of acquiring. We can evidently conclude that the Harvard system comes through its present test with flying colours.

Terrestrial Effects of Solar Disturbances.—We are perhaps fortunate that our Sun is such a well-behaved member of the stellar universe. Life as we know it, for instance, would not long survive any sudden decision on its part to exhibit, say, Cepheid

variability, or to become a nova. Careful observations made at the Smithsonian Institution do indeed show that the so-called *solar constant*, *i.e.* the rate at which radiation falls on unit area of a surface held normal to the sun's rays just outside the earth's atmosphere, may vary by 2 or 3 per cent. in a few days. Such variations, however, though interesting in themselves, are neither large enough nor long enough continued to have any practical importance. The meteorological effects produced by sunspots, too, are so minute that only statistical investigation of masses of data can disentangle them from the much larger random variations. Thus although there is a certain amount of correlation between the eleven-year sunspot cycle and the mean temperature at the earth's surface, and although the amount of tropical rainfall also exhibits a tendency to vary in an eleven-year period, such effects are very small.

The major effects of sunspots on the earth are magnetic. A close relation exists between the frequency of spots and those of magnetic storms and of auroral displays. The amplitude of the diurnal variation of magnetic declination follows quite closely the range in sunspot numbers. Moreover, the passage of an active spot across the central meridian of the sun is frequently followed some twenty or thirty hours later by a magnetic storm and a brilliant aurora. There seems little doubt that these effects are due to streams of electrified particles which are shot out from sunspots and which, reaching the earth, affect and are deflected by its magnetic field.

Within the last three years, however, quite another type of disturbance has been discovered, and shown to have terrestrial consequences of importance (*Journ. of Applied Physics*, **8**, 709, 1937). The spectrohelioscope, invented by Hale only some ten years ago, has now become part of the equipment of so many observatories that almost continuous observation of the sun is now possible, given good weather. Chief amongst the transient phenomena to the study of which this instrument is so well adapted are *chromospheric eruptions*, in which the brightness of localised parts of the solar disc, as observed in monochromatic light, increases enormously in a few minutes. These eruptions have been observed chiefly in the red light of hydrogen, but the other Balmer lines, and those of sodium, helium, magnesium and ionised calcium, have also been employed to study them. The typical eruption reaches its maximum brightness, which may exceed that of the continuum adjacent to the line in which it is observed, some five or ten minutes after it has been noticed, and gradually fades completely in the next forty or so. It often occurs in a disturbed region of the disc, and is sometimes but by no means always associated with a spot.

Such outbursts are never observable directly, in white light, so an ordinary telescopic image of the sun fails to show them, just as it fails to show the prominences.

There is no longer any doubt that with the brighter of these eruptions are associated periods of sudden fading in radio transmission at wave-lengths of about 20 metres. These often occur a few minutes after an eruption and last about the same time. Simultaneous small changes in the earth's magnetic force—much smaller than those caused by sunspots—and earth currents which may affect telegraphic communication appear also to be causally connected with these eruptions. It is significant that the effects reach a maximum at those points on the earth's surface where the sun's rays are vertical, and disappear on the dark side of the earth. The inference is unmistakable. Both the radio and the magnetic phenomena must surely be due to electromagnetic radiation travelling from a chromospheric eruption with the speed of light, and disturbing the earth's ionosphere. The fact that the eruptions effective in producing radio fading, unlike sunspots causing magnetic storms, favour no particular heliographic longitude supports this view: obviously radiant energy can reach the earth regardless of its place of origin on the disc. Radio soundings of the ionosphere during an eruption point to the creation of an intensely ionised region at the base of or just below the E-layer. To this region of improved conductivity must be ascribed the absorption of high-frequency radio signals, and the increase in the strength of the tidally-induced currents which on Balfour Stewart's theory cause the diurnal variations in magnetic force. The radiation which causes this abnormal ionisation is presumably more or less monochromatic ultra-violet emission from the chromosphere. It can hardly be continuous Planck radiation from a very hot area of the sun's surface, or else the outbursts would be visible in white light.

The theory dealing with these phenomena is naturally by no means complete. One wonders, for example, why every eruption is not effective in producing radio fading. Conversely, if these sudden short-wave attenuations are caused solely by solar disturbances—and no other cause is known—why are not all fade-outs preceded by chromospheric eruptions? Answers to such questions may be available in three or four years' time, when observations made during the present maximum of solar activity have been sifted. In any case, the information acquired will be welcomed alike by geophysicists interested in the electrical conditions prevailing in the ionosphere, and by astronomers whose chief concern is with this unruly behaviour of the solar chromosphere.

PHYSICS. By F. A. Vick, Ph.D., University College, London.

Vacuum Practice.—In these days of thermionics, photoelectricity, X-rays, etc., there is no need to stress the great importance of vacuum technique. Much progress has been made during the last ten years, especially since the introduction of the low vapour pressure oils and greases (C. R. Burch, *Proc. Roy. Soc.*, A, **123**, 271, 1929), and yet the books on vacuum practice published before this period (L. Dunoyer, trans. J. H. Smith: *Vacuum Practice*, 1926; F. H. Newman: *The Production and Measurement of Low Pressures*, 1925; G. W. C. Kaye: *High Vacua*, 1927; A. Goetz: *Physik u. Technik d. Hochvacuums*, 1926) still remain the standard works. Recent information is thus scattered, much of it appearing incidentally in papers on other subjects. An account of some of the advances in this field may therefore be of service.

Prior to 1929, mercury vapour diffusion pumps (condensation pumps), backed by rotary oil pumps, were universally used for the production of high vacua. The various types are described in detail in the books mentioned above and in a general article by Dushman (*J. Frank. Inst.*, **211**, 689, 1931). The general theory of their action is given by W. Molthan (*Zeits. f. Techn. Phys.*, **7**, 377 and 452, 1926; **8**, 195, 1927). These pumps are very efficient, but mercury has a vapour pressure at room temperature of 7×10^{-4} mm. of mercury, so it is quite essential that some kind of trap be placed between pump and apparatus. This usually takes the form of a liquid air trap. Not only have these traps the drawbacks of the expense and constant attention required to keep them cool, but if they are designed to be efficient in condensing mercury vapour the effective pumping speed is cut down considerably. It will be remembered that when the mean free path of the gas molecules is as long as the diameter of the connecting tubing, the "resistance to flow" is proportional to the length of the tubing and inversely proportional to the cube of the diameter (see, for example, Dunoyer, *loc. cit.*). It would obviously be a great advantage to replace mercury as a working fluid in the pump by one which has a negligible vapour pressure at room temperature, and eliminate the trap. As is well known, this was made possible by the vacuum distillation of oils by C. R. Burch (*loc. cit.*), the final products, the "Apiezon" series, having vapour pressures between 10^{-6} and 10^{-7} mm. Shortly afterwards *n*-butyl phthalate and other esters were shown to be possible substances (Hickman and Sanford, *Rev. Sci. Instr.*, **1**, 140, 1930; Hickman, *J. Frank. Inst.*, **221**, 383, 1936).

However, very soon after these oils became widely used, conflicting opinions became evident regarding the highest vacua

obtainable without traps, the lowest pressures recorded varying between 10^{-3} (Becker and Jaycox, *Rev. Sci. Instr.*, **2**, 773, 1931) and 10^{-8} mm. (Henderson, *ibid.*, **6**, 66, 1935). To short-circuit many speculations, we may say at once that these discrepancies can be traced either to lack of homogeneity of pump oil or to the errors in the use of gauges which will be discussed later. Even the most carefully prepared organic pump oil contains small quantities of heavier and lighter constituents. In ordinary diffusion pumps there is a temperature gradient from the hot boiler at the bottom to the high vacuum junction above the jets. The heavier components of the oil condense near the bottom, the lighter portions near the top, perhaps above the jets. The oil which has best opportunity of diffusing into the apparatus is thus the most objectionable portion. It will be seen that only a small quantity of lighter impurity in the oil will make a great difference because a large proportion of it will be in the vapour phase. The higher vapour pressure constituent is continually augmented by inevitable slight "cracking" of oil in the pump. The whole problem has been examined in valuable papers by K. Hickman (*J. Frank. Inst.*, **221**, 215 and 383, 1936), who describes pumps in which the oil is "self-fractionated." In effect, such a pump is split into a number of jets and reservoirs in series, connected so that the condensate from each jet is collected in the reservoir nearest the backing side, leaving the lowest vapour pressure constituents nearest the high vacuum connection. An interesting extension of this principle has been employed by Lockenvitz (*Rev. Sci. Instr.*, **8**, 322, 1937) in the design of a six-stage pump using ordinary commercial lubricating oil. A final pressure of less than 10^{-6} mm. without a cooled trap is claimed. A further design has been published recently by Malter and Marcuvitz (*Rev. Sci. Instr.*, **9**, 92, March 1938). In all oil diffusion pumps it seems desirable to provide some sort of baffle at the top, in the path of oil molecules moving towards the apparatus from the top jet. To quote one example, a combined baffle and solid CO_2 trap has been described by More, Humphreys and Watson (*Rev. Sci. Instr.*, **8**, 263, 1937), consisting of two concentric brass tubes, the inner one being closed at the lower end to hold the solid CO_2, and the outer attached to the head of the pump, with a side tube at the top going to the apparatus. Between the tubes are two helical ramps diametrically opposed to each other, each making one turn in the 12-inch length of the baffle and reaching to just below the side tube. (See also Edwards, *Rev. Sci. Instr.*, **6**, 145, 1935; Burch and Sykes, J.I.E.E., **77**, 129, 1935.)

One of the drawbacks of oil as a pump filling is the solution in

it of quantities of air and vapours ; indeed, it may be said that often the rate of attainment of a vacuum is limited by the rate of conditioning of the pump oil. In many cases it is advisable to have a (large) tap between pump and apparatus and keep it closed until the pump has " de-gassed " its own oil. Such a tap is useful also if it is desired to admit air to the apparatus without waiting for the pump to cool, because the oil is damaged by exposure to air while hot. It is more sensitive to boiler temperature than mercury and requires a lower backing pressure. Thus a pump with a large jet, using Apiezon A (vapour pressure 10^{-7} mm.), has another diffusion pump using Apiezon B (10^{-5}) between it and the backing pump. To offset these disadvantages, oil diffusion pumps may, under proper conditions, be operated without a cooled trap (but with the baffle mentioned above) down to 10^{-5} or 10^{-6} mm. The oil wets glass and metal and so does not form globules round the jets ; also it does not readily " bump " on heating. There have been several suggestions for preventing bumping of mercury, for example Boddy (*Rev. Sci. Instr.*, **5**, 278, 1934) uses induction heating, while others (*e.g.* Bearden, *Rev. Sci. Instr.*, **6**, 277, 1935) employ immersion heaters.

An important point sometimes overlooked in connection with pumping speed is that at about 10^{-1} mm. the speed of rotary pumps has fallen already to perhaps a quarter of the maximum, while diffusion pumps have hardly started working, reaching their maximum speed at about 10^{-3} mm. Kerris (*Archiv. f. Tech. Mess.*, **65**, 153, 1936) gives speed-pressure curves for these pumps and also of a special mercury-jet pump designed to cover the region 0·1 to 0·01 mm. For the determination of pumping speed see Matricon, *J. Phys. Rad.*, (7), **3**, 127, 1932.

To return to the problem of measuring the final pressures, an ionisation gauge is commonly used. It will be recalled that this is similar to a three-electrode thermionic valve, the electrons emitted from a heated filament being accelerated by a positively charged grid, and the positive ion current, resulting from collisions with gas molecules, collected by a negatively charged electrode. This gauge may give false readings when used with an oil diffusion pump for two reasons. The lighter constituents of the pump oil diffusing into the apparatus may be decomposed at the hot filament of the gauge, giving rise to localised high pressure. On the other hand, the vapours of pump oils are powerfully adsorbed on clean glass and metal surfaces, so that if the gauge has just been baked, oil vapour present may adsorb on the gauge walls and connecting tubing, and the readings will be misleadingly low.

The theory of the ionisation gauge is discussed by Morgulis (*Phys. Zeits. Sowjet*, **5**, 407, 1934). It is sufficient here to state that below 10^{-4} mm. the positive ion current is proportional to the gas pressure and to the electron current, for a given gas and gauge. Jaycox and Weinhart have described a carefully designed gauge (*Rev. Sci. Instr.*, **2**, 401, 1931), but recently most attention has been paid to keeping the electron current constant. Jaycox and Weinhart used an on-off regulation of the filament current by a mechanical relay through which the electron current flowed. Overbeck and Mayer (*Rev. Sci. Instr.*, **5**, 287, 1934) use a saturated transformer, while Hoag and Smith employ a thyratron (*ibid.*, **7**, 497, 1936).

Copley, Phipps and Glasser (*Rev. Sci. Instr.*, **6**, 371, 1935) describe a small ionisation gauge for detection of molecular rays, but this is more the province of the Pirani hot-wire gauge, in which advantage is taken of the dependence of thermal conductivity of a gas with pressure in the range 10^{-1} to 10^{-4} mm. See Fraser's *Molecular Rays*, pp. 34–41 (1931), and Ellett and Zabel, *Phys. Rev.*, **37**, 1102, 1931. An interesting summary of recent improvements in gauges is given by Penning (*Philips Tech. Rev.*, **2**, 201, 1937).

To ascend to regions of higher pressures for a moment, there have been several attempts to use the new low vapour pressure oils in U-tube manometers, but the main trouble is the solution of gas in the oil and its subsequent liberation in the closed side. This has been removed either by tilting the gauge and degassing the oil just before working (Malmberg and Nicholas, *Rev. Sci. Instr.*, **3**, 440, 1932), or even by a miniature pump to evacuate the "closed" side (Hickman, *J. Frank. Inst.*, **213**, 142, 1932).

The ease with which oil vapours can be adsorbed has been mentioned already, and this property has been employed in the construction of a trap for use with oil diffusion pumps. In its early form (Becker and Jaycox, *Rev. Sci. Instr.*, **2**, 773, 1931) it consists of activated charcoal, from which the gases adsorbed from the atmosphere have been removed by preliminary heating in vacuum, placed in a nickel gauze container in the connecting tube between the apparatus being exhausted and the pumps. After the backing pump has reduced the pressure somewhat, the trap, apparatus and gauge are baked together, and when the charcoal has reached 400° C. the diffusion pump is started. The period of baking may be twenty minutes or several hours. The charcoal is cooled down just before the current is switched off from the heater surrounding the rest of the apparatus, and at room temperature adsorbs oil vapours coming from the pump and water vapour, etc., from the apparatus as a whole. A pressure of 10^{-7} mm. can easily be

reached in this way, without using a refrigerant round the trap. At room temperature the trap does not adsorb appreciable amounts of mercury vapour. Any oil which is adsorbed on to the charcoal is decomposed when the charcoal is heated, and so does not come off as oil. Recently, P. A. Anderson (*Rev. Sci. Instr.*, **8**, 493, December 1937) has described a modification of this trap in which the charcoal is heated by passing an electric current through it. Sometimes a charcoal or silica gel trap is placed between the diffusion pump and backing pump. For mercury vapour Finch (*Ergeb. d. Exakt. Naturwiss.*, **16**, 367, 1937) has used a trap containing staggered tiers of tinfoil.

Returning to liquid air traps, it is sometimes useful to remember that the height of refrigerant cooling a trap can be used for regulating the (low) pressure in an enclosure, by altering the amount of gas and vapour adsorbed (Johnson and Vick, *Proc. Roy. Soc.*, A, **158**, 60, 1937).

Another development of the last few years which has had far-reaching effects is the improvement of glass to metal seals. Not only has platinum been replaced by copper-plated nickel-iron and other alloys (Scott, *J. Frank. Inst.*, **220**, 733, 1935), but tubular seals of large diameter have been increasingly successful since Housekeeper originally described the joining of copper and glass tubes (*J. Amer. Inst. Elec. Eng.*, **42**, 954, 1923). In the latter case the edge of the copper is made so thin that on cooling it may be plastically deformed without causing the glass to break. (For sealing copper to pyrex see Skinner and Burrow, *J. Sci. Instr.*, **7**, 290, 1930.) The development of new chrome-iron alloys has made even this precaution unnecessary. A lathe with two chucks is used, one holding the chrome-iron tube and the other the glass tube. While both are turning, the edge of the metal tube is slightly oxidised and glass deposited on the hot edge by hand, from a thin glass rod. The edge of the glass tube is then heated, and the two chucks brought together (still rotating), the glass-glass seal being made in the ordinary way. Such joints are used in large X-ray tubes and transmitting valves (van Embden, *Philips Tech. Rev.*, **2**, 306, 1937). Much information on glasses and metals used in vacuum work is collected in a recent book by Espe and Knoll (*Werkstoffkunde der Hochvakuumtechnik*, 1936).

It is obvious that in a short article of this kind it is possible to mention only a very few of the developments of a subject in which so much ingenuity and thought are being applied.

GENERAL AND ORGANIC CHEMISTRY. By O. L. BRADY, D.Sc., F.I.C., University College, London.

CONDENSATIONS BROUGHT ABOUT BY MAGNESIUM AND ITS COMPOUNDS.—Magnesium and its compounds seem to be particularly effective in inducing certain condensations to take place and fresh examples come to light from time to time.

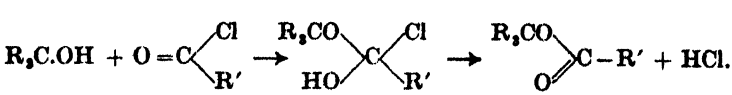
Phenylmethylamino magnesium bromide, PhMeN.MgBr, prepared from methylaniline and a Grignard reagent, has proved useful in an ether-benzene medium for extending the aldol condensation of ketones (Grignard and Colonge, *Compt. rend.*, 1932, **194**, 929; Colonge, *Compt. rend.*, 1933, **196**, 1414; Colonge, *Bull. Soc. chim.*, 1934, [V], **1**, 1101). Previous to the introduction of this reagent, ketones containing a tertiary group attached to the carbonyl group could not be made to undergo the aldol condensation, but with it all ketones condense provided there are not less than three hydrogen atoms attached to the carbons contiguous to the carbonyl group. Thus, pinacoline yields 2 : 2 : 3 : 6 : 6-pentamethylheptan-3-ol-5-one.

$$2CH_3.CMe_2.CO.CH_3 \longrightarrow CH_3.CMe_2.CMeOH.CH_2.CO.CMe_2.CH_3$$

4-Methylhexan-3-one, $CH_3.CH_2.CO.CHMe.CH_2.CH_3$ also undergoes the condensation, but *tert*-butylethyl ketone $Me_3C.CO.CH_2Me$ and di-*iso*propyl ketone $Me_2CH.CO.CHMe_2$ do not.

Tertiary alcohols cannot be esterified except with great difficulty, for example, tertiarybutyl alcohol and acetic acid for 200 hours at 155° give only about 7 per cent. of ester; the addition of dehydrating agents or the use of acyl chlorides or anhydrides involve difficulties owing to the ease with which the tertiary alcohols lose water to give unsaturated hydrocarbons. Spassov (*Ber.*, 1937, **70**, 1926) has got over the difficulty by adding an acyl chloride dissolved in ether to a solution of the alcohol in ether containing magnesium powder. Using such alcohols as tertiarybutyl alcohol, dimethylethyl carbinol, methyldiethyl carbinol, triethyl carbinol and tribenzyl carbinol with acetyl, propionyl, butyryl and *iso*butyryl chlorides, yields of 50–90 per cent. of the corresponding esters were obtained; triphenyl carbinol, however, could not be acylated.

The author represents the reaction as addition of the alcohol to the carbonyl group of the acyl chloride followed by loss of hydrogen chloride which reacts with the magnesium.



Magnesium is specific and it cannot be replaced by aluminium, iron or zinc.

Esters of certain aliphatic monocarboxylic acids, for example, ethyl tertiarybutyl acetate, $Me_3C.CH_2.CO_2Et$, do not undergo the aceto-acetic ester condensation in the presence of sodium ethoxide although they contain an α-methylene group. Spielmann and Schmidt (*J. Amer. Chem. Soc.*, 1937, **59**, 2009) have found that if a 0·2 molecular proportion of mesitylmagnesium bromide, $Me_3C_6H_2MgBr$, is used as the condensing agent ethyl tertiary-butylacetate gives a 32 per cent. yield of ethyl α-γ-di-tertiary-butylacetoacetate

$$2Me_3C.CH_2CO_2Et \longrightarrow Me_3C.CH_2.CO.CH(CMe_3).CO_2Et + EtOH.$$

Similarly ethyl *iso*valerate gave 51 per cent. of ethyl *iso*valeryl*iso*-valerate and ethyl *iso*butyrate, 26·5 per cent. of ethyl tetramethyl-acetoacetate.

Reference may also be made to the work of Hauser and Renfrow (*J. Amer. Chem. Soc.*, 1937, **59**, 1823), who find that ethyl *iso*butyrate will undergo the aceto-acetic ester condensation if sodium triphenyl-methyl is used as the condensing agent.

When an equivalent of magnesium filings mixed with aluminium filings and activated by the addition of a trace of iodine is added to a solution of benzaldehyde in toluene a vigorous reaction takes place and benzoin is formed ; if insufficient magnesium is employed benzyl benzoate is the product (Shornigin, Isagubzantz and Guseva, *J. Gen. Chem. U.S.S.R.*, 1934, **4**, 683).

$$2\ PhCHO \longrightarrow PhCHOH.COPh \qquad 2\ PhCHO \longrightarrow PhCO_2.CH_2Ph.$$

The binary mixture of magnesium and magnesium iodide brings about some interesting reductions in ether (Gomberg and Backmann, *J. Amer. Chem. Soc.*, 1928, **50**, 2762). Aromatic acids give iodo magnesium salts which are slowly reduced on boiling in ether by excess of the binary mixture to compounds which are decomposed by water to give benzil and benzoin.

$$R.CO_2H \longrightarrow 2R.CO_2H,\ MgI_2,\ 2Et_2O \longrightarrow$$

$$R.C(OMgI)_2.C(OMgI)_2.R \qquad R.C(OMgI):C(OMgI)R$$

$$\downarrow \qquad\qquad\qquad\qquad \downarrow$$

$$R.CO.CO.R \qquad R.CO.CH(OH).R$$

A yield of 43 per cent. of a mixture of benzil and benzoin was obtained from benzoic acid.

Esters of aromatic acids are decomposed by the magnesium iodide to alkyl iodides and iodo magnesium salts. These react with the magnesium-magnesium iodide mixture to give compounds

which with water give benzoins and hydrocarbons. Thus benzylbenzoate gives benzoin and dibenzyl.

$$2PhCO_2.CH_2Ph \longrightarrow PhCO.CHOH.Ph + PhCH_2.CH_2Ph.$$

Reduction by Aluminium Alkoxides.—Solutions of the alkoxides of aluminium in the corresponding alcohols have been found to be valuable agents for the purpose of reducing aldehydes and ketones to primary and secondary alcohols. (Verley, *Bull. Soc. chim.*, 1925 [IV], **37,** 537, 871; 1927 [IV], **41,** 788; Meerwein, *Annalen*, 1925, **444,** 221; *J. pr. Chem.*, 1936, **147,** 211; Lund, *Ber.*, 1937, **70,** 1520.) When, for example, an aldehyde is heated with aluminium ethoxide, the following equilibrium is set up:

$$RCHO + CH_3.CH_2Oal \rightleftharpoons R.CH_2Oal + CH_3.CHO \quad (al = \tfrac{1}{3}Al).$$

If the aldehyde on the right is more volatile than that on the left and is removed together with the excess of alcohol by vaporisation, the RCHO is almost quantitatively converted to the aluminium alcoholate, which is readily decomposed by water. The aluminates of primary alcohols will not reduce ketones, but if aluminium *iso*propoxide is used both aldehydes and ketones may be satisfactorily reduced. The reagent may be easily prepared by dissolving aluminium foil in an excess of *iso*propyl alcohol containing a small amount of mercuric chloride, the aldehyde or ketone is then added and the acetone and excess of *iso*propyl alcohol distilled away.

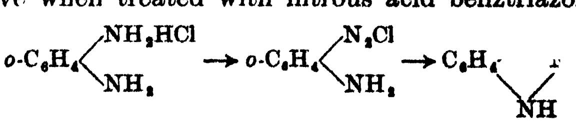
$$RR'C = O + Me_2CHOal \longrightarrow RR'CHOal + Me_2CO.$$

The peculiar advantage of this method is that other reducible centres in the molecule are not attacked, thus cinnamaldehyde gives cinnamyl alcohol

$$PhCH : CH.CHO \longrightarrow PhCH : CH.CH_2OH,$$

the nitrobenzaldehydes give nitrobenzyl alcohols and bromal gives tribromethyl alcohol. The yields are good, often over 90 per cent., and many types of compounds can be reduced, aliphatic and aromatic aldehydes and ketones, cyclic ketones, such as *cyclo*hexanone, camphor and xanthone and diketones like benzil. Easily enolised ketones such as ethyl acetoacetate are not reduced, neither are phenolic ketones and ketonic acids whose aluminium salts are insoluble in *iso*propyl alcohol.

Tetrazotisation of Aryl Diamines.—The tetrazotisation of aryl diamines has always presented difficulties, especially in the case of *o*- and *m*-diamines; under ordinary conditions salts of the former give when treated with nitrous acid benztriazoles



and the latter aminoazo-compounds, such as Bismarck brown.

$$m\text{-}C_6H_4(NH_2)_2 \longrightarrow (NH_2)_2.C_6H_3.N{:}N.C_6H_4.N{:}N.C_6H_3(NH_2)_2.$$

The difficulty of obtaining the bisdiazonium salts prevented the employment of the Sandmeyer reaction in many cases where it would be particularly useful. Hodgson and Walker (*J. Chem. Soc.*, 1935, 530) have overcome this difficulty by dissolving the diamine in glacial acetic acid and adding the solution gradually at a temperature below 30° to a well-stirred solution of sodium nitrite in concentrated sulphuric acid. When the solutions were poured into cuprous chloride in hydrochloric acid 70 per cent. yields of the dichlorobenzenes were obtained.

PREPARATION OF DIAZOAMINO-COMPOUNDS.—The preparation of diazoamino-compounds by the usual methods given in text-books of preparative organic chemistry, namely, the action of sodium nitrite on a mixture of, for example, aniline and aniline hydrochloride, almost invariably gives dark-coloured compounds not infrequently mixed with tar. The compounds, however, can be obtained in beautiful clear yellow needles by passing a stream of carbon dioxide for some hours through an aqueous alcoholic solution of the base and two equivalents of sodium nitrite (Le Fèvre and Vine, *J. Chem. Soc.*, 1937, 1807).

PHYSICAL CHEMISTRY. By H. W. MELVILLE, D.Sc., Ph.D., Colloid Science Laboratory, Cambridge.

REACTIONS IN SOLIDS.—Compared with the enormous volume of work done on gaseous and liquid systems, experiments on reactions in solids would appear to have been avoided by chemists, and for a very good reason. The fixed positions occupied by atoms or groups of atoms in a crystal lattice preclude their interaction with anything but their nearest neighbours. In consequence, reactions in solids almost invariably reduce to a study of reactions at solid-solid interfaces. The problem is thus much more complicated than that in heterogeneous gas reactions because of the immobility of the atoms constituting the interfaces. As Bernal (*Trans. Faraday Soc.*, July 1938) has pointed out, there are essentially two types of reactions in solids : (*a*) those in which there is no transfer of matter across the interface and (*b*) those in which matter does cross the interface. In the first class are the polymorphic transformations which rightly belong to the domain of the X-ray crystallographer, since such transformations can most satisfactorily be followed by X-ray spectrography. The numerous interesting results obtained in the study of these processes tend to show that this simpler type of reaction will prove the more amenable to systematic treatment.

At present, however, the second class is of more interest to the chemist and in this article some account will be given of the progress recently made in this direction.

Diffusion in Solids.—First of all mention may be made of some measurements of diffusion in solid systems, in order to emphasise the point made at the beginning of the article. That diffusion of one solid into another could occur at a measurable speed was shown many years ago by Roberts-Austen in his classical experiments on the diffusion of gold through lead. Expecting that self-diffusion in lead could be measured just as easily by utilising the radioactive isotopes Thorium-B or Radium-D, Groh and Hevesy (*Ann. der Physik*, **63**, 85, 1920) made the surprising discovery that at 165° C. the self-diffusion coefficient of lead is 10^6 times less than that of gold in lead, the figures being $1{\cdot}2\ 10^{-14}$ and $5{\cdot}8\ 10^{-8}$ cm.^{2} sec.$^{-1}$ respectively. Some indication of the sensitivity of the radioactive method for following solid diffusions developed by Hevesy and Seith (*Z. Physik*, **56**, 79, 1929; **57**, 869, 1929; **79**, 197, 1932) may be had from the fact that it is estimated that a diffusion coefficient of 10^{-18} cm.^{2} sec.$^{-1}$ could be measured. (For comparison the diffusion coefficient of the common gases at atmospheric pressure is of the order 10^{-1} cm.^{2} sec.$^{-1}$.) With the latter value for the diffusion coefficient a molecule would change places with a neighbour only 10^{-8} cm. away, once in 100 seconds. The sensitivity of the method is due to the fact that the concentration of the radioactive isotope *in* the solid is not measured; instead, the radioactive material is deposited on the surface of the lead and the decrease of ionisation with time determined above the radioactive lead, allowance being made for the fact that the range of α-particles in lead is 50μ. Hevesy (Hevesy and Seith, *Z. Elektrochem.*, **37**, 528, 1931; Hevesy, *ibid.*, **39**, 491, 1933) suggested that the relatively high mobility of the gold compared with that of the lead was due to the fact that the attractive force between gold and lead atoms is much smaller than that between two lead atoms. Consequently it was anticipated that in metals more akin to lead the diffusion coefficient of lead should decrease the more closely that metal approached lead in its properties. The experiments of Seith (*Z. Elektrochem.*, **39**, 33, 1933; **41**, 872, 1935) showing that the diffusion coefficients decrease in the order silver, cadmium, mercury, bismuth, thallium and tin, confirms this supposition. At 250° C. the diffusion coefficients for Pb-Au, Pb-Sn and Pb-Pb are respectively $3{\cdot}5 \times 10^{-7}$, $1{\cdot}5 \times 10^{-11}$, and $5{\cdot}1 \times 10^{-11}$, cm.^{2} sec.$^{-1}$. The criterion for such diffusion to be observed is that the metals form solid solutions. Gold just fulfils this condition, the solubility being 0·09 atoms per

cent. at the temperatures used for diffusion. A less soluble metal would probably diffuse more quickly but a practical risk in the formation of a two-phase system would arise. The diffusion of gold is therefore probably the fastest diffusion process which can be observed in lead.

By the use of the radioactive isotope of gold McKay (*Trans. Faraday Soc.*, July 1938) has measured the self-diffusion coefficient of gold. Radioactive gold can be produced in the usual way by bombarding ordinary gold with neutrons. Neutrons of the velocity range used in these experiments are half absorbed in a layer of gold 0·017 mm. thick. Two methods were used. The first method consisted in rolling a piece of activated gold foil 0·01 mm. thick with another thicker piece through which diffusion was to be measured. The second method consisted in allowing neutrons to impinge on gold foil after passing through a cadmium filter, the other side of the gold being protected by a thick gold filter to which the neutrons were impermeable. In this case, ionisation is produced by β-particles and allowance had of course to be made for the absorption in gold before the diffusion coefficient could be calculated. The two methods gave results in general agreement, although the coefficients obtained by the first method were about twice those found by the second. At 917° C., for example, the average value of the coefficient was $1 \cdot 0 \times 10^{-9}$ cm.^{2} sec.$^{-1}$ The extrapolated value for the gold system at 165° C. is $6 \cdot 1 \times 10^{-25}$ cm.^{2} sec.$^{-1}$ compared with $1 \cdot 2 \times 10^{-14}$ cm.^{2} sec.$^{-1}$ for lead at the same temperature.

Unfortunately the data are not yet sufficiently comprehensive to indicate whether there is any relationship between diffusion coefficient, energy of activation for diffusion and the magnitude of the deviation of the behaviour of the solid solutions from Raoult's law.

Of the various types of reactions in solids, the decomposition of detonating substances, such as azides and the dehydration of salt hydrates, have been experimented on most frequently. In spite of this work, it cannot yet be said that the problem is completely solved, but in recent years it has become systematised and clarified to an extent which justifies a review of the present position of the subject.

Decomposition of Detonating Substances.—By a detonating substance is meant any solid body which, when heated to a temperature below the melting point, decomposes explosively, the velocity of the explosion wave being of the order of several thousand metres per second—a velocity comparable with that of a detonation wave in a gaseous system. The substances studied from the point of

view of elucidating the nature of the solid reaction are the alkali and alkaline earth azides, lead azide, so-called nitrogen tri-iodide, lead styphnate $PbO.CH(OH)_2(NO_3)_3$ and mercury fulminate $Hg(ONC)_2$. The general characteristics exhibited by these molecules are (*a*) that on heating they decompose after the lapse of an induction period, reaction thereafter accelerating, reaching a steady state and finally declining as the decomposition proceeds to completion; (*b*) that detonation may occur after an induction period. When the products of the reaction are solid it is evident that the reaction occurs at the interface between decomposed and undecomposed crystals. The question thus arises as to how this interface is formed in the first place. In view of the fact that a perfect crystal cannot be prepared, it is most probable that the centres from which reaction starts are associated with imperfections and traces of adventitious impurities present in the crystal. It might be thought that any uncertainty in this connection could be eliminated by generating artificially and in a controlled manner such centres by means of α-particles (Garner and Moon, *J. Chem. Soc.*, 1398, 1933), fast electrons or X-rays. Unfortunately none of these agents is very effective, though Kallman and Schrankler (*Naturwiss.*, **21**, 379, 1933) have shown that ions of hydrogen, mercury and argon are effective for this purpose. Recent work by Maggs (cited by Garner, *Trans. Faraday Soc.*, July 1938) has shown that ultra-violet light is effective in producing centres for the production of nuclei in the alkaline earth azides, since the induction period for reaction is very materially reduced. If some idea of the primary process could be obtained, this method would seem to be a very promising one for the investigation of precisely how nuclei grow from such centres of disorganisation.

In order to provide data from which a mechanism may be constructed, reliance must therefore be placed mostly on the variation of decomposition rate and detonation time with temperature. First of all, the explosive decomposition may be considered (cf. Garner, *J. Chem. Soc.*, 720, 1934, and *Trans. Faraday Soc.*, July 1938). In lead azide, for example, the velocity of the detonation wave is $5{\cdot}3\ 10^5$ cm. sec.$^{-1}$ Since molecules are separated from each other by distances of the order of 10^{-8} cm., the detonation wave will pass through a layer of molecules in 10^{-13} seconds, which time is incidentally comparable with the frequency of molecular vibrations. Such a detonation wave thus travels at the maximum velocity. The decomposition of one molecule of lead azide is accompanied by the evolution of 106 kg. cal., which prompts the following question: Is the propagation of the detonation wave due

to the specific transfer of energy from reacted molecules to a relatively small number of adjacent molecules of azide, or is the energy spread over a wider volume, thereby raising the temperature of that part of the crystal as a whole? This is an extremely difficult point to settle. Were the latter alternative correct, it would be expected that the artificial generation of nuclei would have reduced the temperature of detonation, whereas in fact the effect is small.

From measurements on the rate of decomposition of lead azide it can be shown that the energy of activation required for decomposition is 38 kg. cal. This energy together with the exothermic heat of reaction would only activate three or four neighbouring molecules of lead azide and thus is insufficient to start a spherical detonation wave. Such a spherical wave could, however, be started if two molecules of lead azide were to decompose simultaneously side-by-side sufficiently frequently. Garner (*loc. cit.*) has shown that this possibility is capable of realisation. Lead azide does not explode *in vacuo* below 290° C., at which temperature $1{\cdot}1 \times 10^{16}$ molecules react per cm.^{2} per sec. This is equivalent to 30 layers of molecules decomposing per sec. and therefore the probability that two molecules will decompose in adjacent spaces in the interface within 10^{-13} sec. is 4×10^{-9}, or such a coincidence would occur 4×10^4 times per sec. Since the induction period before detonation is 20 sec. at this temperature, the process is seen to be very probable. Similar calculations show that a ternary event only occurs once in every 10^7 sec. and can thus be neglected.

In contrast to the behaviour of the azides is the decomposition of nitrogen tri-iodide, $NI_3.NH_3$, which yields $N_2 + 2NH_3 + 3I_2$ (Garner and Latchem, *Trans. Faraday Soc.*, **32**, 567, 1936; Meldrum, *ibid.*, July 1938). It is not possible to compare this reaction directly with any other, for even at 0° C., if the pressure of any gas above the solid is reduced below some 2×10^{-2} mm., detonation occurs immediately. Above this pressure the ammonia, but not the iodine, is so strong an inhibitor that the stable decomposition can readily be followed at pressures of ammonia up to 375 mm. It is suggested that NI_3 molecules either are or furnish the nuclei for the initiation of detonation. When, however, ammonia is present in the gas phase the velocity of the formation of NI_3 is so much cut down that no detonation wave is started.

THE SLOW DECOMPOSITION OF DETONATORS.—In dealing with the slow decomposition, matters might conceivably be simplified by studying substances which yield volatile products, such as sodium and potassium azide (Garner and Marke, *J. Chem. Soc.*, 657, 1934). These azides decompose at measurable rates between 270° and 330° C.

There is at first a fast reaction, which soon stops, simultaneously accompanied by erosion along the crystal cracks; this stage is succeeded by a long induction period terminated by a rapid reaction in which the rate of decomposition dp/dt (p is the pressure of the N_2 evolved) is given by the equation

$$\frac{dp}{dt} = e^{kt}, \text{ (k is a constant)}$$

which holds for all these reactions. During the second stage the crystal assumes a honeycomb structure owing to the selective activity of certain crystalline blocks. Unfortunately the results are not very reproducible and no reliable conclusions can be drawn with regard to the mechanism. The most surprising feature of the reaction is the very large acceleration caused by the presence of the corresponding alkali vapours, the same velocity of decomposition being obtained at 100° lower in the temperature scale. By so performing the experiments, an interface is thereby created, leading to enhanced activity. At the same time the results become more reproducible. The two-stage character of the decomposition is eliminated, there being only a period of induction of short duration. The following table summarises the results:

Substance . .	NaN_3*	KN_3†	BaN_6	CaN_6‡	α-PbN_6	β-PbN_6
Range of temp. for decomp., ° C. . . .	240–275	222–255	100–130	60–130	222–260	200–270
E, kg. cal.	34·4	35·1	21	18–19	38	37

* In presence of Na. † In presence of K.
‡ Marke, *Trans. Faraday Soc.*, **33**, 775, 1937.

The one regularity immediately apparent is that the lower the energy of activation, the smaller the temperature at which a given velocity of decomposition is observed. Actually the above results fall into two fairly well-defined groups and it would therefore be desirable to observe the behaviour of similar substances having different values of E in order to see whether the general conclusion may be drawn that the absolute velocity of reaction is governed, among a group of similar substances, by the energy of activation for the propagation of decomposition.

In order to account for the behaviour of the stable decomposition of detonating substances, Garner and Hailes (*Proc. Roy. Soc.*, A, **139**, 588, 1933) have put forward the following hypothesis. Decomposition commences at nuclei, formed on crystal surfaces and cracks, from imperfections in the crystals during the induction period. By

grinding the crystals, such imperfections are increased in number, with the result that the induction period may be almost eliminated, although the rate of subsequent reaction is unaffected. When the nuclei starts to spread and reaction becomes apparent each molecule of azide, on decomposition, activates one molecule in its immediate vicinity. Occasionally two molecules may be so activated. Such a process of energy transfer cannot continue indefinitely. It may come to a stop when an intercrystalline crack is encountered or even a Smekal crack in a single crystal. Further, if there is present any considerable amount of decomposed crystal, there is a high probability that the energy is uselessly wasted in activating molecules of the product. Thus it happens that a partially decomposed crystal will refuse to detonate under conditions where a fresh crystal will explode readily.[1] Hence if N_0 is the number of centres of reaction generated per sec. and K is the number of times per sec. two molecules are activated, then

$$\frac{dN}{dt} = N_0 + KN,$$

N being the number of reaction centres in existence at time t. Therefore on integration

$$N = \frac{N_0}{K}(e^{Kt} - 1)$$

and if $e^{Kt} \gg 1$

$$ln N = Kt + ln\frac{N_0}{K}.$$

But since $\frac{dp}{dt}$, the rate of increase of pressure, which gives the rate of decomposition, is proportional to N,

$$ln\frac{dp}{dt} = k_1 t + \text{const.},$$

which accounts for the variation of rate with time after the lapse of the induction period. k_1 is temperature dependent being proportional to $e^{-A/RT}$, where A is a constant having the dimensions of energy and may be termed an energy of activation. Garner and Hailes suggest that it is a measure of the energy of activation for the decomposition of the explosive group in the molecule, for example (CNO) in fulminate. The "constant" in the above equation will vary with temperature, since both N_0 and K will be

[1] It is of interest to note that the entity formed during the process is very stable, for, if the heating be interrupted and subsequently recommenced, the induction period is not increased.

temperature dependent, but to a smaller extent than *k*. With mercury fulminate it is significant that the value − 30 kg. cal.—calculated from the stable decomposition—has practically the same value as that calculated from the temperature coefficient of the length of the induction period preceding detonation. This fact, if not fortuitous, would indicate that there is a very close connection between the two processes.

Dehydration of Salt Hydrates.—In an attempt to get at the root of the problem of the mechanism of the removal of water from salt hydrates, Garner and his co-workers (Bright and Garner, *J. Chem. Soc.*, 1872, 1934; Garner and Southon, *ibid.*, 1705, 1935) have made a very extensive study of the formation of nuclei, from which dehydration spreads, on crystals of $CuSO_4.5H_2O$, $NiSO_4.7H_2O$ and chrome alum, $K_2SO_4.Cr_2(SO_4)_3,24H_2O$. In these experiments the number and also the linear rate of growth of nuclei were measured microscopically, the lower limit for size being about 10^{-3} cm. As has been pointed out above, such nuclei in crystals are probably produced in the first place by some disturbance in the lattice. From that disturbance the nuclei grow until they can be observed microscopically. As might be expected from the complex lattice structure, the nuclear growth varies with direction, giving rise to elongated star-shaped bodies with $CuSO_4.5H_2O$ (which is dehydrated to monohydrate). In one crystal—chrome alum—the nuclei grow uniformly, in the initial stages, in all directions. The facility for growth in any one direction is probably governed, among other factors, by the ease with which water can escape in that direction (see especially, Garner and Pike, *J. Chem. Soc.*, 1565, 1937). The essential features of the behaviour of visible nuclei is that there is an induction period before they appear or increase in number, if there are some already present; thereafter the number increases linearly with time, except with $NiSO_4.7H_2O$, where the number increases with the square of the time. Above 10 cm.$^{-3}$ linear growth is also proportional to time. The striking characteristic of these phenomena is that, in spite of the fact that the observed behaviour must undoubtedly be an average of a number of processes of somewhat different velocity, yet the temperature coefficient of each is approximately identical—a fact which would indicate a very close connection between them.

The most difficult observation to explain is the small rate of growth of the nuclei, when small in size, 10^{-3} cm. being only an upper limit fixed by the usable magnification of the microscope. Cooper and Garner (*Trans. Faraday Soc.*, **32**, 1739, 1936) make the suggestion that, since there is a contraction produced during

dehydration, a negative tension is set up which reduces the vapour pressure of the hydrate and therefore rate of evaporation of water to such a small value that nuclear growth, in the initial stages, is extremely slow. In fact, it is possible to make a partial test of the theory when the nuclei begin to grow at a significant speed. Assuming that the tension is independent of nuclear size, it can be shown from the theory of the variation of vapour pressure with interfacial curvature that

$$\mathrm{RT}\log_e p = \frac{2\mathrm{M}s}{\rho}.\frac{1}{r} + \mathrm{RT}ln p_0,$$

where p is the vapour pressure of a spherical drop of radius r and density ρ, p_0 is the pressure above a plane surface, M is the molecular weight of the vapour and s the surface tension. Now since the linear rate of growth, dr/dt, is equal to the rate of evaporation and therefore, as a first approximation, to p, the above equation may be transformed to

$$\mathrm{RT}\log_e\frac{dr}{dt} = \frac{\mathrm{K}_1}{r} + \mathrm{K}_2{}^2,$$

where K_1 and K_2 are constants. This relationship actually holds exceedingly well between the values $r = 10^{-3}$ and 10^{-2} cm., the tension working out at the remarkably high figure of $7{\cdot}5 \times 10^6$ dynes cm.$^{-1}$

GEOLOGY. By G. W. TYRRELL, A.R.C.Sc., D.Sc., F.R.S.E., The University, Glasgow.

VOLCANISM AND PLUTONISM.—The résumé of J. Verhoogen's paper, "Hypothèse sur les causes de l'activité volcanique" (*Ann. Soc. Géol. de Belgique*, LX, 1936, 29–37), is as follows (translation): "By adiabatic compression tectonic forces cause the accumulation of a considerable quantity of energy in gas pockets at high temperature. These energy accumulations explain the production of liquid basalt by isothermal fusion of the basaltic layer, and also the high temperatures characteristic of certain volcanic phenomena. The proposed hypothesis takes account of the relation between volcanism and diastrophism. Orogenic phenomena are accompanied or followed by volcanism only where such accumulations of energy probably exist."

In his paper "Zur Theorie und Klassifikation der eruptiven vulkanischen Vorgänge" R. A. Sonder (*Geol. Rundsch.*, XXVIII, 1937, 499–541) attempts to construct a classification of eruptive processes on the basis of physico-chemical principles. Differences in behaviour between basic and acid magmas depend chiefly on

viscosity, basic magma being richer in gases. Explosivity depends on the depth of origin of the magma, since at greater depths magma is able to dissolve more gas. Explosive phenomena result from rapid uprush and consequent diminution of pressure. The theory of tectonic activity resulting from retrograde boiling, as advocated by Rittmann, is rejected on the ground that the gaseous tension thus produced could not fulfil the energy requirements. A classification of eruptive activity on the basis of viscosity and explosivity is established.

In a paper on "Flood Basalts and Fissure Eruption," G. W. Tyrrell (*Bull. Volc., Naples*, Sér. II, **1**, 1937, 89–111) suggests that for the basaltic accumulations of enormous area and volume which have hitherto been designated "plateau basalts," the term "flood basalt" is more descriptive and appropriate. "Plateau basalt" merely refers to an accidental and inessential feature resulting from levelling-up, erosion, or earth movements. From a review of the world's flood basalts and of volcanic phenomena in Hawaii the conclusion is arrived at that flood basalts are due to the combined operation of shield volcanoes of Hawaiian type and fissure eruptions.

In describing "Flow-Units in Basalt" from New Mexico, R. L. Nichols (*Journ. Geol.*, XLIV, 1936, 617–30) demonstrates how multiple lava flows may be formed. A "flow-unit" is a tongue-shaped structure within a single flow produced by small lenses 100–300 ft. long and from 10 to 20 ft. thick, separated by thin slaggy crusts. These are believed to have been formed by successive ejections of the liquid interior of the flow along its front and lateral margins.

A. E. Jones deals with the "Formation of Basaltic Lava Flows" (*Journ. Geol.*, XLV, 1937, 872–80), and extends Nichols' observations to Hawaii and Mount Lassen. He also makes valuable suggestions regarding the surface structures of basaltic flows, and in regard to the sampling of lava flows in order to detect any petrological variations during eruption.

E. W. Skeats and A. V. G. James describe "Basaltic Barriers and other Surface Features of the Newer Basalts of Western Victoria" (*Proc. Roy. Soc. Vict.*, XLIV, Pt. II, 1937, 245–78). These remarkable features are ridges from 10 to 60 ft. high, which rise like waves or pressure ridges in ice from the surfaces of the basaltic lava flows, with intervening valleys and funnel-shaped depressions. Every barrier is more or less breached by troughs which may be more than 20 ft. in depth. The hypothesis put forward to explain these features is "partial collapse of the solid crust of a basaltic sheet due to the withdrawal of molten basalt from beneath as a

result of ruptures of the temporarily solidified front of the basalt sheet."

In his paper " Die Entstehung der Schildvulkane und der vulkanischen Tafelberge Islands," N. H. van Doorninck (*Proc. Kon. Akad. v. Wetensch. Amsterdam*, XXXVIII, 1935, 77–87) suggests that serious consideration should be given to Von Knebel's hypothesis that Icelandic shield volcanoes are due to single extrusions. H. Reck's view of the Icelandic volcanic table mountains as horsts is rejected until evidence of the supposed bounding faults is obtained. It is suggested that their isolation may be due to glacial erosion.

The same author also discusses the origin and mode of eruption of the 1783 outburst from the Laki fissure in Iceland (*ibid.*, 74–6).

In his paper " A Volcano under an Ice-cap: Vatnajökull, Iceland, 1934–36 " (*Geogr. Journ.*, XC, 1937, 6–23), and in a beautifully illustrated quarto monograph (*Vatnajökull: Kampen mellem Ild og Is.* Copenhagen: H. Hagerup, 1937, 124 pp.), N. Nielsen describes the effects of recent volcanic eruptions under the Vatnajökull ice sheet. The phenomena of this eruptive type were briefly described in SCIENCE PROGRESS, XXXI, 1936, 336. A quotation will serve to illustrate Dr. Nielsen's results: " It is quite clear that these two eruptions, the Grimsvötn eruption and that west of Hágöngur, (were) very different in energy. The Grimsvötn eruption had the strength to melt 10 cub. km. of ice and after that to penetrate the ice cover and send a column of about 10 km. height into the air for several weeks; and the Hágöngur eruption could not even penetrate a thin ice-cap of about 100–200 m. . . . the Grimsvötn eruption seems to be poor in ash material and enormously energetic, while the Hágöngur eruption probably produced a considerable amount of lava and very little energy."

T. F. W. Barth describes volcanic ash from a subglacial volcano in Vatnajökull 1922 and 1934 (*Norsk Geol. Tidsskr.*, **17**, 1937, 31–8), which proves to be a brown glass of " plateau basalt " composition, or sideromelane (Peacock). It is thus a modern analogue of the Pleistocene sideromelanes which in Iceland are believed to be products of drastic chilling of basaltic magma extruded under the ice sheet.

In a short résumé of " Thermal Activity in Iceland " the same author (*ibid.*, **16**, 1936, publ. 1937, reprint 4 pp.) classifies the hot springs as, (1) fumaroles (steam vents); (2) solfataras (acid springs depositing sulphur); and (3) alkaline springs, generally depositing siliceous sinter. All Icelandic springs are strongly influenced by earthquakes and volcanic eruptions. Definite relations have been shown to exist between the various types of springs and the adjacent

rocks ; for example, the alkaline springs show a pronounced preference for the Palagonite Formation.

The origin of the Icelandic Palagonite Formation is discussed by N. Nielsen and A. Noe-Nygaard (*Geogr. Tidsskr. Copenhagen*, **39**, 1936, 36 pp.). It is shown that the formation is partly sedimentary and partly igneous, and that the sedimentary facies shows signs of the action of subaerial agents such as wind, weathering and solifluction. Eruptions beneath ice sheets give rise to pillow lavas and fragmental glassy accumulations. The Palagonite Formation thus includes both subglacial formations (tillites and pillow lavas) and subaerial formations ; but many of the sediments were deposited subaerially on ice-free land surfaces, and are essentially Interglacial formations.

The great Ægean volcano of Santorin and its eruptions from 1925 to 1928 is the subject of a monograph by H. Reck *et alia* (*Santorin : Der Werdegang eines Inselvulkans und sein Ausbruch 1925–1928.* Berlin : D. Reimer, 1936, 3 vols. quarto, 606 pp., 109 text figures, 49 plates, and one profile 25 ft. long.). A full review by A. Knopf is given in the *Amer. Journ. Sci.*, XXXIV, 1937, 161–3.

J. E. Richey discusses " Some Features of Tertiary Volcanicity in Scotland and Ireland " (*Bull. Volc., Naples*, Sér. II, **1**, 1937, 13–34). He deals particularly with the history of the basalt caldera of Mull, which is illustrated by some illuminating diagrams, and with the post-basaltic volcanoes of Ardnamurchan and Slieve Gullion. He concludes with a discussion of calderas essentially due to explosion.

A. G. MacGregor briefly reviews the main facts relating to Scottish Carboniferous and Permian volcanicity (*ibid.*, 41–58). These volcanic regions are included within the graben of the Midland Valley of Scotland.

In a paper on " Tuffs and other Volcanic Deposits of Katmai and Yellowstone Park," C. N. Fenner (*Trans. Amer. Geophys. Union, 18th Ann. Meeting*, 1937, 236–9) calls attention to the special features of the great rhyolite-tuff " sand-flow " of the Valley of Ten Thousand Smokes, Katmai, Alaska. Its rapid and firm consolidation is ascribed to the action of self-evolved gases shortly after emplacement. Similar deposits are found in the Yellowstone Park ; but another type of fragmental volcanic deposit consisting of horizontal beds of rounded boulders, which is developed in the Absaroka Mountains, is believed to be due to volcanic landslides aided perhaps by floods from burst crater lakes.

Writing on " The Structure of the Rhyolites in Yellowstone

Park," H. A. Brouwer (*Journ. Geol.*, XLIV, 1936, 940–9) states that much of the banding and lamination of the lavas is to be considered not as the result of the spreading of a non-homogeneous magma, but as the result of crystallisation along planes developed by movements after the flow structure was completed.

The well-known Mono Craters in eastern California are shown by E. B. Mayo, L. C. Conant and J. R. Chelikowsky (*Amer. Journ. Sci.*, **32**, 1936, 81–97) to continue southward as a chain of six widely-spaced rhyolite domes, the morphology of which indicates that upheaval of the highly viscous lava was preceded and accompanied by violent explosions.

W. D. Smith and C. R. Swartzlow discuss the question whether Mount Mazama, the Cascade volcano in which Crater Lake has been formed, is due to explosion or collapse (*Bull. Geol. Soc. Amer.*, **47**, 1936, 1809–30). The distribution, character and known quantity of the volcanic ejecta, the shape and character of the materials of the crater, and the non-existence of recent lavas, lead to the conclusion that explosion is the most acceptable explanation of Mount Mazama's peculiar features.

The validity of the original explanation of the Tertiary (Lower Keechelus) breccias of the Cascade Range as of pyroclastic origin has been questioned by G. E. Goodspeed and H. A. Coombs (*Amer. Journ. Sci.*, XXXIV, 1937, 12–23). They offer the alternative view of recrystallisation-replacement, and believe that a sandy shale has been changed into a pseudo-igneous rock by a process of additive hydrothermal metamorphism.

Seventy-one occurrences of small dike-like masses of igneous rock and explosion-tubes (diatremes) have now been found by G. W. Rust (*Journ. Geol.*, XLV, 1937, 48–75) within an area of 75 sq. miles in south-eastern Missouri, rendering this district comparable with regions of a similar type of volcanicity in Swabia and Scotland. These igneous exposures are believed to represent small bore-holes drilled by gaseous explosion above a postulated magma chamber of laccolithic type.

A. G. MacGregor gives a preliminary account of the geology of Montserrat based on work accomplished during the recent Royal Society expedition (*Proc. Roy. Soc. London*, B., **121**, 1936, 232–52). The whole of the island is volcanic. Its localised and recurrent *soufrière* and seismic activity are related phenomena of volcanic origin connected in some way with molten or semi-molten magma beneath the Soufrière Hills, the most recently active volcano. The eruptions of Soufrière were of the *nuée ardente* type, and took place during Pleistocene or Recent times.

In a paper on "Vulkanische Asche vom Ausbruch des chilenischen Vulkans Quizapú (1932) in Argentina gesammelt," W. Larsson (*Bull. Geol. Inst. Upsala*, XXVI, 1936, 27–52) describes what he calls the aeolian differentiation of the ash with distance from the point of eruption. The ash seems to have been of andesitic composition, but at increasing distances from the volcano it becomes progressively more rhyolitic, owing to the light acid residual glass being carried farther by the wind than the crystalline constituents.

In his paper on the "Origin and Movements of Magmas in a Strong Earth," J. S. de Lury (*Amer. Journ. Sci.*, XXXIV, 1937, 222–34) dissents from current views that a continuous shell of weakness exists beneath the earth's crust, and that magma is under hydrostatic or isostatic control and a passive participant in earth movements. The conclusion is reached that magma is formed in horizontal sheets within environments that decree its forceful migration, growth and eventual intrusion into belts of deformation.

Continuing his study of magma and magmatic environments the same writer, in a paper on "Heterogeneity of Parent Magma" (*Journ. Geol.*, XLV, 1937, 381–90), ascribes only a minor and local part to processes of differentiation in producing the diversities of igneous rocks. Using thermal evidence, he derives the view that most magma is generated by frictional heat during its wide and forceful migration and, thus dependent on its compositional environment, is more or less heterogeneous from birth.

Starting with the assumptions of his "undation theory" (see SCIENCE PROGRESS, XXX, 1936, 491), R. W. van Bemmelen deals with "The Cause and Mechanism of Igneous Intrusion; with Some Scottish Examples" (*Trans. Glasgow Geol. Soc.*, XIX, Pt. III, 1937, 453–92). He distinguishes between positive hydrostatic pressure in a magmatic column due to differentiation, leading to doming of the crust, thrusting and magmatic injection, and negative hydrostatic pressure which causes stoping and the drawing-up of magma into potential vacua within the crust. The phenomena of sills are ascribed to this latter mode of action. The escape of volatiles by retrograde boiling is believed to play an important part in the ascent of magma only at higher magmatic levels and during volcanic outbursts. Van Bemmelen's views are illustrated by reference to certain Scottish asthenoliths, batholiths and Tertiary igneous complexes.

In his paper on "The Volcano-tectonic Structure of the Residency of Malang (Eastern Java)—An Interpretation of the Structure of the Tengger Mountains," R. W. van Bemmelen (*De Ingenieur in Nederlandsch-Indie. IV. Mijnb. en Geol.* "*De Mijn-ingenieur*"

Jaarg. IV, No. 4, 1937, 159–72) shows that the Tengger volcanic group consists of olivine-basalt and andesite lavas erupted in the order stated. The interpretation of the structure is based on the theory of gravitational tectogenesis. The northern part of the mountains is believed to have been broken off along large crescentic faults, and to have collapsed into the geosynclinal depression to the north.

In " A Contribution to the Mechanics of Intrusions," F. Loewinson-Lessing (*Report XVIth Internat. Geol. Congr., Washington, 1933*, 1935, Preprint, pp. 14) concludes that the displacement of magma is caused by external dislocation pressure. Most batholiths are believed to be sheet-like and not bottomless. In so far as they are sheet-like, the emplacement of sills and batholiths is due to a process resulting from a " mobile equilibrium of masses." The mechanics of the process is plastic hydrostatic sinking of the base of the intrusion chamber, and " hydrodynamic exchange of place by rock-flowage between the country-rock and the magma."

In his important paper on " The Dynamics of the Formation of Cone-sheets, Ring-dykes and Caldron-subsidences," E. M. Anderson (*Proc. Roy. Soc. Edin.*, LVI, Pt. II, 1936, 128–57) mathematically and geologically works out the implications of his previously published theory of the origin of igneous ring-structures. H. Jeffreys contributes an elucidatory and critical " Note on Fracture " (*ibid.*, 158–63).

In P. H. Kuenen's paper " Intrusion of Cone-sheets " (*Geol. Mag.*, LXXIV, 1937, 177–83), based on observations in Mull and Ardnamurchan, evidence is adduced to show that the injection of cone-sheets is accompanied by vertical lifting of the upper wall, thus confirming E. M. Anderson's explanation of the phenomenon. It is also argued that the fracturing of the country-rocks with the concomitant injection of cone-sheets are effects which take place with great rapidity.

A discussion of the " Sierra Nevada Pluton and Crustal Movement," by E. B. Mayo (*Journ. Geol.*, XLV, 1937, 169–92), serves to show that while part of the magma was guided by regional N.W.–S.E. trends of cleavage and folding, the primary controls were oriented nearly N.–S., as zones of tension in this direction guided the Tertiary and Quaternary volcanic eruptions as well as some of the older intrusions. These zones occur in echelon, and are thought to indicate horizontal drift by which the Sierra Nevada block has been shifted northward relative to more eastern blocks. The geologically young uplifts and plutonism of the region may indicate the emplacement at depth of another pluton.

A paper by H. Gerth, on "Die Bedeutung des Magmas in der Orogenese der südamerikanischen Kordillere" (*Geol. Rundsch.*, XXVII, 1936, 87–9), ascribes the uplift of blocks of Pre-Andean structures, with concomitant folding of the sedimentary cover, to magmatic movements in depth. The meridional parts of the South American cordilleras are thus regarded as "magmatic mountains."

F. F. Grout discusses the "Structural Features of the Saganaga Granite of Minnesota-Ontario" (*Report XVIth Internat. Geol. Cong., Washington, 1933*, 1936, 255–70). The internal structure of the batholith and the drag on its walls indicate a lengthening of the mass during crystallisation in an upward and westerly direction. The deepest parts of the mass are root-like extensions which may be "feeders." These extensions deform and confuse the structures of the ancient gneisses they penetrate, and show little sign of having derived their materials from the adjacent country-rocks.

In his paper "Gravity Stratification as a Criterion in the Interpretation of the Structure of Certain Intrusives of the North-western Adirondacks," A. F. Buddington (*Report XVIth Internat. Geol. Congr., Washington, 1933*, Preprint, Dec. 1935, 1–6) discusses several Pre-Cambrian igneous complexes hitherto interpreted as batholithic or stock-like intrusions. As good indications of gravity stratification are present, several of these masses are tentatively interpreted by Buddington as isoclinally folded sheets.

L. R. Wager and W. A. Deer (*Geol. Mag.*, LXXV, 1938, 39–46) describe an immense Tertiary dolerite dike-swarm which closely follows the eastern coast of Greenland for 500 miles. The dikes cut the Tertiary basalt series and the underlying metamorphic complex. The swarm is densest at the coast and thins inland. It follows the line of a well-defined monoclinal flexure in the basalt lavas, which was formed during the later stages of the Tertiary igneous episode. Tension developed in the convex part of the flexure produced successive fractures which were occupied by the basaltic magma underlying the whole region.

According to J. E. Hibsch's work, "Über die vulkanischen Gangspalten im Böhmischen Mittelgebirge" (*Neues Jahrb. f. Min.*, B.-B. 70, Abt. A., 1936, 571–7), the very numerous dikes of the Bohemian Mittelgebirge are arranged as a very perfect radial swarm centring on the essexite intrusions of Rongstock.

PEDOLOGY. By Professor N. M. Comber, D.Sc., A.R.C.S., F.I.C., The University, Leeds.

Soil Surveys.—It was mentioned last year that Kellogg of the United States Soil Survey had produced a concise account of our

present knowledge of soil genetics and classification. Kellogg (*United States Department of Agriculture*, Miscellaneous Publication No. 274) has followed this with a "Soil Survey Manual." This gives a detailed account of the technique of sampling soils, of all field examinations and of the methods of soil mapping.

With the development of soil survey work there have arisen a number of suggested rapid tests including colour tests for potassium, phosphorus, various forms of nitrogen and other elements. A typical illustration is "The Universal Soil Testing System," by M. F. Morgan (*Connecticut Agric. Expl. Station*, Bull. 392, 1937). In this system the soil is extracted with a highly buffered solution of sodium acetate and acetic acid and the tests carried out on portions of the extract. Many of the tests and the expediency of the system of testing have been critically considered (for references see *Imperial Bureau of Soil Science*, Monthly letter No. 71). It appears that there is a real amount of usefulness in these tests but that considerable experience and previous knowledge of the soils concerned are necessary in the interpretation of the results.

Soil Genetics and Classification.—The development of the study of soil genetics and the general adoption of the genetic basis for classification has led in the last year or two to some difficult problems of confused terminology and of interpretation of reports of soil grouping. Some fundamental questions concerning the genetic classification have therefore been raised for reconsideration. In a discussion at the British Empire Section of the International Soil Society, Blackpool, 1936, Dr. W. G. Ogg has raised and examined the questions: (i) Is a broad world grouping of soils desirable? (ii) Are the groups as at present defined satisfactory? (iii) Can British soils fit into the existing groups? No pedologist is likely to question the desirability of a world classification. But it is admitted in all quarters that the present classification is not adequate for universal application and that British soils cannot all be fitted into it. A great deal more field and laboratory work must be done before a universally adequate grouping can be made, and because of this advances in the study of soil genetics in the immediate future are not likely to be rapid.

The Brown Earths are a group which figures prominently in the European literature and about the definition and meaning of which there is great confusion. Some attempts have been made to mitigate this confusion and an interesting paper comes from Mr. G. R. Clarke (*British Empire Section of the International Soil Society*, Oxford, 1937). The term "Brown Earth" originated in the writings of Ramaan in Germany a good many years ago. In subsequent

writings it has been used in a vaguer way than that in which Ramaan used it, and moreover the term Brown Forest Soil has come into the literature without any agreement as to whether it is virtually synonymous with Brown Earth or essentially different. Mr. Clarke makes interesting suggestions for reviving Ramaan's conception of a Brown Earth and for defining a Brown Forest Soil as different from but related to it. The Brown Earths of Ramaan had formerly been under broad-leaved forest which continued as forest by natural processes of regeneration until human intervention in one way or another stopped this regeneration. Such soils under forest can be termed Brown Forest Soils.

In these Brown Forest Soils the water level will be lowered by the transpiration of the forest trees, and the leaf-fall will reduce evaporation, thus maintaining a permanently moist A horizon. Between this surface moisture and the lowered ground water level will be a drier B horizon.

Human intervention may remove the forest conditions and bring the surface into cultivation. The absence of forest trees then causes the ground water level to rise. The cessation of accumulating leaf litter on the surface enables a greater percolation of water at times of rainfall, with consequent increase in mineral decomposition, and a drying out of the surface at other times. There are no longer two permanently moist horizons separated by a drier one and the surface becomes dominantly mineral and not humic. The soils thus produced from the Brown Forest Soils by deforestation and cultivation are the Brown Earths as Ramaan understood them and as they may now be usefully redefined.

Desert Soils.—The genetics of the American desert soils have been investigated by Nikiforoff (*Soil Science*, 1937, **43**, 105). The characteristic of these soils is a hard cemented crust or hardpan generally overlaid by loose and biologically inert material. In addition, and usually beneath the crust, is a clay layer. Earlier theories had associated the crust and the clay layer as genetically related. The author concludes that they are the results of independent processes, that an upward water movement brings about an accumulation of salts and the formation of the crust, and that the hydrolysis of minerals *in situ* is responsible for the clay layer.

Soil Micro-organisms.—Waksman and others (*Soil Science*, 1937, **43**, 51, 69, 77) have carried out investigations into certain antagonistic and collaborative effects of micro-organisms. Reviewing the literature makes it clear that a vast number of micro-organisms have injurious effects upon themselves and upon other organisms. This may be due to a variety of causes, competition for

nutrients, alterations of reaction and of oxidation-reduction potential, or the formation of toxic substances. A study by the authors of the effects of a particular species of actinomyces showed that its toxic effect on other organisms was specific, and not due to changes of reaction or other environmental factors.

The complexity of the interrelations of the soil organisms is such that it is difficult to base conclusions on the effect of one organism on another in isolated cultures. The alleged effect of the soil protozoa on some processes by reason of their effect on bacteria is thought to be exaggerated.

It was found that a cellulose decomposing fungus attacks the proteins and not the cellulose of lucerne, but in the presence of certain organisms which could only attack protein it attacked the cellulose. It was found, too, that certain non-cellulose-decomposing organisms accelerate the decomposition of cellulose by other organisms.

An instance of the influence of associations of organisms is found in the fact that certain actinomyces will not attack lucerne in pure culture, but do so vigorously when accompanied by certain fungi. Similar associative influences affect the loss of nitrogen from soil organic matter.

Chlorosis.—The inability of soils to provide adequate supplies of iron is the underlying view of the cause of chlorosis. It has long been clear, however, that there is no simple relationship between the reaction of the soil, as the factor that has been held to affect the solubility and mobility of iron, and the incidence of chlorosis. From the work of Wadleigh and others (*Soil Science*, 1937, **43**, 153) it appears that the attempt to associate the mobility of the iron with pH ignores other factors, particularly the phosphate concentration. It also appears that nitrate concentration is in some way associated with chlorosis, and that plants supplied with nitrogen in the form of ammonium salts are relatively free from chlorosis when compared with plants supplied with nitrate.

Nitrification.—It has been claimed from time to time lately that the biological oxidation of nitrogen compounds to nitrate in soils has been exaggerated and that sunlight has important photochemical effects. This has been denied as often as it has been asserted. The matter has been subjected to very careful investigation by Waksman and others (*Soil Science*, 1937, **44**, 361, 441). They have failed to find any significant increase in the oxidation of the soil's own nitrogen by exposure to sunlight, but in certain experiments added ammonium compounds were oxidised. They conclude that biological oxidation is still to be regarded as the outstanding cause of nitrate formation in soils.

Artificial irradiation with ultra-violet light had a destructive effect on certain organisms, including the nitrifying bacteria, and no photochemical formation of nitrate was found.

BOTANY. By Professor E. J. Salisbury, D.Sc., F.R.S., University College, London.

The changes exhibited by heavy clay saline soils in Holland with the passage of time forms the subject of a paper by D. J. Hissink (*Soil Science*, **45**, 83–94, 1938). Data are furnished respecting the Dollard polders, which range in age from 6 to 380 years. They show a progressive leaching of the calcium carbonates which van Bemmelen described in 1863 as being washed out at the rate of about 1 per cent. in a quarter of a century. Actually the data here given indicate a period of some 270 years as requisite for complete removal of the 9·5 per cent. calcium carbonate in the young polder, but, since the rate of removal is at first rapid and then diminishes, the middle part of the curve does not depart materially from van Bemmelen's figure. Despite this loss of calcium, however, the total exchangeable bases remain practically constant in amount. There is a loss of exchangeable calcium, but in the older polders this is apparently compensated by an increase of exchangeable magnesium which, it is suggested, diffuses out from the interior to the surface of the soil particles.

The humus content shows a slight initial decrease and then remains almost constant at about 3·5 per cent. The reaction, which is at first markedly alkaline (7·8), becomes slightly less alkaline with age, but it is only in the oldest polder, reclaimed for 380 years, that an acid reaction (*p*H 5·9) is realised. The exchangeable potassium would appear to decrease during the first 200 years and then to approximate to a constant low value of about 0·6 milligram equivalents per 100 gm.

The disease of sheep known as the coast disease is, like sheep sickness, apparently due to the absence of the requisite micronutrients in the herbage on which they browse, but, whereas " sheep sickness " is apparently due to the absence of minute traces of cobalt alone, the coast disease, which is confined to highly calcareous dune soils, is apparently due to the absence of the requisite traces of both cobalt and ccpper, since food containing both these metals restores the animals to complete health. It would be interesting to know whether the plants of these areas show structural or morphological features associated with these soil deficiencies (*Bull. Council Sci. Indust. Res.*, No. 113, 1938).

W. H. Pearsall furnishes some interesting data regarding wood-

land communities in the North of England, particularly in respect to the oxidation-reduction potential of their soils. The oxidising soils devoid of nitrates and deficient in bases, with a *p*H below 3·8, are characterised by communities of *Betula*, *Vaccinium myrtillus*, *Deschampsia flexuosa* and *Dicranum majus*. Those oxidising soils between *p*H 3·8 and 5·0, which, though deficient in bases, contain nitrates, are usually characterised by oakwoods with *Holcus mollis* and *Millium effusum*. Oxidising woodland soils above about *p*H 5, with nitrates and either calcareous or slightly deficient in bases, yielded *Mercurialis perennis* and *Brachypodium sylvaticum*. The soils of alder woods were found to be oxidising in summer, but tend to be reducing in winter. These have a *p*H above 3·8 and during summer contain nitrates. *Phalaris arundinacea* and *Ulmaria palustris* are suggested as probably characteristic of such conditions. The wet soils of the *Salix cinerea* woods were found to be reducing with a *p*H value above 5 (*p*H 5·1–*p*H 5·5) and an absence of nitrates.

There are thus two types of soils deficient in nitrates, the one a reducing type of soil tending towards a *p*H above 5, and the other the oxidising soil with a *p*H below 3·8 (*Jour. Ecology*, XXVI, 180–209).

A paper in the same journal by J. F. H. Simpson describes the flora of calcareous greensand and compares it with that of the non-calcareous greensand. From this the conclusion is reached that the majority of so-called calcicoles are restricted to calcareous soils owing to chemical rather than physical conditions. The following six species in particular are cited as chemically determined "calcicoles," *viz. Avena pubescens*, *Bromus erectus*, *Helianthemum vulgare*, *Leontodon hispidus*, *Poterium sanguisorba* and *Scabiosa columbaria*.

Physically determined calcifuges are held to include *Erodium cicutarium*, *Myosotis collina*, *Plantago coronopus*, *Potentilla argentea*, *Pteridium aquilinum*, *Saxifraga granulata*, *S. tridactylites*, *Trifolium arvense*. Although classed as "calcicoles," it is admitted that these species are tolerant of free calcium carbonate if the soil be sandy, but it is nevertheless surprising to find *Saxifraga tridactylites* included as a "calcifuge," since it is an abundant constituent of the vegetation of some calcareous soils and occurs on the mortar joints of old walls.

Miss P. M. Jenkin (*Jour. Marine Biol. Assn.*, XXIII, 301–43, 1937), from a study of the oxygen production by the diatom *Coscinodiscus excentricus*, concludes that the compensation point in the clear open water of the English Channel in summer is attained at a depth of about 45 metres. This corresponds to about 0·13 gm. calorie per sq. cm. per hr. By comparison of the energy available

at varying depths and determining the oxygen production the conclusion was reached that diatoms can utilise energy for photosynthesis equally well in all parts of the visible spectrum.

In a paper by S. C. Varma (*Ann. Bot.*, N.S., II, 205–25, 1938) the nature of competition between species and strains in the early stages of development is considered. It is shown that in some instances the severity of competition, as measured by mortality, is greatest in mixed cultures, whereas in other combinations of species or strains the severity of competition is greater between the individuals of the same kind than between individuals of different kinds. Thus Darwin's generalisation is not universally true. Experimental evidence is afforded that the effect of one strain on another is in part at least due to soluble toxic substances.

From a study of the length of viability of seeds of *Populus*, E. H. Moss concludes that under natural conditions this is of two to four weeks' duration and is markedly influenced by atmospheric humidity. The most prolonged viability appears to be attained at a relative humidity of about 10 per cent., but with moister conditions the seeds rapidly lose their capacity to germinate. Natural reproduction appears to depend upon the soil remaining moist throughout the first week of growth (*Bot. Gaz.*, **99**, 528–42, 1938).

The seedling reproduction of the chestnut oak (*Quercus montana*) is the subject of a paper by O. M. Wood in the April number of *Ecology* (**19**, No. 2, 276–93, 1938). Considerable losses are due to destruction of acorns upon the tree, but evidence was obtained that destruction by animals was far greater amongst the fallen acorns, of which it was estimated that 87 per cent., of 1080 fruits originally upon the ground, were destroyed in this manner. Germination may be as high as 96 per cent., but great mortality from a variety of causes occurs amongst the seedlings, so that at the present time the reproduction is inadequate. Experiments showed that oak seedlings grew better in lightly limed soil with a *p*H of between 5 and 6 than either in the unlimed soil with a markedly acid reaction of about *p*H 4 or in more heavily limed soil with a reaction of between *p*H 6 and *p*H 7.

In the same journal (pp. 188–207) O. B. Stanley discusses the indicator value of various types of ground vegetation in stands of *Pinus strobus* in which six are recognised. The less productive are characterised by *Cladonia-Andropogon* or *Vaccinium-Gaultheria*, the better stands by *Maianthemum* type, *Cornus-Lycopodium* type, *Aspidium-Dicksonia* type or *Mitchella-Pteris* type.

V. D. Zotov (*N.Z. Jour. Sci. Tech.*, pp. 474–87, 1938) suggests the use of the mean midsummer month temperature as a basis for

defining the climatic vegetation belts of New Zealand. On the Tarurnas there is, it is stated, no correlation between the timber line and duration of the snow-free period, but the vegetation belts show marked correlation with the January isotherm.

From a consideration of the Dicotyledonous plants which exhibit monocotyledonous seedlings A. W. Hill (*Ann. Bot.*, N.S., II, 127–243, 1938) concludes that this condition is the outcome of suppression, as in Cyclamen, and not of fusion as claimed by Sargant. *Ficaria verna* sometimes has two cotyledons, whilst the anatomy of the single cotyledon of *Bunium* corresponds with that of one of the paired cotyledons of the allied *Anethum*. In this paper the author considers the seedlings of various genera of Gesneraceæ. Some of these possess but a single cotyledon, which persists as the single leaf throughout the life of the plant. This is held to be an enlargement of the meristematic region at the base of the cotyledon, which may appear as a glabrous tip more or less distinguishable from the hairy lamina interpolated behind. This interpretation would indicate a morphological equivalence to the normal phyllodic monocotyledonous leaf. Its pinnate venation thus has a particular significance, since we might expect to find a parallel veining as in other petiolar structures.

The range in variation of floral and inflorescence morphology exhibited by the members of the Betulaceæ is the subject of a paper by E. C. Abbe (*Bot. Gaz.*, **99**, 431–69, 1938). The variations recorded include the presence of the median flower in the normally biflorous cyme constituting the partial female inflorescence of *Alnus* and *Carpinus*; the presence of tricarpellate pistils in *Alnus*, *Betula* and *Ostryopsis*; the occurrence of hexamerous male flowers in *Alnus* and of hermaphrodite flowers in various genera.

The morphology and phylogeny of the perianth is the subject of a paper by J. Mattfeld (*Sonderber. d. Deutsch. Bot. Gesell.*, LVI, 2, 86–116), in which the view that the perianth has had diverse origins is upheld. It may be of staminal origin, derived from bracts, or be partly sporophyllous in origin, partly derived from leaf structures.

The differentiation of fruit buds of apples in Australia appears to begin in late December or early January, and the time does not seem to be appreciably affected by climatic conditions. On the other hand, the degree of cropping has a definite effect on the season of differentiation, heavy cropping being associated with an earlier cessation of shoot growth and earlier fruit-bud differentiation (*Bull. C.S.I.R.*, 113, 1938).

PLANT PHYSIOLOGY. By PROFESSOR WALTER STILES, Sc.D., F.R.S., The University, Birmingham.

PERMEABILITY AND RELATED PHENOMENA.—The complex relationship between the concentration of ions within and without plant cells, shown by Stiles and Kidd for storage tissues as long ago as 1919, has been confirmed and examined by a number of subsequent workers and the phenomenon shown to be exhibited by plant cells and tissues of very diverse kinds. Recently D. R. Hoagland and T. C. Broyer ("General Nature of the Process of Salt Accumulation by Roots with Description of Experimental Methods," *Plant Physiology*, **11**, 471–507, 1936) have shown that under certain experimental conditions potassium salts accumulate very rapidly in the sap of excised roots of barley. This accumulation only takes place in presence of an adequate supply of oxygen. The authors point out that under anaerobic conditions there is no accumulation of ions although there is a production of carbon dioxide, from which they conclude that the mere production of carbon dioxide does not account for salt accumulation. Other conditions which must be carefully controlled in experiments on salt accumulation include age of the roots and proportion of active cells, the initial salt content of the roots and the available carbohydrate.

Two interesting papers bearing on the salt relations of tissues have been published by G. F. Asprey. In the first of these ("On the Relationship between Exosmosis and Salt Absorption by Potato Tuber Tissue previously treated with Various Salt Solutions," *Protoplasma*, **24**, 497–504, 1935) it is shown that pre-treatment of potato tuber tissue with chlorides of the alkali metals, sodium, potassium and lithium, brings about an increase in the subsequent absorption of ammonium by the tissue and also in the exosmosis of electrolytes into distilled water. Pre-treatment with calcium chloride, on the other hand, induces a decrease, both in the absorption of ammonium and in the exosmosis of electrolytes into distilled water. Pre-treatment of the tissue with aluminium chloride brings about a decrease in the subsequent intake of ammonium, but an increase in exosmosis into distilled water, but, as the author points out, this may be related to the partial hydrolysis of the salt producing an acid solution. The author interprets his results as due rather to alterations in the permeability of the tissue than to any quantitative relationship between the two processes of absorption and exosmosis.

In the second paper Asprey deals with the effect of different periods of immersion of potato tuber and artichoke tuber tissue in running tap water on the subsequent absorption of ammonium chloride and on exosmosis of electrolytes ("Some Observations on

the Absorption and Exosmosis of Electrolytes by Storage Organs with particular reference to Potato and Artichoke Tubers," *Protoplasma*, **27**, 153–68, 1937). The general course of exosmosis by potato tuber into distilled water was shown to be similar to that observed by Stiles in 1927 for various tissues and by Briggs in 1931 for carrot root, and the exosmosis took the same general course for periods of washing varying from 24 to 119 hours, although the maximum amount of exosmosis was lower, and reached sooner, the longer this period. The absorption of the ions of ammonium chloride by potato tuber was found to be increased by previous washing in running tap water, the longer the washing, up to 125 hours, the greater the absorption of both ions. This was not the case with artichoke tissue, in which neither the subsequent absorption of ammonium and chloride ions, nor exosmosis into distilled water, was significantly affected by the length of preliminary washing in tap water. Although suggestions are made to account for the differences in behaviour of the two tissues, the reasons for it remain obscure.

S. C. Brooks has attempted to examine the parts played by protoplasm and vacuole respectively in the accumulation of ions by *Valonia ventricosa* ("The Accumulation of Ions: Relations between Protoplasm and Sap in *Valonia*," *Journ. Cell. and Comp. Physiol.*, **6**, 169–80, 1935). After the cœnocytes had been immersed for various periods of time in sea water to which a certain amount of isotonic rubidium chloride had been added, the cell sap was separated from the protoplasm and cell wall and the rubidium estimated in each of these fractions as well as in the external solution at the end of the experiment. Although, owing to difficulties of analysis, exact determinations of rubidium in what may be called the protoplasm, failed, the results indicated that rubidium accumulated rapidly in the protoplasm during the first two days, the protoplasmic accumulation ratio being, in these experiments, of the order of 500 or 1000, whereas in the sap it was about 39. After this time rubidium passed from the protoplasm to both sap and external solution, but owing to the more rapid increase in the rubidium concentration in the latter the accumulation ratio of the sap fell progressively to the twentieth day.

De Haan has examined the effect of various ions on the permeability to water of the epidermal cells of the bulb scales of *Allium cepa* (I. de Haan, "Ionenwirkung und Wasserpermeabilität. Ein Beitrag zur Koazervattheorie der Plasmagrenzschichten," *Protoplasma*, **24**, 186–97, 1935). The method employed consisted in plasmolysing the cells in a pure sucrose solution of concentration

0·646M, and in a solution of the same osmotic strength containing sucrose to which a quantity of salt was added ; the latter constituted the experimental series of experiments, the former the control. After plasmolysis the control cells were deplasmolysed in a pure sucrose solution of concentration 0·2M, while the experimental cells were deplasmolysed in a solution of this same osmotic concentration, but containing sucrose and the same concentration of salt as the plasmolyte. The rate of deplasmolysis was then regarded as giving a measure of the permeability of the protoplasm to water.

The salts employed were sodium nitrate, calcium nitrate and cobalt ammonium chloride, each in different concentrations. It was found that sodium nitrate increased the rate of deplasmolysis and so the permeability of the protoplasm, the increase being greater the more concentrated the sodium nitrate in the solution. With the other salts used there was a reduction in permeability with low concentrations of salts, but an increase with higher concentrations.

The effect of carbon dioxide on the exosmosis of electrolytes from the stems of seedlings of *Lupinus albus* has been examined by H. Kaho (" Über den Einfluss der Kohlensäure auf die Exosmose von Elektrolyten auf Stengelzellen," *Protoplasma*, **27**, 502–22, 1937). Pieces of the stem were divided into halves longitudinally and the exosmosis of electrolytes from each half into water was examined by following the change in electrical conductivity of the water. After some time the half from which the rate of exosmosis was less was then transferred to water containing carbon dioxide, while the other half was transferred to distilled water. After an hour both pieces were then re-transferred to fresh distilled water, which was renewed after every hour. The exosmosis of electrolytes was then always found to be greater from the tissue treated with carbon dioxide than from the control not so treated. Kaho regards these experiments as demonstrating that carbon dioxide brings about an increase in the permeability of the cells. While this is one possible explanation of the fact recorded, it seems clear that, in view of much recent work on salt intake by plant cells, other explanations are at least as likely. Kaho relates his findings with records on the alleged influence of light on permeability, and he points out that workers with chlorophyll-containing cells have recorded an effect of light in increasing the permeability of such cells, whereas workers with non-green cells have not been able to find any such influence. The latter observation was confirmed by Kaho himself for the case of chlorophyll-free stems of seedlings of *Lupinus albus* (" Über den Einfluss künstlicher Belichtung auf die Exosmose von Elektrolyten aus Stengelzellen," *Protoplasma*, **27**, 453–5, 1937).

The conclusion is drawn that when chlorophyll-containing cells are exposed to light an increase in permeability is easier to recognise because, owing to photosynthesis, the carbon dioxide diffusing into the protoplasm also induces an increase in permeability. When this latter factor is not present the effect of light in increasing permeability has not always been observed, because when an artificial source of light is employed the glass of the lamp cuts off ultra-violet radiation, which, by increasing the degree of hydration of the plasma colloids, is held to bring about an increase of permeability.

Kaho has also examined the effect of various ions on the permeability to water of the cells of the staminal hairs of *Tradescantia virginica* ("Ein Beitrag zur Kenntnis der Wasserpermeabilität des Protoplasmas," *Cytologia*, Fugii Jubilee Vol., 129–48, 1937). The method employed is quite simple in conception. The cells are plasmolysed in isotonic solutions of various salts, and when plasmolysis is complete, which takes from 5 to 7 minutes, the cells are transferred to tap water and the time required for deplasmolysis noted. The differences in these times are ascribed to different effects of the respective ions on the permeability of the cells to water. In general, the anions, in order of their accelerating influence on water absorption, can be arranged in the series CNS > I > Br, Acetate > NO_3,Cl > Citrate > Tartrate, SO_4. For kations Kaho could not give a single series applicable to all salts. Thus for alkali nitrates the order was found to be Rb > K,Na > Li, and for alkali sulphates K,NH_4 > Rb, Cs > Na,Li. In comparing his results with earlier ones on the rate of entry of different ions into the cell, Kaho concludes that, considering the salts of any one metal, the more rapidly a salt penetrates the protoplasm the more it accelerates the passage of water into the cell. On the other hand, as regards the effect of kations, it was found that although the bivalent ions Mg and Ca enter cells more slowly than the monovalent K and Na ions, the salts of the bivalent metals have a greater accelerating action on the passage of water into the cell than the corresponding alkali salts.

ENTOMOLOGY. By H. F. Barnes, M.A., Ph.D., Rothamsted Experimental Station, Harpenden.

General Entomology.—Recently attention has been drawn (*London Illustrated News*, January 1, 1938, 19–20) to the phenomenal speed of flight supposedly attained by *Cephenomyia pratti*, a deer botfly. It was stated that the males flew at an estimated speed of 818 m.p.h. and the females at 614 m.p.h. These statements were based on observations made by C. H. T. Townsend (*J. New York*

Ent. Soc., **35**, 1925, 245–52), who wrote, "On 12,000-ft. summits in New Mexico I have seen pass me at an incredible velocity what were certainly males of *Cephenomyia*. I could barely distinguish that something had passed—only a brownish blur in the air of about the right size for these flies and without sense of form. As closely as I can estimate their speed must have approximated 400 yds. per second." Irving Langmuir (*Science*, **87**, 1938, 233–4) has discussed this estimated speed. He worked out that the wind pressure against the fly going at 818 m.p.h. would be about 8 pounds per sq. inch or more than half an atmosphere. This force unopposed would give an acceleration of 500 times gravity if the fly's weight were 0·2 grams. The power consumption needed to maintain this velocity would be 370 watts or about one-half horse-power. Further, on the basis of man's thermodynamic efficiency, the insect would have to consume 1½ times its own weight each second to deliver 370 watts. Irving Langmuir, after describing experiments with a piece of solder on a thread, states that the appearance of the fly given by Townsend seems to correspond best with a speed in the neighbourhood of 25 m.p.h. The power requirements of a fly at 25 m.p.h. would involve a food consumption of about 5 per cent. of the fly's body weight per hour. It is obvious, therefore, from this discussion, that the speed of 818 m.p.h. is a ridiculous estimation and beyond all bounds of possibility. It is probable the fly travels at a speed of about 25 m.p.h.

The nocturnal activity of insects is an intriguing subject. O. Park recognises four categories of activity, all of which are specific. The environmental type appears to be controlled by the operating environmental influences and so has an apparent rhythm induced by the rhythmic day-night cycle. In the inherent type, activity is innately rhythmic and the basic periodicity less subject to destruction by alteration of the environmental influences. The third type is the arhythmic. In this the activity is evenly distributed over the 24-hour cycle. The last type is composite and is made up of at least two of the preceding types. In a recent paper (*J. Anim. Ecol.*, **6**, 1937, 239–53) this investigator shows that the beetle *Passalus cornutus* is arhythmic. He also has described a new recording apparatus based upon the audio-frequency principle.

R. Goldschmidt (*Amer. Nat.*, **71**, 1937, 508–14) has written an anticritique of certain statements in Kinsey's "The Origin of Higher Categories in Cynips" (Science Progress, XXXII, 1938, 542–3). He points out that the actual results so far as the taxonomic side is concerned are identical with his own work on *Lymantria*. He also claims that Kinsey's work does not throw light on the higher

categories, that the results of his studies do not go beyond what has been known in other groups, and lastly that no facts have been brought forward which will force geneticists to change their views.

Renewal of oxygen in the tracheæ of insects is generally assumed to take place by diffusion, aided in some cases, *e.g.* dragonflies, grasshoppers and wasps, by ventilatory movements of the body wall. The factor of oxygen diffusion through the skin has been neglected. G. Fraenkel and G. V. B. Herford (*J. Expt. Biol.*, **15**, 1938, 266–80) have carried out experiments on blowfly larvæ. They ligatured the larvæ behind the ganglion, so obtaining the respiration of the hind parts which represents the basal metabolism of the animal, and they also ligatured off the hind spiracles in addition. As a result of this double ligaturing, all respiration takes place by diffusion through the skin and was found to be about one-quarter of the basal value. There were indications that part of the metabolic processes are maintained by anaerobic respiration. The O_2 tension inside the larvæ was also measured. It was found that a much larger amount of oxygen diffuses through the skin in the double-ligatured than in normal larvæ. The same investigators found that when insects are submerged in water their spiracles are not able to function and all respiration takes place by diffusion through the skin. Similar results were obtained using other insects, *e.g. Chærocampa elpenor* larvæ (spiracles blocked with grease) and *Tenebrio* and *Culex* sp. larvæ (submerged in water).

The senses of insects have been compared to those of higher animals by N. E. McIndoo (*Proc. Ent. Soc. Wash.*, **40**, 1938, 25–35) in a most interesting and fully documented paper. His final conclusion is that man lives chiefly in a world of sights and sounds, while dogs and bees live mostly in worlds of odours.

The insect fauna of cultivated and wild grasses have been studied in recent years by F. Venturi. The third and fourth of his papers, one on *Agromyza mobilis* and the other on *Dizygomyza lateralis*, are now to hand (*Boll. Entom. Bologna*, **8**, 1935, 1–26, and **9**, 1936, 1–22). The insects, fungi and bacteria associated with copra are dealt with in Bulletin 20 of the Department of Agriculture, Straits Settlements and Federated Malay States (1937, 108 pp.).

A list of the many publications of the late W. M. Wheeler is to be found in a recent number of *Psyche* (**44**, 1937, 61–92). Naturally the vast majority of the papers are concerning ants, but others relate to other social insects, parasitism and evolutionary phenomena.

COLEOPTERA.—A study has been made by R. E. Campbell (*Ecology*, **18**, 1937, 479–89) of the temperature and moisture content of soil preferenda exhibited by wireworms. Their temperature

preferendum varies with the season, being higher in the summer and autumn as compared with that in the winter and spring. There were indications that the preferendum of the wireworms does not change until a month or more after a higher or low temperature has been administered to them. While both low and high temperatures are inimical, there is a wide range of temperature through which the wireworms are active. Dry soil stimulated the wireworms to move downwards to a more favourable environment.

LEPIDOPTERA.—The feeding mechanism of adult Lepidoptera, especially the means by which the proboscis is extended, has been studied by J. B. Schmitt (*Smithsonian Misc. Coll.*, **97**, 1938, 28 pp.). Blood pressure created in the stipes of each maxilla causes the coiled proboscis to extend. The pressure is caused by three pairs of muscles which by contracting press the stipes against the head wall.

A comprehensive account of *Cydia* (*Laspeyresia*) *funebrana*, the Red Plum Maggot, in Switzerland, has appeared by P. Bovey (*Rev. Path. Veg.*, **24**, 1937, 189–317). This includes sections on morphology, anatomy, biology, parasites, other insects attacking plum and control measures.

HEMIPTERA.—Instances of parental care among insects are not common, especially among the Homoptera. A. P. Beilmann (*Psyche*, **44**, 1937, 58–9) has described daily movements of young terrapin scale (*Eulecanium nigrofasciatum*) on hawthorn, followed by a return in the evening to the adult females under which they spent the night.

In 1936 Marion A. Watson suggested that starvation of aphids might have some effect on their efficiency as vectors of virus. Recently (*Proc. Roy. Soc. Lond.*, B., **125**, 1938, 144–70) this worker has shown that *Myzus persicæ* is much more likely to transmit Hy 3 virus if made to fast immediately before being fed on the source of infection. Its efficiency as a vector increased rapidly during the first hour of fasting, but further periods of fasting do not give equally large increases in infectivity. Discussing the results of starvation experiments in the light of modern ideas on the mechanism of virus transmission, the author concludes that the reduction in the infectivity of the virus during transmission by feeding insects may be caused by contact with trypsin.

HYMENOPTERA.—After an extensive study of the comparative morphology of the male genitalia in the Hymenoptera, O. Peck (*Canad. J. Res.*, D., **15**, 1937, 221–74) has come to the conclusion that in the Ichneumonidæ the male genitalia and adjacent sclerites present few if any tribal or subfamily characteristics of practical

value to the taxonomist since they are masked largely by specific variation.

Workers on insecticides are frequently searching for more suitable insects on which to test out the toxicity of their products. It is therefore of passing interest to note that honey bees have been used by F. C. Nelson (*J. New York Ent. Soc.*, **45**, 1937, 341–52) for testing liquid insecticides. Apparently, although there are some advantages, such as a steady available supply throughout the winter of fairly large insects of the same sex, of about the same age, and which are very sensitive to most nerve poisons, the disadvantages were sufficiently important to cause this insect to be replaced by others more suitable.

D. Powell (*Ann. Ent. Soc. Amer.*, **31**, 1938, 44–9) has described the biology of a Bethylid wasp (*Cephalonomia tarsalis*), which is parasitic on the beetle *Oryzaephilus surinamensis*. Fertilised females always deposit a male and a female egg on a single host, whereas virgin females only produce males.

W. A. Dreger (*Ecology*, **19**, 1938, 38–49), after studying the seasonal weight and total water content of *Formica exsectoides*, has shown that there is no seasonal variation in the total water content relative to temperature. Active and hibernating ants have the same total water content. Reduced water content is not a causal factor in the altered metabolism previously shown by the same worker (1932) to occur in this species during hibernation.

The chalcid, *Eupelmella vesicularis*, was found by K. R. S. Morris (*Parasitology*, **30**, 1938, 20–32) in certain parts of Hungary infesting cocoons of *Diprion setifer* already parasitised by another chalcid, *Microplectron fuscipennis*. Experiments showed that *Eupelmella* from these districts confines itself almost exclusively to *M. fuscipennis*. The stimulus which enables it to detect parasitised *Diprion* is at its maximum with the chalcid pupæ and zero with its eggs. One egg is laid per cocoon and the predatory larvæ, after feeding on 10 to 20 *Microplectron* larvæ or pupæ, kill all the remaining living parasites within the cocoon before pupating.

Joyce Laing (*J. Anim. Ecol.*, **6**, 1937, 298–317) has discussed how a parasite finds the area in which its host occurs and how the parasite finds the host when both are within the same limited area. The insects used in the experiments were *Trichogramma evanescens*, *Alysia manducator* and *Mormoniella vitripennis*. It is shown that the two latter parasites are attracted to an environment likely to contain their hosts by the qualities of the environment itself, independent of the presence of hosts. In a host-containing area *Trichogramma evanescens* can perceive its hosts by sight. As soon as it

has found a host a definite change in behaviour takes place. Instead of a straight or widely curving movement a spiral twisted path is taken. This naturally greatly increases the chances of the parasite running across its host.

DIPTERA.—Interesting facts concerning the seasonal incidence and abundance of sewage flies have been described by Ll. Lloyd (*J. and Proc. Instit. Sewage Purif.*, Pt. 1, 1937, 16 pp.). The species studied were *Psychoda alternata*, *P. severini*, *Spaniotoma minima*, *Metriocnemus longitarsus* and *M. hirticollis*. The thresholds of development and thermal constants have been worked out for all the phases of *S. minima* and the two *Psychoda* and also for the incubation and pupation of the *Metriocnemus*.

A *Mermis* nematode worm has been found in *Corixa geoffroyi* in the New Forest by C. J. Banks (*J. Soc. Brit. Ent.*, **1**, 1938, 217–19).

A notable revision of the British species of the short-palped craneflies has been made by F. W. Edwards (*Trans. Soc. Brit. Ent.*, **5**, 1938, 1–168). Workers on craneflies are now very well provided for in the identification of adult craneflies since recently Audcent (*Trans. Ent. Soc. S. Engl.*, **8**, 1932, 1–34) dealt with the sub-family Tipulinæ.

V. B. Wigglesworth (*J. Expt. Biol.*, **15**, 1938, 248–54) has found that the fluid present in the tracheal system of *Ædes ægypti* is removed at hatching and moulting by an active absorption which only takes place when the respiratory siphon is open at the surface so that air can enter. The nervous system is apparently concerned in the initiation of this absorption. If first-stage larvæ are kept under water the larvæ retain the fluid in the tracheal system until they obtain access to the air. If they do not gain this access within three days, the tracheal fluid does not become absorbed until the first moult. Second-stage larvæ must absorb the fluid within a few minutes of moulting or they are unable to do so.

A few years ago H. J. Koch (1934) suggested that the function on the anal papillæ (anal gills) of Dipterous larvæ might be analogous to that of the renal tubules of the vertebrate kidney. If so they should absorb salts from the surrounding medium and secrete them into the general body cavity. Now (*J. Expt. Biol.*, **15**, 1938, 52–60), using larvæ of *Chironomus* and *Culex*, he has shown that the larvæ take up chloride ions from very dilute solutions and also that this active absorption takes place exclusively through the anal papillæ. It had previously been shown by Krogh (1937) that the skin of frogs also behaves in this manner.

V. B. Wigglesworth (*J. Expt. Biol.*, **15**, 1938, 235–47) has compared the capabilities of *Ædes ægypti*, which breeds in rain-water

in rot-holes of trees, and *Cules pipiens*, which breeds in small pools, cesspits and rain-water barrels. The *Ædes* was more efficient in absorbing and retaining chloride in dilute media, while *Culex* was slightly better in keeping chloride out in more concentrated media. In media nearly free from chloride, the anal papillæ suffer functional hypertrophy and become greatly enlarged. In more concentrated media they are reduced in size. It is interesting to recall that mosquito larvæ which occur in salt water have very reduced anal papillæ, and that species which occur in rain-water collected in plant cavities have large sausage-shaped papillæ.

The larvæ and pupæ of four identified and one unidentified Tachinids parasitising *Pieris rapæ* and *P. brassicæ* have been described by G. A. Bisset (*Parasitology*, **30**, 1938, 111–22).

Dipterous parasites of spiders are rare, so it is with interest one can refer to a short paper by B. J. Kaston (*J. New York Ent. Soc.*, **45**, 1937, 415–20). The author gives two further records, but the more valuable part of the contribution is the list of references.

ARCHÆOLOGY. By E. N. Fallaize, B.A.

Recent Archæological Discovery in Great Britain.—Among recent events of major significance in the archæological world, the exhibition of finds illustrating discovery in Great Britain in the five-year period 1933–38 must take a high place. The exhibition was held at the Institute of Archæology of London University, Regent's Park, London, N.W., from March 21 to May 21. It was notable that not only did the investigations represented cover a wide area, the sites being situated in over thirty counties in England, Wales and Scotland, with one site in Northern Ireland, but also every one of the main phases of the prehistoric period was covered, from the prepalæolithic to Romano-British, with, in addition, some few exhibits from Saxon, Medieval and Stuart times.

Apart from a methodological exhibit, the collection contained seventy units of exhibition, though the number of sites was somewhat smaller, as several sites contributed to more than one of the chronological sections, into which the finds were distributed. The remark made by Sir Charles Peers, when declaring the exhibition open, on the flourishing state of archæological studies in Britain, was fully justified. In mere bulk and range the exhibition was distinctly impressive; but the quality of the work, which lay behind, was unquestionably, from the scientific point of view, all of the first order.

One feature of the exhibition, which made it especially attractive, was that an appreciable proportion of the material was derived from

excavations so recent as to be still unpublished, and indeed in some instances, still undescribed in any form. The material from last season's (1937) excavations at Maiden Castle, Dorchester, upon which Dr. R. E. Mortimer Wheeler had reported before the Society of Antiquaries only a week or two before the opening of the exhibition, proved especially attractive to visitors, although the finds being distributed in their respective chronological sections, the range of the results showing the length of the period of occupation—from neolithic to Roman with a Saxon burial to boot, possibly made less impression than was its due. The evidence of cannibalism in the mutilated skeletal material from the neolithic mound, shown reconstructed as found, with the skull which had been holed and broken to get at the brains, as well as the evidence of child burial from the same source, not unnaturally attracted much attention. These finds do not come from the earliest period of occupation. The mound in which they were found overlies the ditch of an earlier settlement, belonging to a simple Neolithic A village culture, which shows affinities with north-western France. Here were found the remains of a chalk idol of the type ultimately derivative from Hither Asia, by way of the Mediterranean and the Atlantic coast, of which one example only, from Windmill Hill, was previously known to occur in Britain.

In his report on these excavations Dr. Wheeler drew a graphic picture, based on his finds, of the last phase at Maiden Castle, when the fortress-city fell before the Romans—the system of defences guarding the gate through which the troops broke through, the barrage of projectiles covering the advance, and the determined and savage character of the attack. Enlarged photographs in the photographic section of the exhibition illustrated the last-mentioned inference from the mutilating wounds in the skeletal remains, evidently hurriedly buried after the battle.

In the following brief notes, mention will be made of one or two finds, only, which at present are unpublished; but before passing on, attention must be directed, on account of its intrinsic interest, to the Selmeston site in Sussex, on which pit-dwellings of the mesolithic period have been scientifically investigated for the first time in Britain. These pits were discovered in a sandpit, and were excavated by Dr. Grahame Clark. They produced an extensive flint industry, which seems to belong to the later phases of the period. Remains of later cultures have been shown to be secondary, while sherds of Neolithic B belonged to a hearth high up in the filling of one of the pits.

Several features of interest were noted in connection with the

excavation of the Whiteleaf barrow, Monks Risborough, Bucks, which is not yet complete. This barrow is situated on the ridge of the Chilterns, and is kidney-shaped, hollow on the eastern side, and surrounded by a ditch. An inner mound of earth is revetted with large timbers. In the centre opening, towards the hollow eastern side, was a small wooden chamber containing an intact left foot. The rest of the burial, that of a dolichocephalic male aged thirty-five, had been shovelled out and lay in confusion ; but no second burial had been made. Around the chamber were pits cut in the chalk. These were sterile of artefacts. One pit was deep, and packed with clay and flints, with a central filling of sand from the plain below. The pottery is of Neolithic A type, linking with the Upper Thames valley and the Cambridge district, but upstanding lugs have no British, but western Mediterranean affinities.

Among exhibits of Middle and Late Bronze Age date were antiquities from two hitherto unrecorded mounds in Wales, which were examined by Sir Cyril and Lady Fox in 1937, on behalf of the National Museum of Wales. These have since been described by Sir Cyril Fox before the Society of Antiquaries. They are situated near Bridgend, Glamorgan. The Simondston Cairn, Coity, consisted of a barely perceptible mound, which covered the scanty remains of a cairn 43 ft. in diameter. There was a central cist, one stone of which was cup-marked. The cover stone had been removed ; but the contents had not been disturbed. They were two urns of enlarged food-vessel type, containing respectively the bones of an adult, and an adult and child, near the former of which was a flint-flake knife, and near the latter a flint fabricator and a natural cup, part of a nodule of pyrites. Possibly these are symbolic articles suitable to man, woman and child. Subsequently the cairn, possibly in the next generation, was used as a cemetery. Five cremation burials were found, within one of which was evidence for the use of coal as fuel, probably the earliest known use in Britain.

The Pond cairn, the second of the two cairns, is situated half a mile away from the Simondston cairn. Its structure and the ritual acts, which this connotes, are alike unusual and indeed remarkable. Near the centre was a rock-cut pit, probably dedicatory, filled with stone, and containing the scattered burnt bones of a child. Beside it and on ground level in the centre of the mound was an overhanging rim urn of about 1400 B.C., containing the principal burial. Covering it was a heap of stones beneath a vertical-sided turf stack. A basin with projections, phallic in plan, and lined with charcoal, fronted the urn. Around the turf stack ran a continuous turf ring of about 60 ft. diameter. After this ring had been completed charcoal was

scattered over the floor of the interspace and trodden hard, presumably in a ritual dance. A hole in the broken inner wall-face of the ring contained a greasy black mass, the remains of vegetable matter, among which were found grains of wheat, probably *Triticum vulgare*, barley, and cheat or chess, a weed of cultivation. These grains are believed to be the first scientific record of Bronze Age food-grains in southern Britain. Fourteen hundred years later the site was occupied by Romano-British squatters, who left wheat and barley by the side of their fires. The unusual type of the Pond cairn finds affinities in Devonshire, which is the probable proximate source of the culture it represents. The Pond cairn and the secondary material at Simondston, Sir Cyril Fox concludes, represent an intrusion across the Severn sea into the Glamorgan sea-plain.

The excavation of Soldier's Grave, Nympsfield, Gloucestershire, a much-ruined cairn situated about 230 yards north of the Nympsfield long barrow, was found on excavation by Mrs. E. M. Clifford to cover a rock-cut, boat-shaped tomb, unique in this country. Pottery, apparently of Bronze Age date, was found on a ledge, which carried the slabs of stone used to roof the tomb.

[In this connection it is interesting to note in parenthesis that a barrow excavated by Mrs. Frank Elgee, Loose Howe, overlooking Rosedale, in Yorkshire, was found to contain a burial in a coffin composed of two canoes with a third canoe by its side. Associated with the burial was a bronze dagger. This find is unique, and is interpreted as an indication of a Bronze Age canoe ritual at present otherwise unknown to archæology.]

One of the most interesting exhibits in the section of the Iron Age illustrated a problem of which the solution was for long a puzzle to archæologists, more especially in Scotland. This was the occurrence of vitrification in stone forts. At Ffridd Faldwyn Camp, Montgomery, excavated by Mr. B. H. St. J. O'Neil, a mass of vitrified material was found. The native rock, Wenlock Shale, is unsuitable for dry-walling, and masses of stone fragments with soil had been heaped up to form a rampart, held in place by timbers. The whole construction had then been burnt slowly at a high temperature. The resultant mass resembles clinker. With the material from this site was exhibited vitrified material from an Iron Age hill fort at Dunagoil, Bute, and a specimen produced experimentally by Mr. Wallace Thorneycroft at Plean Colliery, Stirlingshire.

Mention of one further site must conclude an inadequate account of a remarkable collection of archæological material. At Castle Dore, Golant, Cornwall, Mr. C. A. Ralegh Radford has now com-

pleted the excavation of the ring fort lying beside the ancient transpeninsular ridgeway, about 3 miles north of Fowey. Occupation covers a period extending from the Bronze Age to Roman times and then, after a period when the site was deserted, to a reoccupation, of which the date suggested by fragments of pottery and beads is in the fifth or following centuries. The interest and significance of the site in relation to the trans-Cornish trade-route in early days has been somewhat obscured by the attraction of its recognition as the palace of King Mark of Arthurian legend, in which that monarch is associated with Tristram and Iseult. The identification is upheld by the appearance of Mark's name on a contemporary gravestone found near by.

ARCHÆOLOGICAL STUDIES IN IRELAND.—The recent activities of archæologists in Ireland challenge comparison with those of their colleagues in Great Britain ; but in Ireland they have had the inestimable advantage of assistance from public funds. Since 1934 in the Free State and 1935 in Northern Ireland, the respective Governments, with a vision and judgment, which by archæologists must be deemed worthy of all praise, have made use of part of the funds available for the relief of unemployment in conducting archæological excavation upon a systematic plan under the supervision of advisory boards with expert knowledge. So far twenty-six excavations have been carried out in the Free State in four years, several extending over more than one season, nearly all devoted to sites of outstanding importance, and supervised by some fifty experienced archæologists. The stimulus to develop archæological studies in this manner was in part supplied by the remarkable results achieved by the Harvard University archæological expedition to Ireland, which for five successive years carried out excavations on a scale and by scientific methods, such as had not been attempted previously in Ireland.

The results achieved by Irish archæologists in the last five years, for the most part under the Governments' schemes, have been reviewed recently by Dr. Adolf Mahr, Keeper of Antiquities and Director of the National Museum of Ireland, in a Presidential Address to the Prehistoric Society, which in itself is a remarkable piece of archæological analysis and exposition (*Proc. Prehist. Soc.*, 1937, pp. 261–466).

Some indication of the possibilities which Ireland holds out to the archæologist are afforded by a piece of cartographical survey work which has been carried out under the supervision of Miss M. Gaffikin on behalf of the Belfast Naturalists' Field Club. This survey is to record the monuments, archæological sites, and

antiquities generally of Northern Ireland. The first results to be published are accompanied by two distribution maps, which include (1) megalithic monuments and (2) raths, cashels, and crannogs. Some 600 megaliths have been listed, and about 2000 later monuments, though it is recognised that the lists are not complete. It will be noted that the Free State is not touched. Nothing has as yet been done for that part of Ireland on these lines.

Another branch of enquiry which has already produced material of great moment for prehistoric studies, and will become increasingly significant, is the chronological investigation of the peat by pollen analysis. As a large proportion of the antiquities salved in Ireland come from finds in the peat, the importance of this investigation needs no emphasis. The investigation of the peat in Ireland by pollen analysis began in 1928 with the researches of Dr. G. Erdtmann. It is now being continued by Dr. K. Jessen of Copenhagen under the Quaternary Research Committee. Dr. Jessen is engaged in examining specimens from the deposits of sixty sites, mostly archæological. Up to the present, tentative conclusions have suggested a datum line for the chronology of post-glacial deposits, which gives point to studies of the retreat of the ice-sheet. In the chronology of later periods, the investigations made by Dr. Jessen up to the time of writing dealt with a period identified as the beginning of the Late Bronze Age, for which, however, a date very much later (*i.e.* about 400 B.C.) than that accepted by archæologists (about 900 B.C.) is suggested. It is evident that further investigation is needed.

No attempt will be made here to summarise the mass of detailed evidence from excavation with which Dr. Mahr has dealt so ably. For this reference must be made to the address itself. He himself, however, has singled out four major results, which emerge. Of these, three are due to excavation, while the fourth, for which he is responsible, is the result of careful analysis and comparison of museum material.

The first point to which Dr. Mahr directs attention emerges from the investigations in Northern Ireland, and more especially of the "horned" cairn by E. Evans and O. Davies. By these not only have our ideas as to the cultural position and distribution of an almost forgotten but very important class of megalithic monument of Ireland, north of the central plain, been advanced, but a hotly contested question of megalithic architectural typology and development in Ireland has been settled and a whole class of north-western monuments has been placed in true generic position.

Secondly, it has been shown that the souterrain, and hence the

rath so frequently surrounding it, as a type can be as early as the Middle Bronze Age.

Thirdly, at least one crannog has been shown to be of Late Bronze Age date. This is the site at Knocknalappa, Co. Clare. The oldest previously known crannog was of La Tène dating.

And fourthly, a hitherto unobserved cultural facies has been recognised, to which Dr. Mahr has given the name of the "Riverford" culture, or people, so-called on account of the fact that finds of this lithic culture have been made either at fords, or dredged from river waters at such points. The differentiation of this culture started from a stone club found in the waters of the river Barrow, but its focal point in Ireland is the river Bann. Dr. Mahr suggests that this culture, which he associates with salmon fishing, is to be traced through northern Britain to the culture centring in northern Europe around the Baltic, to which Professor Gordon Childe has given the designation of "Forest Culture." Dr. Mahr discusses further the possibility of identifying the people with the Picts.

In connection with a discussion of the Celtic problem, Dr. Mahr sets out the succession of archæological facts bearing on immigration into, or invasion of, Ireland as follows:

(1) A mesolithic stratum (Glenarm, etc.). Probable source, the western Continent. Route: South Britain.

(2) Another lithic stratum, the Riverford people or the Picts. Ultimate source: Maglemose culture, somewhere around Denmark. Route: Dogger Bank–Scotland–Hebrides.

(3) The Megalithic invasion, heralding the Bronze Age. About 2100 B.C. Source, Iberia. Route: Brittany–Cornwall–Irish Sea.

(4) The Late Bronze Age invasion. About 900 B.C. Source, Middle (and Lower?) Rhine and Eastern plus Northern France. Route: Lowland Britain–Cumberland–Wigtownshire.

(5) The La Tène wave. About 200 or 150 B.C. British Celts coming from Wigtownshire. Occupied only a limited district of north-eastern Ireland.

In reference to (3), it is suggested that this was mainly a non-Aryan invasion; and many of the non-Aryan traits of ancient Ireland must be due to it.

NOTES

The Sodium Lamp (F. A. V.)

In the December 1937 number of *Philips Technical Review* is an interesting survey by Dorgelo and Bouma of the problems encountered in the development of sodium vapour discharge lamps. Since the vapour pressure of sodium at room temperatures is so low that no discharge can occur, the lamp is provided with a rare gas filling, which makes ignition possible. The gas discharge then heats up the walls of the tube until the vapour pressure of the sodium becomes high enough for the radiation from it to predominate. Two problems arise immediately. The heat insulation must be such that the working temperature can be maintained economically, and a glass must be found which does not quickly go brown under the action of the hot sodium. The first problem is solved conveniently by surrounding the lamp with a removable double-walled evacuated glass container. Mounting the lamp permanently in an evacuated bulb gives a simpler construction, but then this lamp has to be replaced as a whole, while the removable vacuum glass can be used again with another discharge tube. Moreover, the circulation of air between the vacuum glass and the lamp promotes uniformity in temperature. To remove the second difficulty, ordinary glass is coated with a thin layer of a borate glass, which is not attacked by sodium but is difficult to work with by itself.

For use on alternating currents the lamp, is in the form of a tube (like a neon sign), bent into a U, so that the connections for both electrodes are in one cap. These electrodes are spirals of wire, exactly similar since each has to serve as anode and cathode in alternate half-cycles. They are kept hot by the discharge. The temperature of the discharge tube must be within the limits 250°–280° C., and it is particularly important for the temperature along the tube to be uniform, to prevent uneven distribution of sodium. To help this, the leads are taken directly through the bulb without the customary "pinch," around the base of which condensation of sodium may occur. A 100-watt lamp takes 0·6

amp. at 170 volts, supplied by a specially designed transformer in which magnetic leakage paths are arranged to help to keep the discharge current constant. The brightness of the lamp is then insensitive to fluctuations of mains voltage. The efficiency is about 58 lumens per watt.

Since the light from a sodium discharge covers a very narrow wavelength band (5890 and 5896 Å), these lamps can be employed only when colour values are not important. On the other hand, the acuity of vision is found to be considerably greater than with light from an ordinary metal filament lamp. Thus the lamps are very useful for lighting tennis courts, for inspection of small articles in some commercial processes, and similar purposes. Their use for flood-lighting effects is well known.

Natural Reproduction in Salmonidæ (W. L. C.)

The discussions and investigations on the merits of the natural as opposed to the artificial methods of reproduction in salmonidæ have received a valuable addition in a recently published New Zealand paper.[1] Mr. Hobbs is Field Biologist to the Fresh Water Research Committee, Canterbury College, Christchurch, and has been at this investigation for two years. Natural spawning redds were marked out, and systematically examined so as to determine the extent of fertilisation of the eggs, the percentage of eggs that hatch, and the percentage of free-swimming fry produced. The result is a report that, in many respects, will surprise the advocates of the artificial method, since it shows how wonderfully efficient the natural reproduction is, if violent floods do not excavate the gravels in which the eggs are buried. It is shown also that ordinary floods do not disturb the redds materially, but those that carry down fine silt which is deposited on redds are almost as injurious as the floods of unusual violence which carry away the gravels altogether.

In the salmon (*Onchorynchus tschawytscha*, a Red Indian specific title) the loss through unfertilisation was 1·76 per cent. in one of the streams and the total loss up to hatching only 2·74 per cent. In another stream where there was an exceptional flood, the total rose as high as 11·99 per cent.

The efficiency in fertilisation of brown trout eggs was 99 per cent., and any losses were found to be greater in the pre-eyed stage than later. The salmon disease fungus *Saprolegnia* was found to

[1] *Natural Reproduction of Quinnat Salmon, Brown and Rainbow Trout, in certain New Zealand Waters.* By Derisley F. Hobbs, New Zealand Marine Department, Wellington, 1937.

be responsible for losses of ova at late stages. But hatching was most successful.

The streams, in the belt of country dealt with, showed a great variety of character—the rivers from Christchurch on the Pacific side to within a short distance of the Tasman Sea in Westland—and they have their origins in the foothills of the Southern Alps.

The range of the incubation period seems to us very great in view of the temperatures, being from 31 to 32 days at 12·2° C. up to 115 to 165 days, when the temperature was 2·8° C. Observations were made on 104 Quinnat redds in three streams, and samples totalling 16,252 eggs and 4146 alevins were obtained from forty-one of the redds.

The paper is described as a preliminary one, but like the results obtained in British Columbia by Dr. Foerster it already gives pause to those who have advocated hatchery methods.

Recent Work on Rhenium (G. D.)

Rhenium, the element of atomic number 75, Mendeléef's dvi-manganese, was discovered in 1925 and the various advances in knowledge of its properties and compounds have been recorded from time to time in SCIENCE PROGRESS [1].

The early work was hampered by lack of material but after its isolation in relatively large amounts by Feit [2] in 1930, from the sulphide residues left from working up molybdenum ores in certain metallurgical processes,[1] much progress was made in the elucidation of its reactions and its chemical and physical properties.

During the last two years further advances have been made. Thus, certain organic derivatives have been isolated and a number of researches throwing light upon the valency of this congener of manganese have been published. Curiously enough, nothing further has apparently been accomplished concerning masurium, the discovery of which was reported with that of rhenium in 1925 [3].

Until the end of 1937 some six oxides of rhenium had been reported, the lowest of which was the sesqui-oxide, Re_2O_3, which suggested that rhenium had a minimum valency of three and a probable maximum of seven (from Re_2O_7). But Young and Irvine [4] have produced evidence of the existence of still lower oxides. To these they assigned the formulæ, $Re_2O, 2H_2O$ and ReO, H_2O, so that rhenium now has a full range of compounds in which the element exhibits valencies from one to seven :

[1] The association of rhenium with molybdenum had been anticipated by F. H. Loring, *Chem. News*, 1926, **133**, 356.

I	Re_2O
II	ReO
III	Re_2O_3, $ReCl_3$
IV	ReO_2
V	$ReCl_5$
VI	ReO_3
VII	Re_2O_7

The monoxide, $Re_2O\ 2H_2O$, has been obtained in 25 per cent. yield by reducing perrhenic acid with zinc and dilute hydrochloric acid. The black product was purified and distinguished from rhenium, the dioxide and the sesqui-oxide. Its rhenium content was estimated by oxidation to perrhenic acid and precipitation with nitron. The valency of rhenium was found by measuring the oxygen absorbed when the oxide was heated in the gas in a closed system, the final product being rhenium heptoxide.

The oxide of bivalent rhenium was also prepared by reducing perrhenic acid but with cadmium instead of zinc. Its composition was established by the same procedure.

Another important investigation on the reduction products of perrhenic acid and its salts has recently been made by Lundell and Knowles [5], who used the Jones reductor method (zinc amalgam). By this means the halogen oxy-acids are reduced to the hydracids ; *e.g.* periodates are converted into iodides. With potassium perrhenate these authors found that the reaction followed the course :

$$ReO_4^- + 8H^+ + 8e = Re^- + 4H_2O.$$

The product should therefore be hydro-rhenic acid, HRe, but no compound of this nature was actually isolated although the authors consider that the element forms a compound in which its valency is minus one.

Considerable importance is now being attached to the valency shown by rhenium in its complex compounds. The existence of several oxychlorides and such compounds as potassium oxy-rheni-chloride, K_2ReOCl_5, discovered by Jakob and Jeżowska [6], has caused much speculation concerning the most usual valency shown by rhenium. The latest contribution to the subject is a lengthy paper by Hölemann [7], who has directed attention to the prevalence of pentavalent rhenium in its stable and complex compounds. This investigator has extended earlier work [8] on the oxythiocyanate and prepared coloured double salts (precipitates) with silver, copper ($Cu^{\cdot}$ and $Cu^{\cdot\cdot}$), mercury ($Hg^{\cdot}$ and $Hg^{\cdot\cdot}$), thallium and lead. These salts were not, however, obtained free from impurities but the evidence pointed to the existence of pentavalent (and not hexa-

valent) rhenium compounds. The complex oxythiocyanates could be prepared equally well from potassium oxy-rhenichloride or the perrhenate by interaction with ammonium thiocyanate in the presence of stannous chloride and hydrochloric acid or of hydrazine.

The conclusions formed by Hölemann are in accord with the recent isolation of iodorhenic acid, $HReI_5$, by Biltz and his collaborators [9], who obtained this acid by the action of sulphuric acid upon potassium rheni-iodide:

$$K_2ReI_6 + H_2SO_4 = K_2SO_4 + HI + HReI_5.$$

The new acid was extracted with ether, the remaining products being soluble in water, and its composition was determined.

The organic derivatives of rhenium have not attracted much attention, but trimethyl rhenium, $Re(CH_3)_3$, has been obtained [10] from the trichloride by treatment with Grignard's reagent. Ethoxides and *iso*propoxides, $Re(OR)_3$, and compounds of the trichloride with alcohols, $ReCl_3ROH$, have been described [11]. The former, which are only stable in dry air since water readily decomposes them, were obtained according to the reaction:

$$ReCl_3 + 3NaOC_2H_5 = Re(OC_2H_5)_3 + 3NaCl.$$

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Miscellanea.

The following were elected to be fellows of the Royal Society at the meeting held on March 17:—G. D. Bengough, consultant to the Chemical Research Laboratory, Department of Scientific and Industrial Research; C. H. Best, professor of physiology, University of Toronto; W. Brown, professor of plant pathology, Imperial College of Science and Technology; J. W. Cook, professor of chemistry, Royal Cancer Hospital; T. L. Eckersley, research physicist, Marconi's Wireless Telegraph Co.; G. I. Finch, professor of applied physical chemistry, Imperial College of Science and Technology; W. E. Gye, director of the laboratories of the Imperial

Cancer Research Fund; W. V. D. Hodge, Lowndean professor of astronomy and geometry, University of Cambridge; J. S. Huxley, secretary of the Zoological Society of London; J. Jackson, H.M. Astronomer, Cape Town; Sir Robert Mond; J. E. Richey, H.M. Geological Survey; F. S. Russell, naturalist at the Marine Biological Association; B. F. J. Schonland, professor of physics, University of Cape Town; F. S. Sinnatt, director of Fuel Research, Department of Scientific and Industrial Research; K. M. Smith, senior research assistant, Plant Virus Station, Cambridge; E. Stedman, chemist, University of Edinburgh; C. E. Tilley, professor of mineralogy and petrology, University of Cambridge; W. E. Turner, professor of glass technology, University of Sheffield; H. H. Woollard, professor of anatomy, University College, London.

H.M. the King has approved the award of the Founder's medal of the Royal Geographical Society to Mr. J. Rymill, for his organisation and leadership of the British Graham Land Expedition 1934–37, and of the Patron's medal to Mr. E. Shipton for his explorations in the Himalayas.

The Kelvin gold medal for 1938 has been awarded to Sir Joseph Thomson in recognition of the eminent services he has rendered to engineering science.

The gold medal of the Royal Society of Medicine awarded triennially for services in the science and art of medicine has been presented to Prof. Wilfred Trotter, director of the surgical unit at University College Hospital.

The Council of the Institution of Mechanical Engineers has decided to give its highest award, the James Watt gold medal, to Mr. Henry Ford, who was nominated by the American Society of Mechanical Engineers.

Among other medals awarded during the past quarter were the following: Institute of Metals medal to Sir William Bragg; gold medal of the Linnean Society to Prof. Sir D'Arcy Thompson; gold medal of the Institution of Mining and Metallurgy to Prof. S. J. Truscott; Bessemer gold medal of the Iron and Steel Institute to Dr. C. H. Desch; James Alfred Ewing medal of the Institution of Civil Engineers to Mr. C. S. Franklin; Redwood medal of the Institution of Petroleum Technologists to Dr. A. E. Dunstan.

Sir Albert Seward has been appointed to be a trustee of the British Museum.

Prof. W. L. Bragg, N.L., F.R.S., has been appointed to the Cavendish chair of experimental physics in the University of Cambridge, while Prof. R. H. Fowler, M.A., F.R.S., succeeds him as Director of the National Physical Laboratory.

Mr. N. K. Johnson, Chief Superintendent, Chemical Defence Research Department, has been appointed to succeed Sir George Simpson as director of the Meteorological Office.

Professor I. M. Heilbron, F.R.S., Sir Samuel Hall professor of chemistry in the University of Manchester, has been appointed to the chair of organic chemistry in the Imperial College of Science.

We have noted with great regret the announcements of the death of the following well-known scientific workers: Mr. D. Baxandall, of the Science Museum, South Kensington; Mr. W. H. B. Cameron, physicist, of Sheffield University; Sir Raymond Crawfurd, registrar of the Royal College of Physicians and Surgeons; Prof. A. Fischel of Vienna, editor of the *Zeitschrift für Wissenschaftliche Biologie*; Mr. T. W. F. Gann, archæologist; Dr. E. Goulding, formerly vice-principal of the Plant and Animal Products Department, Imperial Institute; Dr. G. B. Grinnell, naturalist, of Yale University; Mr. F. J. Hanbury, botanist, chairman of Messrs. Allen & Hanbury, Ltd.; Prof. D. W. Hering, physicist, of New York; Mr. M. W. Hilton-Simpson, anthropologist; Major F. G. Jackson, explorer; Prof. G. Jäger, physicist, of Vienna; Dr. Charles Lallemand, president of the International Union of Geodesy and Geophysics, 1919–33; Major A. D. Lumb, geologist, of the Imperial Institute; Mr. J. E. Marsh, F.R.S., of the University Museum, Oxford; Prof. F. Mesmil, of the Pasteur Institute, Paris; Mr. Edward Meyrick, F.R.S., lepidopterist, of Marlborough College; Mr. A. E. Munby, architect; Prof. Max Neisser, bacteriologist, of Frankfort; Dr. F. G. Pease, of Mount Wilson Observatory; Capt. T. S. Taylor, founder of Taylor, Taylor & Hobson, Ltd.; Mr. T. H. D. La Touche, geologist.

It is announced that the Australian Government will invite six of the leading scientists in Britain to attend the jubilee meeting of the Australian and New Zealand Association for the Advancement of Science to be held in January 1939.

The University of Oxford has received £426,000 as a result of its appeal for an endowment fund made a year ago. It is now asking for an additional £250,000 to form a research fund for humanities and natural science (£100,000), to build new laboratories for physiology and botany (£100,000), to complete and maintain the physical chemistry laboratory (£35,000), and to extend the Ashmolean Museum (£15,000).

Williams and Pickup (*Nature*, April 16, May 7) have obtained results which tend to confirm the existence of particles of mass intermediate between that of the electron and the proton in the

cosmic ray stream. Tracks presumably due to such particles were obtained in four out of several hundred cloud chamber photographs obtained in the course of an investigation designed to determine the nature of the cosmic ray particles. Three of these tracks could be attributed to particles of mass about 200 times that of the electron m_0 (the charge being negative in one case and positive in the other two), while one indicated the presence of a particle of mass greater than 430 m_0. The first suggestion that such particles may exist was made by Neddermeyer and Anderson in 1937. Street and Stevenson (1937) obtained results which could be explained by the existence of a negative particle with a mass (130 $\pm$ 25 per cent.)m_0. Nishina, Takeuchi and Ishimiya (1937) obtained indications of particles of both signs having masses about one-seventh to one-tenth of that of the proton, while Ruhlig and Crane (1938) considered that certain tracks which they obtained might be due to a positive particle of mass (120 $\pm$ 30)m_0.

In *Nature* (March 26) H. E. Ives of the Bell Telephone Laboratories stated that, by using a new type of canal-ray tube devised by Dempster, he has succeeded in measuring the transverse Doppler effect in these rays and that the results are in complete agreement with the Larmor-Lorentz theory.

Water Pollution Research Technical Paper No. 7 (H.M. Stationery Office, 30*s*.) contains a detailed description of the results of a chemical, hydrographical and biological investigation of the effects of the discharge of crude sewage on the magnitude and nature of the deposits in the Estuary of the River Mersey. The population concerned is nearly 1½ millions and from 30 to 40 million gallons of sewage (180,000 to 240,000 cubic yards) are discharged into the Estuary per day. The capacity of the Estuary decreased by about 52 million cubic yards during the period 1906–31, and it had been suggested that sewage might be responsible by causing the deposition of mud of too glutinous a nature to be eroded by the tidal streams. The investigation has taken four years and cost £26,000, but it has shown that the sewage has no appreciable affect on either the magnitude or the hardness of the deposits. The capacity of the Mersey Estuary fluctuates—it increased by 12 million cubic yards in the five years 1931–36—and the decrease in the previous quarter-century was due to the deposition of sand in the deeper parts. It is anticipated that many of the results of the experimental work carried out during the investigation will be of great value in other water problems.

A note in the *Bulletin* of the Imperial Institute, October–December 1937, gives an account of the attempts which have been made to find oil in the United Kingdom. The D'Arcy Exploration Co. abandoned its Portsdown boring after reaching 6556 ft. and another near Henfield at 5105 ft., while a boring at Kingsclere, near Newbury, had reached 4684 ft. early in December without yielding any signs of oil. Other deep bores are being made at Aislaby-in-Eskdale and at Cousland near Edinburgh. Shallower bores, already abandoned, were made at Toxwell near Worthing, at Poxwell, at Broad Bench in Kimmeridge Bay, at Pevensey and Ham. The Anglo-American Oil Co. abandoned its boring near Hellingly at 3506 ft. and in December had reached 1770 ft. near Dalkeith. Steel Bros. have penetrated 400 ft. through the Lower Carboniferous strata at Upper Booth, Derbyshire, and the Gulf Exploration Co. (Great Britain), Ltd., holds prospecting licences over large areas in Kent, Sussex, Wiltshire, Dorset and the Yorkshire Moors.

Another note in the same number of the *Bulletin* gives an account of the use of aeroplanes to convey machinery and other equipment over the mountainous and tropical country between the Port of Lae in New Guinea and the Bulolo goldfield 35 miles away. It is stated that from April 1931 to December 1936 three aeroplanes conveyed 17,255 short tons without accident to personnel at a cost, excluding depreciation, of about £10 per ton.

The *Bell Telephone Laboratories Record* for February 1938 contains a useful account of the origin and nature of the decibel and phon written by Harvey Fletcher. In the March number R. M. Bozorth describes the preparation and properties of a hollow rectangle of permalloy possessing a maximum permeability of $1{\cdot}33 \times 10^6$ gauss per oersted, the coercive force being 0·0007 oersted and the remanence 10,400 gauss. The rectangle was cut from a single crystal of permalloy containing 66 per cent. nickel, the directions of the cuts being parallel to the three cubic axes of the crystal.

Sands, Clays and Minerals for April 1938 contains, in addition to the usual articles dealing with the subjects indicated by its title, two others of historical interest. The first is a price list of the "Philosophical, Optical and Mathematical Instruments Made and Sold by BENJAMIN MARTIN, At his Shop, the Sign of Hadley's Quadrant and Visual Glasses, near Crane-Court, in Fleet Street," and Mr. Thos. H. Court, by whose permission it is reproduced, puts the date as 1764–65. The numerical prices are in most cases

not very different from those current to-day; *e.g.* a large air pump cost £15 15*s.*, prisms from 5*s.* to £1 10*s.*; "Nose Visual Glasses," 2*s.* 6*d.*, but with the best Pebbles, in Temple Frames, 16*s.* Exceptionally "Fahrenheit's Thermometer" was priced at £1 16*s.*, or, in pocket size, £1 1*s.* Martin also supplied books and maps, and a map "of 20 Miles round *London*" could be had for 6*d.* There is a warning against "*Jews, Pedlars,* &c." who sell Visual Glasses marked with the initials B.M. "too bad for any but themselves to recommend," and a note that "Good allowance will be made to merchants and others who sell again. . . ."

The second article is an extensively illustrated account of Early Technical Balances by A. Barclay, Keeper in the Science Museum, London. It opens with a description of the three balances described by Agricola in his *De Re Metallica,* and closes with an account of the precision balance built by Ramsden for the Royal Society in 1789. One other article may be mentioned, that by F. H. Cotton, describing the effects of various Rubber Fillers, which, *inter alia,* forecasts great developments in the use of mixtures of latex and cement for domestic and other flooring.

Bentley House, the new London headquarters of the Cambridge University Press, was formally opened on February 24. It is to be used for the publication side of the business, handling books only when they have left the binders. The editorial work and the printing is still carried on at Cambridge and the Syndics of the Press retain responsibility for the works which it issues. The façade of the new premises, facing Euston Road, described as being "intentionally academic," is well designed to attract the attention of the passer-by; the interior a model of luxurious efficiency. The Library, a magnificent chamber, 60 ft. × 30 ft., carried out in English sweet chestnut, contains a copy of each of the books in the catalogue, the earliest being Hey's *Lectures on Divinity,* dated 1841. The basement alone houses 600,000 volumes, and the total accommodation for the bound stock extends to 50,000 cu. ft. of rack space.

The brochure, issued on the occasion of the opening, contains a history of the Press from its inception as a department of the University by Richard Bentley to the present day. It includes Newton among its authors, Bentley having provoked and, by perseverance, secured the publication of the second edition of the *Principia.* This edition was printed by Cornelius Crownfield, printer to the University of Cambridge, but Bentley took the whole of the profits—Newton explaining how he came to allow Bentley

to print the work by the remark "he was covetous, and I let him do it to get money." [1]

At the beginning of the nineteenth century "Bibles and Prayer Books constituted the chief stock-in-trade of the Syndics," who sold their wares through London booksellers. Apparently there were financial troubles, for the Royal Commission of 1850–52, in its report on the University, "expressed the view that the Press could not be expected to pay its way unless some man of business had a financial interest in it." Acting on this opinion, the University entered into partnership with C. J. Clay, and it was during this partnership with Clay and his sons that the modern developments of the Press were begun. The first publishing house was established in the ground floor and basement of 17, Paternoster Row, in 1873, the catalogue comprising only 100 items. It now contains 5000, and it is not unusual to despatch more than 1000 packages in a single day. Indeed, one day, in Christmas week, 1930, 2800 copies of Jean's *The Mysterious Universe* were sent out. In addition to its trade in books, secular and otherwise, the Press has been entrusted with the publication of 27 Journals (including the *Proceedings* of the Royal Society) and sends out some 70,000 copies annually.

The Syndics of the Press recently took over the publication of *Discovery*, and the first number of Vol. I in the new series appeared in April. Published in a new format with excellent illustrations, the journal maintains its popular appeal and should have a long and successful life in its new home. The price is 1*s*., and the annual subscription 12*s*. 6*d*. net, post free.

[1] Brewster's *Life of Isaac Newton* on the authority of a Conduitt MS. in the Portsmouth collection.

ESSAY REVIEW

THE DISCOVERY OF RADIUM. By J. R. PARTINGTON, M.B.E., D.Sc., Professor of Chemistry in the University of London. Being a Review of **Madame Curie**, by EVE CURIE. Translated by V. SHEEAN. [Pp. xi + 411, with 31 plates.] [London: William Heinemann, Ltd., 1938. 18*s*. net.]

THE element radium was discovered and the science of radioactivity created by Marya (Marie) Sklodovska, afterwards Mme. Curie, partly in collaboration with her husband, Pierre Curie. She was born in Warsaw on November 7, 1867, both her parents being teachers. She received an adequate education and was described as a gifted child, although without any characteristics of genius. In Warsaw a woman could not attend the University, but Marie studied mathematics, physics, chemistry and biology for herself from books, having no opportunities for practical work. At the age of eighteen she began a career as a governess and was occupied with such work until she was twenty-four. In 1890 she had access to a semi-private laboratory conducted by a circle of Polish patriots and directed by her cousin, and there she made simple experiments in physics and chemistry which developed her taste for scientific research.

Her sister had gone to Paris to study medicine and Marie herself finally decided to go there to study mathematics and physics when she had saved enough money. This was at last possible, and she set out for Paris, travelling fourth class through Germany with a wooden trunk containing her clothes and books; her mattress, sheets and towels had gone before by freight. Arriving in Paris to begin the session 1891–92 at the Sorbonne, she at first lodged with her sister, who had by then married a Polish fellow-student, a doctor, but not finding the quiet necessary for intense study, Marie took and furnished an apartment in a garret, paying for her room, meals, clothes, books and paper, as well as the university fees, out of a sum of three francs a day. Light and fuel were often too expensive and as night fell the student worked in the Library of Sainte-Geneviève until it closed at ten o'clock. She had no knowledge of cookery and her meals were too often inadequate. She says of this period:

"I then lived in a room on the sixth floor of a house in the Schools quarter, and it was a poor lodging, since my resources were extremely limited. I was nevertheless very happy, having realised only at the age of twenty-five the ardent desire which I had long had of making a profound study of the sciences."

In 1893 she was given a piece of research by Prof. Lippmann; she decided to take two master's degrees, one in physics and one in mathematics, and these were duly taken in 1893 and 1894. A friend in Warsaw, Mlle. Dydynska, then providentially obtained for her a scholarship (which she afterwards repaid) enabling her to continue her studies in Paris. In the severe winter her garret was too cold for sleep; she piled her clothes on the bed and then, pulling over her chair, she placed this also on the amassed garments so that, by its weight, it could give some illusion of warmth.

In 1894 Marie Sklodovska met her future husband, Pierre Curie, then a physicist of reputation and great promise. She was engaged in research on the magnetic properties of steels, a subject in which Curie was interested. Her room in Prof. Lippmann's laboratory was too small and it was suggested that she might perhaps find a place in Curie's laboratory in the School of Physics and Chemistry; in any case, she was told, his advice would be helpful. The meeting took place and the two met again; an admiration for her scientific earnestness and high intelligence grew into a warmer attachment and Curie, then thirty-five, asked her to be his wife. There were some months of hesitation; she did not wish to be separated from Poland. Curie was willing to follow her there; he could give lessons in French for a living. At last, in July 1895, Marie wrote to a girl friend of her youth:

"I am about to marry the man I told you about last year in Warsaw. It is a sorrow to me to have to stay for ever in Paris, but what am I to do? Fate has made us deeply attached to each other and we cannot endure the idea of separating. . . . I hesitated a whole year and could not resolve on an answer. Finally I became reconciled to the idea of settling here. When you receive this letter, write to me: Madame Curie, School of Physics and Chemistry, 42 Rue Lhomond."

The two lived in great simplicity; they had no maid and Mme. Curie learnt to cook and attended to the domestic duties, at the same time continuing her scientific research. She decided to study for the Paris doctorate and a subject must be chosen for the thesis.

In 1896 Becquerel found that uranium salts have the property of affecting a photographic plate in the dark; this was the discovery of what Mme. Curie was afterwards to call radioactivity. She

chose this subject for research, working in a cold, damp store-room on the ground floor of the School of Physics and Chemistry. Permission to work in the same institution as her husband, given by the Director, Prof. Schützenberger, was, as Mme. Curie afterwards said, " an innovation quite out of the ordinary " at that time. In 1897 a daughter, Irène, was born, but Mme. Curie was able to continue her work, the care of the child being divided between her and the grandfather, Dr. Curie, who lived with the family.

All the known chemical elements were examined and in 1898 the radioactivity of thorium was discovered. At this point Mme. Curie took a decision which was to be the key to her later discoveries. She examined not only the pure compounds of the elements but also the minerals. Uranium minerals were found to be more radioactive than pure uranium salts, and in April 1898 the conclusion was reached that an unknown element, more active than uranium, was contained in the ores. Four years before this she had written :

" Life is not easy for any of us. But what of that ? We must have perseverance and, above all, confidence in ourselves. We must believe that we are gifted for something, and that this thing, at whatever cost, must be attained."

The opportunity had come to put these precepts into execution. The problem had become one of chemistry, and as chemistry then stood compounds of this unknown element must be isolated from the ore, and the atomic weight of the element determined. Pierre Curie now interrupted his own work and joined in the investigation. There is little indication in the joint publications as to which part of the work belongs to Mme. Curie, but it is highly probable that the isolation of radium is hers. She foresaw the existence of the element, persisted in the chemical work when the man would have turned away from its formidable difficulty, and she also carried out the tedious and complicated chemical operations by which alone the final goal could be reached.

The radioactive element was present in the uranium ore in much smaller amount than had been suspected. The process of separation used was partly a chemical group-separation and partly the fractional crystallisation of salts. The accumulation of radioactive material could be followed by the electrometer method ; it appeared in two fractions, one in the bismuth and one in the barium, so that two elements were apparently present.

In July 1898 the radioelement in the bismuth precipitate was characterised and named polonium, after " the country of origin of one of us " ; in December 1898 the two Curies and Bémont, a demonstrator in the School, announced the existence of radium,

which was precipitated with the barium. Although the two elements were characterised by their radioactive properties, it was necessary to proceed further. As the chemists said: "No atomic weight, no radium."

The isolation of a pure radium salt was to require four more years of hard work, undertaken in an abandoned dissecting-room of the Faculty of Medicine at the School. This shed had an asphalt floor, an old kitchen table, a rusty stove and a blackboard. An Austrian colleague, the geologist Edward Suess, arranged for a ton of uranium residues to be sent to Mme. Curie from the mines at Joachimstahl for the cost of carriage. The chemical treatment of these residues, involving work on the large scale and with boiling acids, was carried out mainly in a courtyard in the open air. Mme. Curie later wrote:

"We had no money, no laboratory and no help in the conduct of this important and difficult task. It was like creating something out of nothing, and if Casimia Dluski called my student days 'the heroic years of my sister-in-law's life,' I may say without exaggeration that this period was, for my husband and myself, the heroic period of our common existence. . . . And yet it was in this miserable old shed that the best and happiest years of our life were spent, entirely consecrated to work. I sometimes passed the whole day stirring a boiling mass with an iron rod nearly as big as myself. In the evening I was broken with fatigue. . . . I came to treat as much as twenty kilograms of material at a time, which had the effect of filling the shed with great jars of precipitates and liquids. . . . In spite of the difficulties of our working conditions, we felt very happy. Our days were spent in the laboratory. In our humble shed there reigned a great tranquillity; sometimes, as we watched over some operation, we would walk up and down, talking about work in the present and in the future; when we were cold a cup of hot tea taken near the stove comforted us. We lived in our single preoccupation as if in a dream."

In 1902 a decigram of pure radium salt was isolated and the atomic weight of the element was determined; radium was discovered in the chemical sense. Previous to this many of the principal features of radioactivity had been published and many other workers had joined in the investigation of the new subject.

Mme. Curie's thesis, "Recherches sur les Substances Radio-actives," was presented in 1902 and published in 1903. In 1903 the Davy Medal of the Royal Society was awarded to Pierre and Mme. Curie, and in the same year the Nobel Prize for Physics was divided between Henri Becquerel and the two Curies.

The shock of Pierre Curie's death in a street accident in April 1906 unnerved Mme. Curie, but she was able to continue her work. The University of Paris awarded her the chair of her late husband, the first occasion on which a post of higher education in France had been held by a woman. She also taught for many years at the École Normale for women teachers at Sèvres and in 1908 became titular professor at the Sorbonne. Her large *Traité de Radioactivité*, in two volumes, was published in 1910. The gram of radium in the form of its salt which she had by now prepared was presented by Mme. Curie to her laboratory. In 1910, in collaboration with Debierne (who had discovered actinium in 1899), she prepared metallic radium, an experiment which is still unrepeated. In 1911 she was persuaded to submit herself for election to the Institute but was rejected ; as a compensation she could claim in the same year a second Nobel Prize, in chemistry, and in 1922 she was elected a member of the Academy of Medicine.

The physiological effects of radium were discovered in 1900 by Walkhoff and Giesel in Germany. Pierre Curie in collaboration with French medical men established the use of radium in the treatment of growths, tumours and certain forms of cancer. The Pasteur Institute and the University of Paris by a joint agreement, each contributing 400,000 francs, founded an Institute of Radium which was to be built in the Rue Cuvier. This was completed in July 1914, and next month the Great War had broke out. The French hospitals, both at the front and behind the line, were very inadequately equipped with X-ray apparatus. Mme. Curie organised this service and herself worked among the wounded.

In 1920 the women of America subscribed for a gram of radium to be presented to Mme. Curie, who went herself to receive it. Even before she received it she presented it to her laboratory. In 1925 she laid the foundation-stone of an Institute of Radium in Warsaw and again went to America to receive a second gram of radium for this Institute. In 1920 the Curie Foundation was established on the initiative of Baron Henri de Rothschild, and in 1923, on the twenty-fifth anniversary of the discovery of radium, the French Government associated itself with the Foundation and passed a law granting Mme. Curie an annual pension of 40,000 francs with the right of inheritance to her two daughters.

Mme. Curie continued to direct the Institute of Radium and in the period 1919–34 publications to the number of 483 came from it. In 1934 the science of radioactivity seemed to have become static, but in that year a new impetus was given to it by the discovery of artificial radioactivity by Mme. Curie's daughter Irène and her

son-in-law and former pupil, Frédéric Joliot; these two received the third Nobel Prize of the Curie family in 1935, the year after Mme. Curie's death, on July 4, 1934. She died of pernicious anæmia, accelerated by exposure to radioactive materials.

Mme. Curie was a little over middle height, her face pale and serious and her grey eyes normally austere. The face, especially in later life, shows calm determination and firm will. She dressed very plainly and after her husband's death mostly in black. She was intensely reserved, even with her family, was unspoiled by success, and had no scientific jealousy. In the laboratory she was a neat, accurate and methodical worker, using simple apparatus. Although she had considerable mathematical ability she had little attraction to theory, but she recognised that radioactivity is an atomic property and suggested in January 1899 that the transformation of radioactive atoms was one possible explanation of the source of the energy of radioactive substances. She possessed great critical power, which she applied to her own work, and kept abreast of the later developments in physics; in her last book, *Radioactivité*, published in 1935, a year after her death, she takes account of wave mechanics and the new theory of the structure of the atomic nucleus.

Nothing can ever take from Mme. Curie the immense achievement of her persistent and brilliant work; as the discoverer of radium, and as the originator of a new science, she will hold her place among the very few highest names in the history of science. Her life is a romantic and inspiring story, which has been told most admirably by her daughter. The collection of personal papers, letters and autobiographical fragments, as well as personal recollections, which she has drawn upon for the book, and the excellent portraits and illustrations with which she has enriched it, make it a document of considerable historical value. It is written in a vivid style which is well preserved in the translation. The book does not profess to deal adequately with the scientific work of Mme. Curie, and what little is presented is sometimes marred by errors of translation. We must not withhold our gratitude that so many intimate details of the life of such a great woman should have been preserved before they could be dissipated and lost in the passage of time. The scientific specialist will find what he requires in the systematic treatises; the reader who can be stirred by a well-told story of great achievements in the face of serious obstacles will take away from this book some conception of scientific research; and the future historian of science will recover from it valuable clues to the better understanding of a remarkable figure.

REVIEWS

MATHEMATICS

Introduction to the Theory of Fourier Integrals. By E. C. TITCHMARSH, F.R.S. [Pp. x + 390.] (Oxford: At the Clarendon Press; London: Humphrey Milford, 1937. 17*s*. 6*d*. net.)

THIS book will be of great service to those interested in Fourier analysis. Prof. Titchmarsh has collected in systematic form most of the present knowledge concerning certain aspects of the theory of Fourier Transforms. This theory has been greatly influenced by the author's own simplified proof of Plancherel's Theorem published in 1923, and most of the material in the book is subsequent to that date.

The Fourier (cosine) transform $F(x)$ of a function $f(x)$ is defined by

$$F(x) = \left(\frac{2}{\pi}\right)^{\frac{1}{2}} \int_0^\infty f(t) \cos xt dt. \quad . \quad . \quad . \quad (1)$$

When $f(x)$ is a suitable function we also have

$$f(x) = \left(\frac{2}{\pi}\right)^{\frac{1}{2}} \int_0^\infty F(t) \cos xt dt, \quad . \quad . \quad . \quad (2)$$

i.e. $f(x)$ *is the transform of* $F(x)$. For example, if $f(x) = e^{-x}$ and $F(x) = (2/\pi)^{\frac{1}{2}} (1 + x^2)^{-1}$ equations (1) and (2) are satisfied.

If $f(x)$, $F(x)$ and $g(x)$, $G(x)$ are suitable pairs of transforms we have the Parseval formula

$$\int_0^\infty f(x)g(x)dx = \int_0^\infty F(x)G(x)dx \quad . \quad . \quad . \quad (3)$$

This formula leads to a series of remarkable results. To take a simple example of (3), if $f(x) = g(x) = e^{-x}$, we obtain

$$\int_0^\infty e^{-2x}\, dx = \frac{2}{\pi}\int_0^\infty \frac{dx}{(1 + x^2)^2}, \quad . \quad . \quad . \quad (4)$$

and other formulæ may be written down at once from known pairs of transforms. This type of formula is of value in all branches of Mathematics where definite integrals have to be calculated, and the formal application of (1), (2) and (3) requires no great theoretical knowledge. In the case when the functions involved all have their moduli integrable over $(0, \infty)$ there is no difficulty, and (2) and (3) may be obtained from (1) in a few lines. But this case is too restricted to be of much theoretical interest. In fact $f(x)$ and $F(x)$ must then both be continuous functions, and the integrals are consequently Riemann integrals. The scope of equations (1) and (2) is increased if the integrals are interpreted in the Cauchy-Cesàro, or again

in the "mean p-th power" sense. The theoretical problem then centres round the conditions to be fulfilled by the functions in order that the formulæ may remain valid with these interpretations.

The first two chapters are concerned mainly with convergence and summability theorems analogous to classical theorems about Fourier series, and the Parseval formula is obtained under various conditions of frequent occurrence. The integrals in this part of the book are Lebesgue or Cauchy-Lebesgue. In Chap. III Plancherel's theorem is proved by four different methods for functions of the class L^2, *i.e.* functions which are measurable and whose squares are integrable over $(0,\infty)$. *If* $f(x)$ *is of* L^2 *and* $F(x)$ *is defined by* (1), *in the "mean square sense," then* $F(x)$ *is also of* L^2 *and* (2) *holds in the mean square sense. Moreover if also* $g(x)$ *is of* L^2 *the Parseval formula holds in the Lebesgue sense.* This beautiful symmetry makes the class L^2 the most satisfactory class of functions to study in this connection. It is neither more nor less general than the class L of Lebesgue integrable functions. For $x^{-\frac{1}{2}}$ is L at the origin, but not L^2, while x^{-1} is L^2 at infinity, but not L. In Chap. IV similar formulæ are obtained for other L^p classes. In Chap. V conjugate integrals (analogous to allied series) and Hilbert transforms, given by

$$g(x) = \frac{1}{\pi}\int_0^\infty \frac{f(x+t)-f(x-t)}{t}dt, \quad f(x) = -\frac{1}{\pi}\int_0^\infty \frac{g(x+t)-g(x-t)}{t}dt, \qquad (5)$$

are studied, and in Chap. VI uniqueness theorems. In Chap. VII many interesting examples are considered in the light of the greater scope of the subject in its wider form. In Chap. VIII general transforms, in which $\cos xt$ is replaced by a more general kernel $K(x,t)$, are discussed, and in Chap. IX self-reciprocal functions, which are those for which $f(x) = F(x)$, *e.g.* $x^{-\frac{1}{2}}$ or $e^{-\frac{1}{2}x^2}$. Finally in Chaps. X and XI are given applications to differential and integral equations, and the book ends with a full bibliography of modern papers.

With regard to the class of readers for whom the book is intended, the author says, "The reader requires only a general knowledge of analysis, though he will presumably be familiar with the elements of Fourier series." The reader is, however, expected to have a working knowledge of "mean convergence" theory. For instance, he is expected to know without reference that if $f_n(x)$ is L^2 and $\lim \int [f_n(x) - f(x)]^2 dx = 0$, and if $g(x)$ is L^2 then $\lim \int f_n(x)g(x)dx = \int f(x)g(x)dx$, and more difficult theorems of the same type, obtainable from Hölder's inequality. The reader who knows nothing of Fourier integrals and only the elements of Fourier series may, however, never have practised the use of these theorems. They are to be found, for example, in Wiener's introduction to his *Fourier Integral* or in Titchmarsh's *Theory of Functions*. The author does indeed say that the present book may be read as a sequel to the latter, but some readers might prefer a few more cross-references or a more detailed introduction. Apart from omissions of explanation of this type, which will present no difficulty to the experienced reader, the proofs are presented attractively and without many misprints. It is a great help to have the essence of so scattered a subject compressed into one book, and Prof. Titchmarsh is to be congratulated not only on the brilliant way in which he has achieved this, but also on his own further contributions to the subject made in the process.

L. S. B.

A Text-Book of the Differential Calculus. By S. Mitra, M.A., and G. K. Dutt, M.A. [Pp. xiv + 302, with 20 figures.] (Cambridge: W. Heffer & Sons, Ltd., 1937. 10*s*. net.)

The authors aim at giving an up-to-date account of the differential calculus, and they address themselves to readers who may be studying the subject seriously for the first time. The first three chapters are devoted to a discussion of real numbers (Dedekind section), complex numbers, limits, convergence, and the elementary functions; here their main source is Hardy's *Pure Mathematics*. They depart from Hardy in treating e^x as the (suitably defined) xth power of e (the sum of an infinite series); log x is what we learned at school, and the derivative properties are obtained without difficulty. Many teachers will be glad to see this natural treatment carried out. The authors take the question of differentials very seriously, and they sort out carefully the ideas of a derivative, a differential coefficient, and a differentiable function of several variables. The topic of differentials is a familiarly illogical one, and the author who is to convince the understanding of his reader must himself be extremely critical of what is usually written on the subject and must constantly be on the look-out for objections which the candid beginner will raise. The authors make a serious and interesting attempt to do this, but they are not altogether successful. Cauchy's increment formula, rather than the first mean value theorem, is used to good effect in the proof of Taylor's theorem. The chapter on expansions has many neat proofs, but the sections on uniform convergence are not as precise or as accurate as they might be. This is a pity, because beginners so often miss the point of uniformity, and, in applying it, fall so readily into loose forms of expression. There are chapters on partial derivatives and on change of variables; a fuller account of transformations is promised in a companion volume on integration (perhaps the authors will provide an index there). About one-third of the text is devoted to differential properties of plane curves. The proof that chord and arc are equivalent infinitesimals is obscure, but this will perhaps be taken up in the volume on integrals. The notion of characteristic points is used as the basis of the chapter on envelopes (the non-intersecting family $y = (x - c)^3$ has $y = 0$ for an envelope), and there are chapters on asymptotes, curvature and curve tracing.

The small stock of text examples will perhaps disappoint the average reader and those who are interested in applications; it is to the teacher and to the student who is making a special study of mathematics that the book will probably make its best appeal. Such readers will find the book stimulating and containing many good things not to be found in the older calculus books. In recommending it to a serious beginner one might add the warning that the text is not always a model of rigour or precision; there are occasional bare patches which the reader will have to cultivate for himself, and others that will have to be weeded.

H. K.

Modern Theories of Integration. By H. Kestelman, M.Sc. [Pp. viii + 252.] (Oxford: at the Clarendon Press; London: Humphrey Milford, 1937. 17*s*. 6*d*. net.)

This treatise is concerned almost entirely with the theory of the Lebesgue integral, and, in the words of the author, it forms a concise and accurate introduction based on post-graduate lectures delivered by Dr. Estermann

at University College, London. The book gives a self-contained and detailed study of the Lebesgue integral. It is extremely condensed and by the use of an ingenious notation the author has succeeded in compressing the standard definition and theorems into an astonishingly small compass. At the same time this conciseness makes it a difficult book to read in the armchair. It is rather a work which needs to be read pen in hand, and with concentrated attention. The most interesting features of the exposition are the simplifications which the author has introduced into the theory of the Lebesgue Denjoi integral by use of the methods of Carathèodory and Romanovski. Such a book as this must inevitably challenge comparison with the other standard treatises on the same subject, namely, *Leçons sur L'Intégration* by Lebesgue, and *Théorie de L'Intégrale* by Saks.

The volume by Saks covers considerably more ground, and the well-known work by Lebesgue gives a much more general treatment of the whole subject, and includes an historical introduction. The present work is eminently adapted for post-graduate students, and the only criticism which the reviewer would venture to advance in that it contains no reference whatever to the work of W. H. Young. The well-known Cambridge Tract by L. C. Young on the *Theory of Integration* gives an alternative approach to the Lebesgue integral which is extremely simple and has many advantages. It seems a pity that not even a passing reference is made to this work in the present book. Apart from that, this volume can be warmly recommended as providing an excellent introduction to the modern treatment of the Lebesgue integration.

G. T.

Trigonometry. Part II: Higher Trigonometry. By T. M. MacRobert, M.A., D.Sc., and William Arthur, M.A. [Pp. 205–341 + xiii, with 20 figures.] (London: Methuen & Co., Ltd., 1937. 4*s*. 6*d*.)

Part I of this work has recently been reviewed in this journal. The subject-matter of Part II is as follows—complex numbers: vectors; products and quotients: Demoivre's theorem; applications of Demoivre's theorem; applications of the calculus: expansions in series; the logarithmic and exponential functions: hyperbolic functions.

The treatment stresses the geometrical aspect of the theory. From the teaching point of view this is a good thing but one feels that at some stage the student needs a warning against arguing too freely by reference to a diagram. This feeling is strengthened by a remark on p. 313, where the authors say (in defining the logarithmic function by means of an integral): "As the definition of a definite integral may be based on geometrical conceptions, this procedure makes it possible to avoid some of the difficulties of defining the logarithmic and exponential functions for irrational values of their arguments. This same difficulty was avoided in connection with the circular functions, which were defined as geometrical ratios. Thus it was found possible to develop the theory of trigonometry without the necessity of basing the discussion on strictly arithmetical considerations." This is simply shelving difficulties, not avoiding them.

The average student will probably be more concerned with the conclusions of the higher trigonometry than with the higher approach. For him this book will be very useful and practical and can be recommended.

Of details, not much need be said. It might, however, be remarked that the term " arg z " is used in an unconventional sense (as meaning the *point* in the Argand diagram which represents the complex number z). The authors state that the principal value of amp z will generally be understood : perhaps they would have done well to show that amp$(^1/z)$ is not always equal to $-$ amp z for principal values.

Calculus methods are employed with advantage. This reflects a welcome modern tendency to discard the old text-book habit of wading through heavy algebra rather than use a process at once concise and elegant.

As in the case of Part I, the price is very reasonable and the production of the book satisfactory.

J. W. A.

ASTRONOMY

Cosmological Theory. By G. C. McVittie, M.A., Ph.D., F.R.A.S. Methuen's Monographs on Physical Subjects. [Pp. viii + 103.] (London : Methuen & Co., Ltd., 1937. *2s. 6d.* net.)

To compress an account of cosmological theory into the compass of this small volume, which forms one of Methuen's series of " Monographs on Physical Subjects," is no easy matter. Dr. McVittie has concentrated mainly on those developments of the theory that are most easily comparable with observation and has excluded those that are of purely mathematical interest. The counts made by Habble of the numbers of extra-galactic nebulæ down to various limiting stellar magnitudes make comparison between theory and observation possible. It is in such a comparison that the interest lies—at any rate for the observational astronomer. Without such a check the various theoretical possibilities can tell us little about the broad features of the physical universe.

Dr. McVittie gives a brief summary of the observational material and then, after two mathematical chapters dealing with tensor calculus and the principles of general relativity, he proceeds to the mathematical investigation of the expanding universe and to a comparison with observation. The conclusion is drawn that the volume of space so far surveyed is a small fraction of the whole. The astronomical evidence appears to be in favour of hyperbolic space, but some extrapolation of data is involved and, in view of the observational uncertainties, this conclusion is to be accepted with reserve. The 200-in. telescope should provide in due course data extending to a fainter limiting magnitude and will perhaps enable a more definite conclusion to be drawn. The possibility that this instrument may provide definite information on this important matter would be sufficient in itself to justify its construction.

Meanwhile, astronomers will be grateful to Dr. McVittie for his careful mathematical discussion of the problem, which abounds in pitfalls. It is emphasised, for instance, that the term " distance " in an expanding universe is ambiguous as long as the method of measurement is not specified.

The volume concludes with an account of Milne's kinematical theory of the universe, developed in such a way that its resemblances to, as well as its differences from, general relativity are clearly brought out.

There is a slip on p. 3 in the definition of a *parsec*, which is the distance at which the radius of the earth's orbit subtends an angle of one second of arc. On p. 68, the author apparently accepts the long time scale of stellar

evolution (not less than 10^{12} years), whereas the astronomical evidence is strongly converging in favour of the shorter time scale inferred from the dynamics of rotating galaxies (shorter than 10^{11} years).

H. S. J.

The Distribution of the Stars in Space. By BART J. BOK. Astrophysical Monographs. [Pp. xvi + 124, with 1 plate and 12 figures.] (U.S.A.: University of Chicago Press; Great Britain and Ireland: Cambridge University Press, 1937. 11*s.* 6*d.* net.)

THIS is the first of a series of monographs which will appear at irregular intervals under the sponsorship of the *Astrophysical Journal.* They are intended to deal authoritatively with modern developments in Astronomy and Astrophysics.

Dr. Bok has divided his subject into three chapters, of which the first deals with the methods of analysis suitable for utilising to the best advantage the masses of observational material now available. In the second he summarises these data, which include star counts, investigations on the distribution of Spectral Types, and the nature and extent of the dark nebulæ whose presence makes the problem so complicated and difficult. He also points out the directions in which further observations are still urgently needed. Chap. III is devoted to a consideration of the structure of the Galaxy in the light of our present knowledge.

The concept of J. S. Plaskett, described in his Halley Lecture, 1935, has undergone little material modification, and it is still true that, while the general outlines of galactic structure may be regarded as having a certain completeness and probability, the finer details are largely unknown. The eccentric location of the sun is among the few well-established facts. Dr. Bok is, however, hopeful of more rapid advance in the near future, and anticipates that positive information on the stellar density distribution near the galactic plane for distances 1,500 parsecs from the sun will soon be available. Assuming a structure similar to that of Messier 33, the sun would be situated in one of the spiral knots, two-thirds of the radius of the nebula from its centre, and a coarseness in the density distribution in the spiral arm would account for the "Local Cluster." Bok suggests that, while the distant centre of the Galaxy is in the direction of Sagittarius, a spiral arm passes from Carina through the sun towards Cygnus.

The monograph is a valuable contribution to an important subject.

R. W. W.

METEOROLOGY

Weather Science for Everybody. By D. BRUNT, M.A. Changing World Library, No. 2. [Pp. xii + 170, with 6 plates and 20 figures.] (London: Watts & Co., 1936. 2*s.* 6*d.* net.)

IT is a moot point whether the absorbing interest most Englishmen are said to have in the weather does not stop short at their own emotional reactions to it. If the lay public really has an objective interest in the weather, or if there is any widespread desire to develop such an interest, here is an appropriate book to meet the need. Writing for the lay public, the author, who is the only University professor of meteorology in the country, has been deprived of the usual props of a technical work, especially the mathematical aids,

and at the same time has had to face the problem of evoking and maintaining interest, not merely of solving technical problems. His weapons have been his command of plain, accurate English, his readiness to employ simple diagrams as an aid to understanding, and his flair for driving home his point by relating it to the life of the ordinary man. How far he has succeeded (or can succeed) in the task of making the essence of meteorology assimilable by the completely non-technical layman is a little difficult for one who already has some familiarity with the subject to estimate, but it is safe to say that this book represents as close an approximation to success as anyone is likely to make. In the chapters that deal with climate as distinct from weather the author hardly maintains his own high standard. This part of the work is not free from ambiguities and even actual errors, some of which are mere slips (as, on p. 128, the erroneous locations of Konakry and Colon), but some (as, on pp. 118 and 124, the alleged dryness of the eastern sides of Asia and North America in intermediate latitudes, and, on p. 120, the misleading account of the ocean currents of the Pacific) are in a rather different category. On the book as a whole these are trivial blemishes, easily to be remedied in a second edition. And if the book achieves the success it deserves a second edition will soon be called for.

R. O. B.

An Introduction to Weather and Climate. By GLENN T. TREWARTHA. [Pp. x + 373, with 7 folding plates and 108 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1937. 18*s.* net.)

THIS book is precisely what it professes to be—an *introduction* to meteorology and climatology—and within the limits of the standard aimed at (equivalent to that of the Intermediate in an English university) it fills very adequately the gap in the literature available to students of Geography. The character of the work is set by the fact that it is written by a geographer for geographers. Though the two parts into which it is divided are of equal length, it is the attitude of the climatologist, not that of the meteorologist, that is dominant throughout. In Part I (weather) physical theory is reduced to a bare minimum (sufficient, however, for the stage at which the student has arrived), and emphasis is laid rather on descriptive and distributional aspects of the subject. Part I serves, therefore, as an introduction to further study of meteorology, and at the same time to the treatment of climate in Part II. This latter fact indicates, incidentally, the unity of the book as a whole—a unity which is not necessarily inherent in the subject-matter, and which the division of the work into two equal parts might tend to camouflage. The feature of Part II (climate) is the adoption of a simplified version of Köppen's classification. A coloured folding map illustrates this classification of Trewartha's, while two others, respectively of Köppen's and of Thornthwaite's classifications, accompany an appendix giving a brief explanation of those systems. The book is clearly and straightforwardly written, is adequately illustrated (in part by a series of detached maps in a cover pocket), and contains detailed references to useful supplementary reading. In short, it is an excellent elementary text-book.

R. O. B.

PHYSICS

College Physics. By John A. Eldridge. [Pp. x + 616, with 426 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1937. 18*s*. 6*d*. net.)

We in England are not accustomed to books like this any more than we are to baseball or iced drinking water. But why not? Here is a really interesting book which contains all the physics necessary for matriculation "and then some." The author declares that "when the student has learned to pass examinations and not to like physics (the case is not unknown), the teacher has failed." Eldridge clearly intends to succeed and probably will.

The approach to mechanics is unusual and perhaps, strictly speaking, illogical; but the author carries it off because his treatment follows the inevitable unconscious approach of the beginner. Thus the ideas of force (including centripetal), equilibrium, couples and so on are dealt with in the first chapter from the point of view of common knowledge, and Newton's laws of motion (*i.e.* the statement of the nature of force and the establishment of its units) are treated in Chap. 10 after elasticity, hydrostatics and some statics. But no harm is done.

The relation of physics to everyday things and to industry is constantly borne in mind and the book is well illustrated with photographs and diagrams. A number of subjects which we are accustomed to pass by in the first year—the second law of thermodynamics, for instance—are treated in a simple and interesting way.

Teachers and beginners alike should find this a stimulating book.

R. C. B.

Textbook of Thermodynamics. By Paul S. Epstein. [Pp. xii + 406, with 64 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1937. 17*s*. 6*d*. net.)

It would appear that in writing a treatise upon thermodynamics to be used as an introductory text two courses are open to an author: either to develop the fundamental concepts of the subject in a rigorous manner, and discuss their application in as wide a field as possible, or alternatively, without any very detailed examination of the development of these concepts, to pass almost directly to a detailed discussion of their application in a rather restricted field. Each method of introduction to the subject has its advocates, and the choice of method will be determined by the teacher rather than the learner. There may be little to choose between the methods in the final result, namely, ability to use the methods and results of thermodynamics as a tool in investigations, but much worry and confusion of thought is probably saved by a detailed exposition of the fundamental laws and concepts.

In the work here reviewed it is the first method which has been adopted and the treatment is wholly admirable. In view of the approaching centenary, the first law of thermodynamics is discussed in much more detail than is usual, but most readers will find this chapter of particular interest. Chapters on such topics as the capillary layer, degenerate perfect gas, electron and ion clouds, are included, and although no development of statistical mechanical theory is given, use is freely made of results from this subject in the chapters dealing with recent work where statistical and thermodynamic theory are so intimately associated.

The book is provided with a representative selection of examples, especially

in the earlier chapters dealing with what one may term the classical part of the subject. Their usefulness might have been increased by including answers. The author has his own ideas on notation and in a brief appendix discusses those systems in common use and justifies his own selection.

Although the material in this book is in excess of that required for degree students, it may be thoroughly recommended both to them and to graduates who require a more detailed knowledge of the subject than is usually acquired in their ordinary courses.

F. E. H.

Principles of Quantum Mechanics. By A. LANDÉ. [Pp. xii + 119, with 15 figures.] (Cambridge: at the University Press, 1937. 7*s*. 6*d*. net.)

APART from the well-known book by Dirac, there are few text-books on Quantum Mechanics in which the principles of the theory are adequately discussed. Landé's book represents, therefore, a very welcome contribution to physical literature. The value of the book lies in the large number of examples by which the typical quantum mechanical concepts are illustrated, such as the scattering of light by a beam of particles, the scattering of an electron beam by a grating, etc. Much weight is put on the "complementary" description of a beam of particles by a charge density and by its capacity of transferring momentum to light quanta.

In the later chapters of the book the uncertainty relation and general transformation theory are treated. The exposition of this difficult but very important part of the theory is very clear and the mathematics kept as simple as possible. The author lays much stress on the correspondence of the canonical transformations in quantum theory and in classical mechanics. It would perhaps have been desirable to discuss in this chapter also the spin (the word spin does not occur in the book), as an example of a typical quantum mechanical quantity which cannot be brought into the canonical scheme.

There are a few points in which the reviewer does not quite agree with the author. In reading the book one is left with the impression of a complete equivalence of the particle and wave theories of an electron beam. This is in some way misleading, as in the transition to the classical theory no room if left for any wave theory of the electron or any particle theory of light. There is also a short chapter on the uncertainty relations of electromagnetic field strengths which is not in full agreement with the important research by Bohr and Rosenfeld on this point.

Landé's book will be very valuable for all those who are acquainted with Schrödinger's equation and its main applications and who want to study the general theory and its fundamental concepts. The book is well produced and easy to read.

W. HEITLER.

An Outline of Atomic Physics. By Members of the Physics Staff of the University of Pittsburg. Second edition. [Pp. x + 414, with 213 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1937. 18*s*. 6*d*. net.)

IN the first edition of this book the authors produced a volume which suited admirably the purpose for which it was designed, namely, to present to students who are not specialising in physics an outline of the modern advances

in atomic physics, if in atomic physics one is allowed to include such subjects as the theory of relativity and astrophysics.

The chief alteration in the second edition has been to bring up to date the chapter on radioactivity, which included artificial transformation and cosmic rays. These two subjects have now been enlarged upon and dealt with in a separate chapter.

One cannot refrain from commenting again on the lucid manner in which the whole book is written, and on the fact that, even when discussing wave mechanics, a minimum of mathematics is used. The book will serve as a useful introduction to the subject for Honours students.

W. E. D.

Atomic Spectra and Atomic Structure. By GERHARD HERZBERG. Translated with the co-operation of the Author by J. W. T. SPINKS. [Pp. xvi + 257, with 80 figures.] (London and Glasgow: Blackie & Son, Ltd., 1937. 18*s.* 6*d.* net.)

THE subject of atomic spectra and their interpretation is now fairly well covered by several recent publications in English, and any newcomer to this field may expect no more than a lukewarm welcome unless it genuinely meets some need left unsatisfied by its predecessors. In particular, the treatise of H. E. White is so excellent and comprehensive that one opens another work on generally similar lines with some misgivings. However, in the case of the present volume these are quickly dispelled. The author's plan has been to present the subject in a manner suitable for assimilation by the beginner and by the worker in related fields. To this end he has concentrated on essentials and excluded much detail which the non-spectroscopist would probably find more tiresome than instructive. Further, he has taken particular pains to explain the contributions which spectroscopy is now making in other domains, notably magnetism and chemistry. The latter section, of some twenty pages, is probably the clearest and most illuminating survey, in such a small compass, which has yet been achieved. These same features are indeed characteristic of the book throughout. Prof. Herzberg has also shown remarkable skill not only in distilling the essential content from the welter of spectroscopic observations but also in imparting to the wave-mechanical picture such an attractive and intelligible aspect. His mastery of the subject is even more apparent in his power of exposing the underlying simplicity of apparently complex phenomena, as, for example, the Stark effect and cooling by adiabatic demagnetisation. As an instance, however, of an aberration such as may occasionally afflict even the most competent author, one may cite the diagram of the Stern-Gerlach experiment, Fig. 42, which shows the atomic beam as perpendicular to the wedge pole instead of parallel to it. Those portions of the text which are not essential for a general understanding of the subject are printed in small type, a practice which, whilst detracting somewhat from attractiveness of appearance, is probably justifiable on the grounds of utility. There are a number of photographs of spectra, plenty of excellent diagrams and a valuable bibliography. The index appears to have been compiled with care and thoroughness, and the translation is so unusually well done that it is difficult to detect even a suggestion of the original German.

As a concise and lucid exposition of present knowledge of atomic spectra and structure the book merits and will repay careful study by chemists and physicists alike.

W. E. C.

Measurement of Radiant Energy. Edited by W. E. FORSYTHE. [Pp. xiv + 452, with 224 figures.] (New York and London: McGraw Hill Publishing Co., Ltd., 1937. 30s. net.)

THIS volume by a number of contributors is compiled under the general editorship of W. E. Forsythe. The title, although describing accurately the subject-matter dealt with, gives little indication of the variety of topics discussed. The whole field of radiation measurements, excluding X-rays, is surveyed and the contents may be classified under five headings. These are, fundamental concepts and the laws of radiation, sources of radiation, methods used for measuring radiation, and finally a consideration of some special problems in radiant energy measurement.

Spectrometers and spectrographs, monochromators, thermopiles, bolometers, radiation pyrometers, photoelectric cells, the Ulbricht sphere are a few of the instruments, selected at random, with which the book deals. Some instruments of particular use in specialised work, of which descriptions should be included in a comprehensive survey, are not to be found here. This limits the usefulness of the book since the majority of research workers to whom this book will be of interest have some acquaintance with the more usual types of instrument, whereas the detailed descriptions of the construction, use, and limitations of the less-familiar apparatus remain scattered throughout many journals and relatively inaccessible.

In spite of this the book is sure to prove of great value for reference purposes, as there is here collected, for the first time in a single volume, a wealth of detailed instruction regarding the more familiar instruments and methods. Many of the finer points in the use of instruments, such as optimum setting of slit widths in spectrographs, the curvature of spectral lines, etc., are dealt with in their practical aspects. Theoretical treatment of such matters is not given in detail, but references to original literature are fairly complete. To those undertaking research involving in any way the measurement of radiant energy a perusal of the relevant sections will undoubtedly prove of value, as will the sections dealing with mechanical design to those apt to consider design a problem entirely for the mechanic.

F. E. H.

Fundamentals of Engineering Electronics. By WILLIAM G. DOW. [Pp. xiv + 604, with 207 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1937. 25s. net.)

HERE is the most comprehensive book on modern physics for electrical engineers which has yet appeared in English. It really does discuss the *fundamentals* of election tubes in a very lucid but by no means superficial manner. The book is not elementary, indeed many graduates in physics will find Chap. II (The Electrostatic Field of the Triode) quite hard going. The student of electrical engineering who works carefully through the book will be well qualified to read with profit almost any specialised text on electron or atomic physics, while the physicist will learn from it how these branches of his subject throw light on problems connected with thermionic valves, photoelectric cells, discharge tubes, etc.

Some of the topics dealt with are cathode rays, space charge flow, thermionics, electrons in metals, distribution of velocities, tetrode and pentode valves, amplifiers, oscillators and rectifiers, atomic energies and spectra, photoelectricity, discharges in gases. Good use is made of potential distribu-

tion diagrams. To pick out one example only, the analysis of screen grid characteristics is particularly clear. There are surprisingly few inaccuracies or obscurities, though one may suggest revision of the account of experimental values of " A " of the Richardson-Dushman equation, and that some mention be made of the Peltier coefficient when relating thermionic work functions and contact potential differences.

At the end of each chapter there are some most instructive problems, though it is perhaps a pity that numerical solutions are not included for the benefit of students working by themselves. There are seventeen useful tables, a twelve-page bibliography and a good index. The diagrams are beautifully drawn and the printers have done their work well.

Prof. Dow deserves not only the congratulations but also the thanks of all those who are interested in the physics of electron devices, for writing what will undoubtedly become a standard work.

F. A. V.

Electrolytic Condensers : Their Properties, Design and Practical Uses. By P. R. Coursey, B.Sc., M.I.E.E., F.Inst.P. [Pp. viii + 172, with 112 figures, including 10 plates.] (London : Chapman & Hall, Ltd., 1937. 10s. 6d. net.)

Although some eighty years have passed since the electrolytic condenser was first conceived and used in an elementary form, the commercial development of these condensers for practical purposes is a recent achievement, and the author, who has been intimately associated with that development, has given in this book a most comprehensive and valuable survey of the latest technique.

After drawing a clear distinction between the electrolytic and ordinary types of condenser, the origin and development of the former are explained. Methods of test and the results obtained with " wet," " semi-dry " and " dry " electrolytic condensers are dealt with in detail. Special attention is given to the separator, with its influence on the performance of the condenser, and the electrical characteristics of the complete unit are thoroughly investigated. Finally a chapter is devoted to a discussion of a number of particularly useful applications in industry. When explaining the calculation of power-factor in the introductory chapter the author makes use of an equivalent series resistance. The writer feels that the method of employing an equivalent parallel resistance is to be preferred in such a case as it also provides a clear conception of leakage current. Incidentally there is an error on p. 8 in the expression given for the current, the figure 1 having been replaced by I. The author also uses the word " capacity " instead of " capacitance," which has now been standardised.

The importance of the electrolytic condenser as a means of obtaining a large capacitance in a small space has come suddenly to the notice of electrical engineers and the need for a book of this kind was becoming acute. The author has filled a real gap and he has done it as effectively and thoroughly as the most critical reader could wish.

H. M. Barlow.

Fundamentals of Vacuum Tubes. By Austin V. Eastman, M.S. [Pp. xvi + 438, with 363 figures.] (New York and London : McGraw-Hill Publishing Co., Ltd., 1937. 24s. net.)

A more accurate title for this book would be *Fundamentals of Vacuum Tube 'Circuits,'* for we find less than one-tenth of the space occupied by the physics

of the vacuum tubes themselves. For example, the space-charge equation is given in its simplest form only, without any derivation, and the general treatment of thermionic emission is confined to ten pages. On the other hand, there are detailed accounts of the basic circuits employing thermionic tubes.

In the introductory chapter are lists of definitions, symbols and notations, generally conforming to the recommendations of the Institute of Radio Engineers Committee. Very useful, too, are the tables of American valve types at appropriate places in the text. Chap. IV on diodes, including mercury arc rectifiers, contains a section on filter circuits which is unusually good for a book of this kind. A short chapter on mercury vapour relays is interposed between two excellent chapters on amplifiers, perhaps the most successful part of the book, though the treatment of oscillators, including crystal and magnetostriction control, the multivibrator and inverter, is also good.

Chap. IX brings together modulation and demodulation (*i.e.* detection) in an interesting way and then goes on to deal quite briefly with heterodyne reception, regeneration (reaction) and the valve voltmeter. Chap. X is concerned with tetrodes and pentodes, with a mention of the beam power tube, dynatron and pentagrid converter. Then the three types of photocells occupy 16 pages, and finally there is the usual scrappy chapter on X-ray tubes, oscillographs and so on. There is a short appendix on Fourier analysis.

While on the whole the book is accurate and the descriptions clear, "potential" is used too often for "potential difference," and potential gradients are measured in volts instead of volts/cm. On p. 14 the thermionic currents of Richardson's equation are measured in amps per square *inch* instead of per square cm. It is difficult to choose subjects to please everyone, but in a future edition one would like to see better treatments of D.C. amplifiers, valve voltmeters and superheterodynes, balanced perhaps by the cutting out of thermocouples, X-ray tubes, etc. There are a few helpful problems at the ends of the chapters, but no numerical solutions are given. The inclusion of the title of each reference makes it easier to find the paper required, though nearly all given here are American in origin.

It will be seen that this book can be recommended to those who wish to study the design of amplifiers, oscillators and other circuits employing thermionic tubes, and whose mathematical equipment does not extend beyond elementary calculus.

F. A. Vick.

Lectures on the Mathematical Theory of Electricity. By F. B. Pidduck. [Pp. viii + 110, with 119 figures.] (Oxford: at the Clarendon Press; London: Humphrey Milford, 1937. 7*s.* 6*d.* net.)

Lecturers on the theory of electricity are often hampered by the fact that only a limited number of problems of the simpler kind can be worked through or set as examples, unless the students' knowledge of mathematics is up to the Honours standard. Such a book as this is therefore particularly welcome, for it consists of a treatment of a selection of important subjects and examples which have a definite physical interest, and for which the mathematical analysis, although essential for their effective treatment, is not too severe.

A list of headings will give an idea of the scope of the book: Electrical Screening; Cylindrical grid, illustrating the electrostatic behaviour of a triode vacuum tube; a full treatment of the quadrant electrometer; Model of a crystalline dielectric; Langmuir's equation; Force between solenoids;

Theory of Aston's mass spectrograph; Self-inductance of a long rectangular circuit, a circuit of thin wire, a finite solenoid; electromagnetic repulsion; introduction to the ether current, etc.

These lectures will undoubtedly prove of value to advanced students as a useful addition to the author's own *Treatise on Electricity* or to Jean's *Electricity and Magnetism*.

G. B. B.

Introduction to Ferromagnetism. By FRANCIS BITTER. [Pp. xii + 314, with 147 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1937. 24*s*. net.)

A BOOK on ferromagnetism is particularly welcome from an author whose contributions to the subject, both experimental and theoretical, have been considerable, and who is also in close touch with the technological developments. As a result, the pure and applied sides both receive attention. After three introductory chapters ("General Introduction," "Historical Development" and "About Metals and Alloys," the last including an account of "Bitter patterns" by Elmore) there is a long chapter (60 pp.) contributed by Yensen, on magnetic materials and their preparation. This contains probably the best available survey, in moderate compass, of the technologically important materials, and also a detailed description of the technique for the preparation and analysis of materials of very high purity. In the next chapter "Internal Fields" are considered. There is a full account of the Heisenberg theory, of Ising's treatment of the linear chain, and of Bitter's own extension of this treatment. Crystalline fields are discussed, and there is an excellent presentation of a tensor method for deriving the general forms for energy expressions compatible with particular types of crystal symmetry. The method is extensively applied in the following chapters on "Magnetization" and "Mechanical Deformation." In the main, it is what may be called the formal theory which is here developed, both for single crystals and polycrystalline material; much of the work is original, and the development is carried out with a degree of completeness which has not hitherto been attempted. In the last two chapters, electrical and thermal properties are briefly considered; and "Co-operative Phenomena" are discussed by Zwicky in a short appendix, very relevant in connection with the structure-sensitive properties of ferromagnetics.

The book as a whole perhaps lacks balance, but with a many-sided subject like ferromagnetism it is impossible to deal at all fully with every side in a single volume, and, probably rightly, the author has chosen to concentrate on those aspects in which his interests have centred. Of these, he gives a masterly account. If only for the discussion of the phenomena which can be correlated by the formal theory, and for Yensen's survey of magnetic materials, the book should be in the hands of all those who have an interest in magnetism. There is, however, much besides; and the book throughout bears the impress of the author's critical alertness of mind and of his independence of outlook.

E. C. S.

A Text-Book of Light. By G. R. NOAKES, M.A., A.Inst.P. [Pp. x + 355, with 276 figures and 1 folding chart.] (London: Macmillan & Co., Ltd., 1937. 6*s*.)

THIS is an interesting and useful text-book written for sixth-form students and scholarship candidates specialising in science. It deals with both geometrical

and physical optics and particular attention has been paid to important topics such as Photometry, Optical Instruments, Resolving Power, etc. The sign convention is dealt with in a comprehensive manner and the explanations given in working out typical problems should be of material assistance to the student. The book is a sound exposition of fundamental principles—the author eschews descriptions of stock experiments and uses the space thus saved for the discussion of phenomena not usually included in a book of this range. It is particularly well written and produced and can be recommended with confidence as fulfilling in high degree the aims of the author.

S. MARSH.

Aeroplanes and Aero Engines. By CAPTAIN P. H. SUMNER. Second edition. [Pp. xvi + 254, with 211 figures.] (London : The Technical Press, Ltd., 1937. 15*s*. net.)

THIS is a profusely illustrated book. An opening historical section includes twenty-three reproductions from photographs of early gliders and aeroplanes. There are some forty similar illustrations of modern aircraft and their components, and a still greater number showing engines and their parts and instruments. These are supplemented by numerous clear diagrams and drawings. Such a collection in a small volume is valuable to the student whatever his grade, giving him an insight into lay-out, construction and progress. The descriptive notes added are very useful but might well have been amplified, for example in regard to wing loading and landing speed, civil machines being quoted where necessary to preserve official secrets. Unfortunately, the theoretical expositions contained in chapters on "Sustentation and Transition," etc., cannot be recommended with equal confidence. The sentence (referring to airscrews), "High velocities will, however, increase skin friction owing to greater viscosity in the dense path of vortices traversed by the blade," instances the advantage of leaving scientific explanations to the text-books.

N. A. V. P.

CHEMISTRY

A Text-Book of Inorganic Chemistry. By J. R. PARTINGTON, M.B.E., D.Sc. Fifth edition. [Pp. viii + 1062, with 390 figures.] (London : Macmillan & Co., Ltd., 1937. 15*s*.)

THE fourth edition of Prof. Partington's well-known *Text-Book of Inorganic Chemistry*, which appeared in 1933, underwent very considerable revision, consequently although many developments have occurred in the intervening years, it has not been found necessary to introduce any major changes in the fifth edition. Minor alterations are evident throughout the text and such subjects as the isotopes of hydrogen, the theory of indicators, thermochemistry, nitrogen fixation and the inactive elements have been extended and brought up to date. A new account of the structure of the silicates has been added and the sections on rhenium and coal-gas have been revised with the help of Prof. H. V. A. Briscoe and Mr. C. J. D. Gair. The new matter has been introduced without disturbing the pagination of the fourth edition.

"Partington" is the constant companion of all concerned with inorganic chemistry, suggestions for its improvement are difficult to find and their trivial nature is a testimony to the outstanding excellence of the book. The description of the metallic carbonyls appears to be due for revision, those

of chromium and tungsten not being mentioned, and the formula $Mo_6(CO)_{24}$ might well disappear. Space might also be found for a discussion of the constitution of the boron hydrides and there are occasional small gaps in the index, *e.g.* the reviewer wanted to see if the new work on perchromic acid had been included but failed to find it in the index, although other per-acids and chromium compounds are included. J. N. S.

Elementary Physical Chemistry. By H. S. TAYLOR, D.Sc., F.R.S., and H. A. TAYLOR, Ph.D. Second edition. [Pp. xvi + 664, with 112 figures, including 2 plates.] (New York: D. van Nostrand Co., Inc.; London: Macmillan & Co., Ltd., 1937. 16*s*. net.)

TEN years have elapsed since the appearance of the first edition of "small" Taylor—a book serving as an introduction to the large two-volume text-book. In the meantime a second edition of "large" Taylor has been published, but is now in some respects out of date, so that the second edition of smaller volume has had to incorporate much new material. There is a considerable and welcome breakaway from the traditional methods of presenting certain aspects of the subject. Naturally the introductory chapter on the atomic concept of matter has undergone extensive modification—the discovery of new isotopes and of new particles and the beginnings of nuclear chemistry have been compressed into some ten pages. The biggest change is the introduction, after the conventional chapter on the gaseous state, of a chapter on the quantum theory, developed for the most part historically. This leads to a consideration of the energy levels of atoms and molecules, a conception which ought to be more fully used in all physical chemistry text-books. The following chapter deals with the rotational heat capacity of molecules, especially of hydrogen and deuterium. Brief mention is also made of dipole moments. But the most refreshing chapter of all is that on chemical kinetics. A decade of research has completely transformed the subject, with the result that it has now reached a sufficiently stable stage to be profitably discussed in an introductory text. In this chapter—wholly confined to gas reactions—the classical theory of activation is discussed in relation to bimolecular reactions. Next follows an account of the calculation of activation energies from quantum mechanical principles. Unimolecular reactions, atomic reactions and the theory of explosions can then be logically considered. Heterogeneous reactions are discussed quite conventionally in the succeeding pages.

With this background photochemistry may then be regarded in an enlightened manner. Here the chapter is brought up to date without radically altering the previous arrangement.

This volume therefore depicts the changing face of physical chemistry—the elimination of unnecessary and often confusing material and its replacement by data and ideas which are more precise and more concise.

The exhortation from publisher to student to preserve the book for future reference seems out of place in an English edition. H. W. MELVILLE.

The Retardation of Chemical Reactions. By KENNETH C. BAILEY, Sc.D., Litt.D., F.I.C. [Pp. viii + 479, with 29 figures.] (London: Edward Arnold & Co., 1937. 26*s*. net.)

THE subject of negative catalysis has attracted many workers for more than a hundred years and in the post-War period its literature has assumed very

large dimensions. This is not surprising when one remembers the theoretical interest of the subject (especially in connection with the theory of chain reactions) and its enormous economic and industrial importance. It *is* surprising to find, however, that this is the first comprehensive monograph on the retardation of chemical reactions. There are so many undesirable reactions which proceed all too easily and damage or destroy valuable products; one may mention in this connection the corrosion of metals, the deterioration of rubber, and the development of rancidity in butter and other fats. Other reactions such as the setting of cements and the combustion of hydrocarbons in the internal combustion engine need to have their speed controlled if the best use is to be made of them. In the volume under review Prof. Bailey has collected the literature on these and other subjects and has provided a clear and critical discussion of many theoretical and practical problems. His monograph is particularly valuable for its detailed accounts of experimental work which are often not readily accessible in the original papers.

The order of the chapters is determined by the types of reaction considered. Of these, oxidation reactions naturally are the most numerous, and the book opens with an account of work on the glowing of phosphorus; this is followed by a discussion of other gaseous oxidations. Then come chapters on oxidations in the liquid phase (a field in which Moureu and Dufraisse have done such remarkable work), on anti-knock compounds, the protection of rubber, the setting of plaster and cement, the stabilisation of hydrogen peroxide and many other topics. Some of these chapters are of necessity summaries rather than full discussions. Thus the chapter on corrosion gives an interesting review of modern views on a very complex subject which is condensed into 20 pages.

The bibliography contains 1630 entries and will be of great value to all workers in this branch of chemical kinetics.

S. S.

The Fine Structure of Matter. The Bearing of Recent Work on Crystal Structure, Polarization and Line Spectra. Vol. II. of **A Comprehensive Treatise of Atomic and Molecular Structure. Part I: X-Rays and the Structure of Matter.** By C. H. DOUGLAS CLARK, D.Sc., A.R.C.S., A.I.C., D.I.C. [Pp. lxxii + 216, with 59 figures.] (London: Chapman & Hall, Ltd., 1937. 15*s*. net.)

IN inorganic and organic chemistry the "comprehensive treatise" is well known. Such books usually consist largely of a mass of empirical data, which is classified and presented in logical sequence as the fundamental principles of the subject are developed. The application of this method to the more physical and mathematical sciences is not so common, and the book before us appears to be an attempt in this direction. Tables of contents, indices and lists of abbreviations occupy one-quarter of the book. About one-half of the remainder is devoted to diagrams and lists of references, and then there are about forty pages of very condensed tables of numerical data. The actual amount of readable matter is thus quite small, and nearly every sentence gives a summary of the results of one or more original papers.

The classification of the crystal structures described in the first six chapters follows closely the "Strukturbericht" of Ewald and Hermann. A more adequate introduction would be very desirable, and the necessary condensation produces several misstatements, *e.g.* the investigations of Dhar

and Robertson do *not* agree regarding the structure of dibenzyl. The diagrams are sometimes unduly complicated and not always free from errors.

The following chapters deal with the structure of amorphous substances, liquids and liquid crystals, fibres and proteins, alloys, intermetallic compounds and solid solutions. The concluding chapter is concerned with the classification of crystal types, ionic, molecular, etc., and the factors affecting crystal structure, and gives a useful summary of the work of Goldschmidt and others.

An immense amount of work has been devoted to the compilation of this book, which covers a field not previously summarised in English. In conjunction with a good general text-book it should be useful in guiding the student to the original sources.

J. M. ROBERTSON.

Colloid Systems: A Survey of the Phenomena of Modern Colloid Physics and Chemistry. By A. VON BUZÁGH. Translated by O. B. DARBISHIRE, B.Sc., and edited by WILLIAM CLAYTON, D.Sc. [Pp. xx + 311, with 68 figures.] (London: The Technical Press, Ltd., 1937. 30*s*. net.)

COLLOID science has travelled far since Thomas Graham distinguished "colloids," which diffused with difficulty through membranes, from "crystalloids" which passed through with ease. The term "colloid" describes now, not a group of substances, but a state, and a great variety of compounds (including many typical crystalloids) have been obtained in the colloidal condition.

The importance of colloid science scarcely needs to be emphasised. The colloidal condition is characteristic of a vast number of natural materials, rubber, silk, cellulose, humus, and the like. Living cells consist mainly of colloidal matter, and proteins are perhaps the most important of the great family of hydrophilic colloids. Glue (Gk. *Κόλλα*), which suggested the term colloid, is indeed a mixture of proteins.

So rapid has been the advance in colloid science in the last quarter of a century that Dr. von Buzágh can say with justice that Wolfgang Ostwald's "World of Neglected Dimensions" has become the "World of Not-to-be-neglected Dimensions."

The principal title of this book indicates that its primary purpose is systematic. "Systems of classification seem to be both the delight and despair of physical science," wrote Dr. Hedges, and the words might serve as a text for the first half of Dr. von Buzágh's book, in which the tremendous task of classifying colloid systems is essayed. The work is carried out with relentless logic, and a wealth of technical nomenclature which sometimes becomes wearisome to the reader, but any such weariness is swallowed up in admiration for the thoroughness with which the project is executed.

At intervals through the early chapters, the author pauses to assert, almost fiercely, the right of colloid science to be "autonomous," to have its own set of laws which may be, and in many cases apparently are, different from the laws of classical physical chemistry. Colloid materials do not fit readily into the usual framework of homogeneous and heterogeneous systems, amenable to phase rule treatment. Dr. von Buzágh very pertinently quotes Willard Gibbs' own reminder that, in the derivation of the phase rule, energy depending on the surfaces separating heterogeneous masses is neglected. Now colloidal systems are those in which the size of the particles of the disperse phase lies between the wave-length of light (about 500 $m\mu$) and average

molecular size (about 1 mμ). In such systems, the portion of energy due to the interfaces is never negligible, and phase rule treatment breaks down. We are reminded, too, that Faraday's laws of electrolysis are not valid for colloidal systems, and that the solubility of a colloid is not independent of the mass of colloid present.

The second half of the book includes masterly treatments of adsorption, the electrical properties of disperse systems, the formation and destruction of colloid systems, and other important subjects, to the study of some of which Dr. von Buzágh has himself made important contributions.

This is a work of solid value. It is scarcely suitable for the junior student or the casual reader, who wishes to get a general idea of the content of colloid science. The author indeed assumes that his reader has already considerable acquaintance with the subject, and introduces such topics as the ultramicroscope, the Hardy-Schulze law, and the Hofmeister series, without detailed explanation. On the other hand, we have here a book which no serious student of colloid chemistry can afford to ignore, one which must take its place on the shelves of every good science library.

The reviewer has observed one error in historical fact. The author (p. 289) attributes the name *syneresis* to Wo. Ostwald. More than seventy years ago, the word *synaeresis* was used by Thomas Graham to describe the phenomenon of shrinking of a gel, with simultaneous extrusion of some of the liquid phase (*J. Chem. Soc.*, 1864, **17**, 320).

A word of praise is due to the translation, which shows none of the occasional awkwardness of phrase usually found in translations from German into English. The book is beautifully produced, and seems quite free from misprints.

KENNETH C. BAILEY.

Colloid Chemistry. By JEROME ALEXANDER, M.Sc. Fourth edition. [Pp. xviii + 505, with 43 figures and 1 folding plate.] (New York: D. van Nostrand Co., Inc.; London: Chapman & Hall, Ltd., 1937. 22*s*. net.)

WOLFGANG OSTWALD once referred to colloids as "Die Welt der vernachlässigten Dimensionen." With the appearance of Jerome Alexander's *Colloid Chemistry* we observe the complete refutation of this colloidal caption. Indeed, the reader of the book (in its fourth edition) might, with some reason, enquire what is not included by the term colloidal chemistry, for although the work goes under this unassuming title, its subject-matter ranges from quantum mechanics on the one hand to consciousness and mentality on the other.

The author has endeavoured to bring together the facts of colloidal chemistry, in the widest sense, in a co-ordinated manner, reliance being placed on natural segregation rather than on any arrangement imposed by the adoption of systematic theories of colloidal phenomena. First, there are seven chapters describing the colloidal state of matter. Then follows a long chapter dealing with properties and behaviour of colloids. The remainder and greater part of the book is devoted to practical applications, which are almost too numerous to mention; suffice it to say that a start is made with meteorology and a termination with bioelectricity. There is a very useful section giving details of a number of simple experiments, while a glossary lightens the burden of the non-specialist reader.

The style is therefore almost wholly descriptive with the intention that

its appeal should be to the general (scientific) reader. In various parts of the book it is sometimes difficult "to see the wood for the trees." For a reader already acquainted with this section of chemistry a surfeit of facts is easily assimilated and often stimulating, but for the non-specialist the result is apt to be confusing. This is a small fault, however, as the exhaustive references in the text and at the end of the book make it easy to follow any line of development. As a text-book, the volume is eminently suitable for students tending towards the biological side of chemistry, many of whom have an apathy towards quantitative expression of chemical phenomena.

H. W. Melville.

Metallography. By C. H. Desch, D.Sc., Ph.D., F.R.S. Fourth edition. [Pp. viii + 402, with 18 plates and 145 figures.] (London, New York, Toronto: Longmans, Green & Co., 1937. 21*s.* net.)

It is fifteen years since the third edition of this well-known work appeared, and twenty-seven years since it was first published, and a comparison of the 1910 edition with the present volume affords an easy method of appreciating the great advances made during the intervening period in this important branch of metallurgy.

Even during the past fifteen years the progress has been so great that for the production of the present volume drastic revision of the whole work and the complete re-writing of many of the chapters has been rendered necessary, and has been carried out in an efficient manner.

The previous editions have proved so valuable to teachers and students of the subject that it is satisfactory to realise that the general plan of the book has been retained to provide a general introduction to the subject, the basis of which consists of thermal and microscopical examination. The improved technique of recent years in connection with both these methods of examination is described and illustrated.

The use of X-ray methods of examination has proved to be of very great value, and it is well to find that two chapters specially prepared by G. D. Preston are devoted to these methods, one to theoretical aspects and the second to experimental work. In these the various methods used are described and illustrated, and to the second of these chapters there are valuable appendices by the author himself, dealing with the lattice structure of the metals and the diffraction of electron beams.

The thoroughness with which the revision has been carried out is well evidenced by the many references to work published during recent years, including 1937, and to subjects such as growth of metallic crystals, single crystals, age-hardening, etc., to which work has been devoted during these years.

The work is excellently produced and well illustrated by diagrams and very satisfactory plates of structures, etc., and can be thoroughly recommended as a text- or reference-book.

C. O. B.

Protective Films on Metals. By E. S. Hedges, M.Sc., Ph.D., D.Sc., A.I.C. Second edition. Vol. V of a Series of Monographs on Applied Chemistry under the editorship of E. Howard Tripp, Ph.D. [Pp. xvi + 397, with 53 figures, including 15 plates.] (London: Chapman & Hall, Ltd., 1937. 21*s.* net.)

To those acquainted with the first edition of Dr. Hedge's book, the revised volume needs little recommendation; the new edition follows closely the

lines of its predecessor, providing in the first six chapters a useful survey of modern knowledge on corrosion. The author sets out to give, not a complete, encyclopædic account of the subject, but a clear and interesting review which will be welcomed by all—whether they are interested primarily in the scientific or in the industrial aspects of the subject. The introductory chapter on the electrochemical character of corrosion is followed by chapters on the attack on metals of pure air, polluted air, and chemical reagents; all these subjects are considered with special reference to the conditions under which protective films are formed. The account of periodic phenomena connected with anodic treatment (with which the name of Hedges is so closely associated) will be particularly appreciated.

It may, however, be felt that, in places, insufficient space has been allocated to recent work. The new material incorporated in that section of the book which possesses most interest for the academic reader, is no more than 10 per cent. of the whole; clearly in this space it is impossible to mention all the developments of the last five years.

The last seven chapters, which have been thoroughly revised, and to which much new material has been added, embrace the technical aspects, including protection by oxide and similar films, electrodeposited, hot dipped and sprayed coatings, cementation and paints.

Whilst an author must be allowed complete freedom in his choice of subject-matter, the hope may perhaps be expressed that the third edition will include some reference to the rôle played by protective films in alloys intended for high temperature service, and a fuller account of modern theories regarding the mechanism of film formation. The recent work of Wagner, Reinhold and many others has given a clear and quantitative explanation of the processes involved and appears worthy of mention. However, these omissions do not detract seriously from the value of this book, which will be welcomed by all.

Finally, a word of praise is due to the publishers; both printing and binding are excellent.

L. E. Price.

Steels for the User. By R. T. Rolfe, F.I.C. [Pp. x + 280, with 90 figures, including 38 plates.] (London: Chapman & Hall, Ltd., 1937. 21*s*. net.)

The publication of this book is the direct result of a number of requests for the republication of matter contained in a series of articles which appeared in *The Iron and Steel Industry* during the years 1934–37. This republication has permitted revision where necessary and the inclusion of additional matter which has added to the value of the work. In the main the book deals with plain carbon steels although certain alloy steels are mentioned from time to time in special connections.

The subject is dealt with from the practical point of view of the user of steel in engineering practice and the book should prove of great value to all engineers and many metallurgists.

The matter is divided into eleven chapters, the first two of which are devoted to the mechanical quality and its assessment and to specified requirements for commercial steels. This is followed by an account of the effect of composition on the mechanical qualities, and here the practical use of the iron-carbon diagram, which is described, is made clear and the chief effects of carbon, silicon, manganese, sulphur and phosphorus are summarised.

Chap. IV is devoted to the two classes of steel, bright and free-cutting, which are largely used in engineering practice, but to a consideration of which little published matter has been devoted.

The heat-treatment of steel has three chapters devoted to it, dealing with theoretical considerations, industrial heat treatment of low carbon steels and heat treatment of high carbon steels respectively.

To the case-hardening of steel a chapter is devoted, in which the various methods of applying the high-carbon case and its subsequent heat-treatment are dealt with together with the nitrogen case-hardening and the various surface-hardening processes.

Chap. IX deals with the important question of the use of steels at elevated temperatures together with methods of determination of limiting-creep stresses.

Chap. X deals with fatigue testing and the final chapter is devoted to the general principles of selection for steels for service in which the uses of steels of carbon content from 0·05 to 1·15 are summarised.

C. O. B.

The Analytical Chemistry of Tantalum and Niobium : the Analysis of their Minerals and the Application of Tannin in Gravimetric Analysis. By W. R. SCHOELLER, Ph.D. With a foreword by G. ROCHE LYNCH, O.B.E., M.B., F.I.C. [Pp. xvi + 198.] (London : Chapman & Hall, Ltd., 1937. 21*s*. net.)

UNTIL comparatively few years ago our knowledge of the analytical reactions of the metals tantalum and niobium was extremely slight ; very little work had been done on these elements since the classic research of Marignac in 1866, and the scanty literature on the subject contained many inaccuracies. In recent years the two metals have become of economic importance, and a fuller knowledge of their chemistry became imperative. In 1919 Dr. Schoeller and his collaborators commenced a systematic study of these two elements from the analytical standpoint ; their work was spread over a period of seventeen years, and the results are embodied in the present volume. The book is divided into three main sections : in the first of these the various minerals containing tantalum and niobium are enumerated, and the methods of opening them up and analysing them are discussed. In this connection it should be pointed out that out of 52 analyses of tantalum and niobium minerals which are quoted, all but 3 were made before the present methods of separation became available. In view of the proved inaccuracy of the older methods the published figures are in many cases open to grave doubt, and a systematic reinvestigation of the composition of these minerals would seem to be called for. The middle section of the book deals with separations of the earth acids from other elements, and also includes the author's new process for the separation of tantalum from niobium, based on the fractional precipitation of the former by the careful addition of tannin and ammonia to an oxalate solution. The tantalum complex which is produced is yellow, the corresponding niobium one red, which allows the progress of the fractional separation to be readily followed.

The great value of tannin as a reagent in analytical chemistry, due to the ease with which it forms adsorption complexes with many elements, is emphasised in the final section of the book, which deals with a number of analytical processes of this type.

Every chemist who has had occasion to carry out analyses of the complex titanoniobate and tantaloniobate minerals will appreciate to the full the extremely valuable contribution which Dr. Schoeller has made to chemical literature by his researches in this field. His long and painstaking work has led to the establishment of analytical methods which, although they are of necessity often rather complex, nevertheless yield separations as perfect as those obtainable in the case of the commoner elements. The book is one which is indispensable to all those mineralogical analysts who are concerned with what must still be regarded as the most difficult field of inorganic analysis.

H. F. H.

Reagent Chemicals and Standards. With methods of assaying and testing them ; also the preparation and standardization of volumetric solutions and extensive tables of equivalents. By Joseph Rosin. [Pp. x + 530.] (New York : D. van Nostrand Co., Inc. ; London : Chapman & Hall, Ltd., 1937. 30*s*. net.)

Until recently the chemicals of analytical purity called for by chemists were, apart from the volumetric reagents, comparatively few in number. Nowadays highly purified substances are needed for such a variety of scientific and industrial purposes that the number is of the order of 500. Indicators and organic reagents alone have added very considerably to the list. All who use high-grade chemicals are much indebted to certain manufacturers for developing and publishing methods of testing reagents. To-day, however, there is room for an independent and more comprehensive work—a need which was for a time filled by Murray's *Standards and Tests for Reagent Chemicals*. The work under review is based on the same general plan, but the methods have been thoroughly revised, and more than a hundred additional substances are discussed.

The author has served on the American Chemical Society Committee on Analytical Reagents, and on the Revision Committee of the U.S. Pharmacopœia. He also brings to the task the experience gained by many years' association with firms producing high-grade chemicals. He rightly stresses the value of determining the percentage purity of reagents, and gives methods of assay for about 70 per cent. of them.

The section of twelve pages on the preparation and standardisation of volumetric solutions includes some recommendations which hardly maintain the up-to-date character of the greater part of the work. Since the monographs section deals with a large number of pH indicators it is surprising that only methyl orange is suggested for the titration of acid against sodium carbonate. This traditional indicator, whilst tolerably satisfactory for normal solutions, is decidedly inferior for acid as dilute as $N/10$. If it is desired to avoid boiling off carbon dioxide (in which case methyl orange is unsuitable) it is important to use an indicator which permits the end point to be judged to within 0·2 of a pH unit, since this is the order of change produced by the addition of one drop of $N/10$ acid beyond the end point. A mixed indicator is much to be preferred. Of single indicators bromo-cresol green, if taken to the greenish-yellow stage, gives a far sharper end point than methyl orange. The recommendation to check the normality of standard solutions by gravimetric methods is good, but surely in these days when so many convenient types of filtering crucible are available it is time that the original Gooch form be relegated to the museum.

It must be remembered that the section criticised is but a small fraction of the entire work, which is far more comprehensive in scope than any previous publication, and forms a most useful addition to the literature of the subject.

T. B. Smith.

The Reactions of Pure Hydrocarbons. By G. Egloff. American Chemical Society Monograph Series. [Pp. xviii + 897, with 42 figures.] (New York: Reinhold Publishing Corporation; London: Chapman & Hall, Ltd., 1937. 84*s*. net.)

The nature of this book is not at once appreciated from its title, but the first two sentences of Chap. I make it clear what is to follow: "It has been the purpose of the author to bring together in this volume the reactions of pure hydrocarbons which occur as the result of thermal, catalytic, photochemical or electrical treatment. Reactions of hydrocarbons in the presence of non-hydrocarbon substances have been included only when the latter act catalytically and the reaction products are hydrocarbons."

The treatment of methane affords a simple example of the general method employed. First the results of investigations of the equilibrium: $CH_4 \rightleftharpoons C + 2H_2$ are recorded in fair detail. Then the attempts to convert methane into hydrogen, into various forms of carbon, into ethane, into olefins, into acetylene and into aromatic hydrocarbons are dealt with. Later follows an account of work on "electro-treatment" of methane (spark and arc; electric discharge; cathode rays) and of the effect of α-particles.

Under "Reactions of Olefin Hydrocarbons" are given the various theories of polymerisation, and under the individual olefins numerous tables recording known results are set out. Some idea of the extent to which the author has surveyed this field is given by the fact that 244 references are given to the literature of olefin "reactions." The polymerisation of acetylene and its homologues is treated very fully, and then follows a long chapter on reactions of aromatic hydrocarbons. The twenty pages of general theoretical discussion are of some interest, as are those which follow on benzene. Other hydrocarbons dealt with include *cyclo*paraffins, *cyclo*-olefins and terpenes. The account of the work on isoprene makes one realise again that this book is written from a very unusual angle.

That the volume under consideration will be very useful to many chemists cannot be doubted for a moment. About 1600 references are given to all kinds of journals, showing how exhaustively the author has searched the literature. There are a few mistakes—surprisingly few: references 119 on p. 137 and 120 on p. 174 appear to have got mislaid in the preparation of the chapter bibliography.

On the whole, the book is rather an undigested collection of abstracts. It would be very handy for anyone starting a research on any of the subjects dealt with. It should appeal strongly to industrial chemists because it enables one to see quickly what work has been done, but it would probably appeal less strongly to academic workers, who would have liked the author to take up a more critical attitude and give his own opinions, which clearly must be valuable ones. The labour involved in collecting all the recorded facts must have been prodigious.

E. E. Turner.

Laboratory Practice of Organic Chemistry. By G. Ross Robertson. [Pp. xii + 326, with 71 figures.] (New York and London: Macmillan & Co., Ltd., 1937. 10*s*. net.)

The author has endeavoured in this book to combat the idea, occasionally voiced by our more armchair teachers, that practical organic chemistry consists mainly of a series of preparations which require little skill and less thought. A considerable portion of the book is devoted to the theories underlying laboratory manipulations, and material usually crammed for examinations under the name of elementary physical chemistry is thus made interesting to the student and shown to have a soul higher than a bit of mathematics. A number of the better "tips" are given for making manipulation easier and more satisfactory. The drying of organic substances is very carefully discussed; anhydrous calcium sulphate is recommended for drying liquids, and E. L. Smith's ester hydrolysis method for drying ethyl alcohol.

The second half of the book deals with actual preparations, and here again the author adopts unusual, but excellent, tactics. He describes the determination of water in rectified spirit by the miscibility method and suggests alternatives to the hackneyed preparations on which most of us were brought up.

The writer recommends all chemists who work at benches to read this book, though more senior ones must be indulgent. If our Ph.D. "orals" were tests of what a candidate knew of laboratory technique and not merely of a highly specialised piece of work, this book might be taken as a good example of "What a Young Chemist should Know."

E. E. Turner.

Sterols and Related Compounds. By E. Friedmann, M.D., Ph.D. [Pp. vii + 100.] (Cambridge: W. Heffer & Sons, Ltd., 1937. 7*s*. 6*d*. net.)

This is a reprint of three lectures given by Prof. Friedmann, surveying the present state of our knowledge of the chemistry and biological properties of the sterols, bile acids, heart poisons, saponins, vitamin D, sex hormones and carcinogenic compounds. Prof. Friedmann makes no claim to have produced an exhaustive treatise, but, in the words of Sir F. Gowland Hopkins, who contributes a foreword, "to those who with small leisure realise the necessity of becoming familiar with the essentials of the subject, this informative volume will prove most valuable." The tremendous and fundamental advances made in this field of organic chemistry in recent years, and the "astonishing variety of tasks Nature can fulfil with sterols and their derivatives" are vividly portrayed in an attractive style, and only an occasional use of an un-English expression betrays the fact that the author is not using his native language. The more salient features of the substances under review are collated from a vast literature, and although there are one or two misprints and a few inaccurate statements, such as the attribution to Diels of the production of chrysene by selenium dehydrogenation of cholesterol at 400°, and the assertion that both stereoisomeric œstradiols have a higher œstrogenic activity than œstrone, in its essentials the information given is reliable and up-to-date. The comprehensive knowledge which has now been gained of the *chemical* relationships of the extraordinary diversity of natural products containing the carbon skeleton of the sterols perhaps tends to

obscure our ignorance of their *biological* origin and transformations, and yet serves to delineate the biochemical problems which still await solution. It is in this realm that the important advances of the future are to be anticipated.

J. W. C.

The Biochemistry of Cellulose, the Polyuronides, Lignin, etc. By A. G. NORMAN, Ph.D., D.Sc. [Pp. viii + 232, with 13 figures.] (Oxford: at the Clarendon Press; London: Humphrey Milford, 1937. 15*s*. net.)

THERE has been, in the past, a tendency to regard cotton cellulose as the prototype of all plant cellulose and to expect in consequence that wood and all other plant materials should yield on appropriate treatment a product closely approximating to or identical with this ideal. This misconception is due to a failure to recognise the almost unique position of cotton cellulose in occurring naturally in the seed hair as a practically pure chemical, and the author rightly points out that it is unjustifiable to use cotton seed hair cellulose as a standard for comparison of structural cell-wall cellulose; in the cotton plant itself the structural cellulose differs from the pure cellulose of the seed hairs being intimately associated with another polysaccharide xylan. For such associated polysaccharides, which may be xylan or in some cases mannan, the author in conjunction with Hawley suggested, some years ago, the term Cellulosan. The amount of cellulosan in structural celluloses from different sources may be as high as 20–30 per cent., but is considerably less in high-quality fibres such as flax, hemp or ramie. The recognition of such cellulosans as essential constituents of structural celluloses explains many difficulties which stood in the way of interpreting experimental findings. A further branch of the subject in which much confusion has hitherto existed has also been elucidated by the work of the present author and others; Schultze, who introduced the term Hemicellulose in 1892 for a group of substances extractable from cell-wall material by alkali, regarded these as true hexosans or pentosans or more commonly as hexo-pentosans; it is now known that they are in reality polyuronides. Dr. Norman's many contributions to the biochemistry of the cell-wall constituents have gained for him the reputation of one who has a systematic and orderly mind, and the clear and logical style of the present volume affords further ample proof of this. There are eight chapters with the following headings respectively—Cellulose, Polyuronide Hemicelluloses, Pentosans Hexosan and Hexopentosans, Pectin, Gums Mucilages and Gel-forming substances, Lignin, Metabolism of Plant Cell-wall constituents, Microbial Polysaccharides. Any one of these chapters may be read with profit and the feeling that light has been brought into dark corners and all who are interested in the biochemistry and composition of the plant cell-wall will be grateful to the author for the clear account he has provided of our present knowledge of an intricate subject.

P. H.

The Chemistry and Technology of Rubber. Edited by C. C. DAVIS and J. T. BLAKE. [Pp. ii + 941, with 109 figures.] (New York: Reinhold Publishing Corporation; London: Chapman & Hall, Ltd., 1937. 75*s*. net.)

THIS publication is one (No. 74) of the Scientific and Technological Monographs on chemical subjects which the American Chemical Society undertook to produce. The Society arranged for the publications of the series

by the Chemical Catalog Company Inc., of which the Reinhold Publishing Corporation is the successor.

The object of the publication is to systematise and co-ordinate the important facts of rubber chemistry, to discuss critically such knowledge in the light of both theory and practice, to call the attention of research workers to original sources of information, to stimulate their work and also to supply non-rubber chemists with authoritative information on every branch of rubber chemistry and technology. In the past, it is suggested, books on rubber chemistry and technology have failed because no one rubber chemist has been able to write authoritatively and critically upon every phase, but to the present publication of twenty-five chapters thirty-nine authors have contributed, all of whom are eminent in their particular section.

The amount of overlapping is small considering the inter-relationship of certain chapters contributed by different authors, and detracts in no way from the value of the book, as the authors have tended to present the same facts from a different aspect.

The book deals first with the chemical and physical properties of raw rubber and the rubber hydrocarbon. Theories of vulcanisation are discussed together with the action of fillers, reinforcing agents, accelerators and anti-oxidants and the theories of their respective influences. Vulcanised rubber is dealt with in detail and is followed by chapters on the deterioration of rubber by heat, light and ozone and on the electrical behaviour of rubber. Latex and its industrial application, gutta percha and balata, reclaimed rubber and substitute rubbers, form the subject-matter of other chapters.

This is an authoritative and comprehensive publication which will be needed by every research and technical chemist interested in rubber, and which may be given an unqualified recommendation.

T. J. D.

Food Preparation. By MARION D. SWEETMAN. Second edition. [Pp. xii + 449, with 54 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1937. 15*s*. net.)

THE first impression of the title Food Preparation leads one to expect the contents to be the processes of manufacture of foodstuffs, either from a commercial or household standpoint. Instead of this the main purpose is to supply the fundamental knowledge of the composition of foods, which includes a concise description of the vitamin content of foods and their effect on the human system; the important effects of the five senses—tasting, smelling, hearing, seeing and feeling—on the preparation and intake of food. Definitions are given of the various terms used in the preparation and manufacture of foods. In a chapter on the structure of foods, useful information on dispersions and their types is included.

As the book is of American origin, there is a wider source of information given of the cereals, cooked and uncooked, with comparisons from the nutrition standpoint, than is generally given in an English book. Two complete chapters have been devoted to the subject of fruits and vegetables, which describe the composition, appraisal as foods, cooking and preservation by means of canning, drying, fermentation and in the case of fruits also with sugar.

Milk and its various forms, skimmed, pasteurised, condensed, evaporated and malted milk, milk products—butter and cheese—and their process of manufacture are briefly outlined. Other subjects dealt with are eggs, fats

and oils, sugars, meat and flour mixtures. The chapter on the last subject named clearly indicates the book has been compiled for the use of students taking the higher courses in domestic science, and also that the products made in America differ somewhat from those made in England.

There is much subject-matter included in the book which will be of value to the vocational student and chemist interested in foodstuffs, and it can be recommended as a text-book for Domestic Science Training Colleges and a reference book for private and public libraries.

MARION CHADWICK.

The Art of Carburation in Theory and Practice, including Fuel Distribution in Manifolds, for Designers and Engineers. By R. W. A. BREWER, F.S.E., A.M.I.C.E., M.I.A.E. [Pp. xii + 176, with 151 figures.] (London: The Technical Press, Ltd., 1937. 21*s*. net.)

COMPREHENSIVE literature on this all-important subject is rare, although much scattered information appears in a variety of technical journals.

The author is to be congratulated on his lucid and thorough exposition of a subject of the greatest importance to designers and research engineers.

First principles are dealt with in a logical and easily comprehended manner. Physical and hydro-dynamical data, together with their applications to the subject, are abundant. Much useful information on the general nature of motor fuels is available also, though, from the petroleum technologist's point of view, this is not particularly extensive or up-to-date. This is a minor criticism and is possibly inapplicable to a work dealing with general principles.

The historical section, dealing with carburettor types, should be brought up-to-date, since it appears to end *circa* 1925.

Inlet manifolds are discussed at some length and the author has obviously made a profound study of the subject.

This is a book that should certainly find a place in every well-equipped design department.

L. G. C.

Practical Agricultural Chemistry. By S. J. M. AULD, D.Sc.(Lond.), Ph.D. (Würzburg), F.I.C., F.C.S., and D. R. EDWARDES-KER, O.B.E., M.A.(Oxon.), B.Sc.(Lond.). [Pp. xxiv + 246, with 32 figures, including 4 plates.] (London: John Murray, 1937. 5*s*. net.)

THIS is a reprint of the second edition of this well-known and popular book at a price which should bring it within reach of all who are interested in the subject. As the text has not been altered in any respect since 1921 it naturally is lacking in any of the more modern developments, but in spite of this the book contains much valuable instruction and is fundamentally sound.

P. H.

Modern Glass Working and Laboratory Technique. By M. C. NOKES, M.C., M.A., B.Sc.(Oxon.). [Pp. xiii + 153, with 84 figures.] (London: William Heinemann, Ltd., 1937. 7*s*. 6*d*. net.)

INCREASE in the importance of glass-blowing and allied technique, not only to the chemist but increasingly to the physicist, renders the appearance of this book most opportune.

The fundamental operations of blowing and working glass, and the repair

and construction of soft glass apparatus, are adequately described in the first four chapters. But the real novelty of the book lies in the chapters that follow. These describe for the information of science teachers and students the handling of Monax and Pyrex glass, the correct methods for making metal-to-glass seals, the assembly of vacuum tubes and photo-cells. The book closes with a valuable section on vacuum technique.

One of the most useful services this book can render is to bring home to the average non-professional glass-blower, who has to make his own apparatus, the fact that it is far easier to make an elaborate piece of glass apparatus out of Monax or Pyrex than out of soft glass, as the liability to crack on cooling is infinitely less with these glasses.

The best test of a book of this nature is a practical one. To make it, a non-expert glass-blower who had never blown Pyrex glass before was asked to make from instructions in the book a Pyrex all-glass condenser, a soft glass vacuum tube and a Monax glass vacuum tube, both the latter with proper aluminium electrodes. It says much for the clarity and accuracy of the instructions that these three operations were performed at the first attempt without any hitch.

It is quite certain that this book should be available in every laboratory in which amateur glass-blowers have occasion to make any glass apparatus for themselves.

In a future edition instructions for making and using a mercury still would probably be welcomed.

W. H. B.

GEOLOGY

The Age of the Earth. By Arthur Holmes, D.Sc., A.R.C.S., M.R.I.A., F.G.S. Nelson Classics. [Pp. 263.] (London: Thomas Nelson & Sons, Ltd., 1937. 1*s*. 6*d*. net.)

The second edition of this little book succeeds in compressing into its 263 pages a surprising amount of informative detail on a subject on which a great deal of painstaking work has been done during the last quarter-century. It is not altogether clear what amount of geological knowledge is demanded of the reader, but with the exception of the mathematical discussions on pp. 148–9, and in places the somewhat free use of petrological terms, the book is one which might be read with interest and advantage by extra-mural students of general science.

After setting out the problem to be dealt with, a clear exposition is given of methods now discarded in the light of later knowledge, and this is followed by descriptions of the newer criteria such as the helium and the lead methods. Cycles and revolutions in the history of the earth are dealt with in Chap. 6, the work concluding with the evidence of meteorites and the cosmic time scale. A most useful glossary of technical terms assists the non-technical reader to follow the more abstruse pieces of reasoning.

It is by no means easy to present in so lucid a manner the results of a mass of long and painstaking work from many countries, and quite apart from its undoubted value to non-specialist minds, the book can be thoroughly recommended to more advanced students as a most admirable survey of a difficult subject.

B. H. Knight.

Les Ressources minérales de la France d'outre-Mer. Tome V : Le Pétrole. [Pp. iv + 263, with 37 figures, including 3 plates.] (Paris : Société d'Éditions Géographiques, Maritimes et Coloniales, 1937. Frs. 45.–.)

THIS is the fifth and last volume of *Ressources* in the fine series of works on the mineral deposits of French overseas possessions which has of late years been published by the Bureau d'Études Géologiques et Minières Coloniales. The book is divided into ten chapters. The first two, by H. de Cizancourt, on the general characters of petroliferous deposits and on the general principles of research and prospecting for oil respectively, form a valuable short treatise (79 pp.) on the geology of petroleum which has been expressly written for the interested technical and colonial public. Chap. III, by L. Migaux, deals with petroleum in Morocco (40 pp.), the richest French oil field. Chaps. IV, V, and VI, all by H. de Cizancourt, deal respectively with Algeria and Tunis (39 pp.), French Equatorial Africa (13 pp.) and Madagascar (16 pp.). In Chap. VII, L. Dubertret writes on prospecting for oil in Syria (18 pp.); and in Chap. VIII F. Blondel deals with petroleum resources in the remaining French possessions (3 pp.), including Indo-China, New Caledonia, French West Africa, etc.

Although Iraq is not a French possession its proximity to Syria, and its value to France as a source of oil supply, have necessitated a chapter (IX) on petroleum in Iraq, which has been entrusted to H. de Cizancourt (11 pp.). Chap. X, by J. Filhol, which completes the work, deals with the position of France in relation to the world oil market (18 pp.). We learn that in 1934 France imported 6,159,672 tons of petroleum, of which the United States and Latin America each supplied about 30 per cent., Russia and Roumania 20 per cent., Persia and the Dutch East Indies 15 per cent., and other sources 5 per cent.

The work is well illustrated by maps and sections, is excellently printed and produced, and, in its writing, continues the tradition of terse clarity established by the earlier volumes of the series.

G. W. T.

PEDOLOGY

The Cycle of Weathering. By B. B. POLYNOV, D.Sc. Translated by A. MUIR. [Pp. xii + 220, with 4 figures.] (London : Thomas Murby & Co., 1937. 10*s*. 6*d*. net.)

THIS book is apparently the forerunner of another which is to deal with Soils and their formation. In this volume the author takes a broad view of the earth's crust—solid, liquid and gaseous—as the arena of two complementary sets of processes. The exothermic processes tend to the production of the substances which are chemically most inert and physically least highly dispersed, while the endothermic processes, in direct opposition, bring about the accumulation of energy. Thus sulphur is considered as being brought during volcanic activity to the earth's surface, there passing rapidly from vapour through liquid to solid and then less rapidly to oxide and sulphate, there reaching its lowest energy level. It is then considered, although a little less convincingly, that these compounds become buried, acquire new forms of energy and return to the surface where the energy is again released.

The author presents the processes in the first few pages as cycloid rather than cyclic, that is to say the cycles themselves are conceived as changing

in long periods of time. The changes are not changes merely represented by a circle for ever turning but by "a point on the circumference of a circle rolling in a straight line." The comparison of the atmospheric and surface composition of the earth with that of other planets convinces the author that the earth is only at one stage in a general transformation.

It is within this stage, however, that the interest for most readers lies and the changes within the present sphere of weathering, as it concerns us in a lifetime, are the main considerations of this book. The various elements are taken in turn and cyclic changes as determined by their association with living organisms, formation of colloidal compounds, power to be absorbed and so forth, are described. In a final chapter these changes are surveyed as a whole.

An appendix gives a concise account, with appropriate references, of present views of the structure of silicates.

Some amount of surmise is inevitable in a treatment of this kind, but its philosophical basis is very intriguing and its argument is very stimulating. It is a very welcome contribution to the literature of pedology and those who read it will hope for the following volume at an early date.

N. M. C.

BOTANY

Plant Ecology. By Hilda Drabble. [Pp. 142, with 12 plates.] (London: Edward Arnold & Co., 1937. 7*s*. 6*d*. net.)

This book is primarily a text-book and it aims at giving, firstly, a straightforward and simple description of British vegetation and, secondly, some idea of the scope of plant ecology in Britain. A book of this type has long been badly needed and there is no doubt that the present volume will serve a very useful purpose. As this indication of its character shows, it is not written primarily for the specialist, but rather for the student and general reader.

The book is divided into two parts. The first of these deals, on somewhat conventional lines, with the general properties of plants as individuals and of their habitats. This section concludes by introducing a few necessary ecological terms and concepts, though these are reduced to the minimum, a very useful feature. The second part deals with British vegetation and with the factors associated with the distribution of different vegetation types. The plant communities considered cover most of those characteristic of this country, particular emphasis being laid on woodlands (four types), heaths, moorlands, downs and grasslands, while the vegetation of fresh water, of the sea shore, of mountains and of cultivated land is treated rather more generally. The descriptions are always adequate. This part of the book also includes a useful chapter describing and illustrating plant succession. It possesses otherwise three attributes of value. It is not overburdened with long lists of species; no attempt is made to deal with the nomenclature of plant communities, the bane of much ecological writing to the general reader; and, finally, the descriptions include some admirable illustrations, these being from photographs by Prof. E. J. Salisbury.

Apart from its value as an aid to students, the chief virtue of this work may prove to be that it may bring to a point the differing opinions on what should be included in the elementary study of plant ecology, a subject too extensive for a discussion in a short review. The purely descriptive method

of treatment adopted in this book has much to recommend it, particularly because it possesses the merit of simplicity. But simplification may lead to omissions or condensation which will not always meet agreement. In the present instance, we may note the absence of any reference to the *Agrostis-Festuca* type of siliceous grassland, a type more extensive in Northern and Western Britain than some of those chosen for description.

The list of references given in conclusion is useful and on the whole representative. Crampton's *Vegetation of Caithness* and Moss's *Vegetation of the Peak District* have had much influence on ecology in Britain and seem to have stronger claims for inclusion in the literature list than some of the more specialised papers included.

W. H. P.

Economic Botany. By A. F. Hill. McGraw-Hill Publications in the Agricultural and Botanical Series. [Pp. x + 592, with 225 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1937. 24*s*. net.)

The literature of economic botany is widespread through a great variety of journals and comprises so extensive a field, whether we approach the subject from the botanical or technicological side, that text-books in this branch of knowledge are as essential as they are rare.

The subject-matter dealt with in this book is classified according to the nature of the plant products. The successive chapters in the first section deal with: Fibres and Fibre plants, Forest products, Tanning and dying materials, Rubber and other latex products, Gums and resins, Essential oils, Fatty oils and waxes, Sugars, starches and cellulose products. The second section treats of Drug plants and drugs, a third with Food plants, in which the chapters are allocated to the history and nature of food plants, the Cereals, Legumes and nuts, Vegetables, and Fruits. Two chapters classed as food adjuncts concern spices, flavouring materials and beverages. There is also a systematic list of the species cited and a brief bibliography of 160 reference works.

Most of the illustrations are reproductions from photographs of the plants or their parts and in many instances occupy from a third to a half of a page. When allowance is made for these, it will be realised that with so wide a scope the information accorded is necessarily often very brief and sometimes meagre. Indeed, one cannot but feel that, whilst these pages contain much that is of interest, the attempt to cover so wide a field in so comparatively short a space has resulted in a not infrequent superficiality of treatment that detracts from an otherwise admirable production.

E. J. S.

BIOLOGY

Biology for Students of Pharmacy. By E. J. Moore, M.C., M.Sc. [Pp. viii + 415, with 204 figures, including 8 plates.] (London: Edward Arnold & Co., 1937. 15*s*. net.)

This book has been published to meet the altered needs of the student working for the Preliminary Scientific Examination of the Pharmaceutical Society, now that he is called upon to study zoology in addition to botany in his first year. This change is obviously a very desirable one, since the pharmacist now has to an increasing extent to deal with substances of animal

origin, such as insulin and other endocrine compounds, while the use of drugs of plant origin is on the decline.

Mr. Moore's aim is to cove: the whole of the ground required in one volume. The first half is devoted to botany, and in this he gives a straightforward account of his subject. This part of the book is clear and accurate; the diagrams are good, though some are rather small.

The zoological section which occupies the latter half of the book is less satisfactory. The author seems to have kept the rather confined syllabus too much in mind, with the result that the text amounts to the mere description of the types to be studied. Thus there is too little reference to function, adaptation or to comparative anatomy. Without these zoology becomes a dead subject. Further, it is surprising that in the chapter on the Malarial Parasite there is no mention of quinine, though one would have thought that reference to treatment by drugs would have been of special interest to the budding pharmacist. A number of errors have been noted, *e.g.* the account of the copulation of the earthworm is inaccurate (p. 293); the bones of the rabbit's skull do not *articulate* in sutures (p. 311); the text (p. 357) suggests that the young liver-fluke passes from the intestine to the liver of the new host along the bile-duct instead of by way of the blood stream; and the term "Protista" is used in a very unusual way (p. 274). It is to be feared that the student will not get the zoological knowledge which will stand him in good stead in his examination, as the book stands at present.

C. C. HENTSCHEL.

Ecological Animal Geography. Authorised, rewritten edition based on R. Hesse's *Tiergeographie auf oekologischer Grundlage*. Prepared by W. C. ALLEE and K. P. SCHMIDT. [Pp. xiv + 597, with 135 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1937. 30*s*. net.)

AT last we have an English version of Richard Hesse's pioneer work. It will be acclaimed, not only by teachers of zoogeography, but by all those zoologists—and nowadays they are the majority—who are concerned to discover what determines an animal's ability to live in one environment rather than in another. Only those who have tried for themselves to extract from a scattered literature the relevant facts of ecology as these affect the world-distribution of animals can appreciate the task Hesse undertook and the debt they owe him for the unified and vivid book that in 1923 was the outcome of his labours.

Ecological and "historical" zoogeography must be considered as complementary aspects of the same subject. Animal ecology is a relatively new study, and it has found but little place in English text-books read by the geographer. Yet, as Hesse pointed out, "it bears the germs of a truly causal science," and so provides a necessary corrective to the dogmatism or the fancifulness indulged in by those who follow solely the older method of approach to the problems of present-day distribution.

This edition in English of Hesse's book is admittedly a very free translation. Moreover, with the author's full consent, Prof. Allee and his colleagues have rewritten certain portions, have done something to mitigate the severities of technicality, and have seriously attempted to bring the work up-to-date in a number of important respects. While scrapping what seemed obsolete, it is a pity that they did not go a little farther in some

directions. Nowadays it is impossible, for instance, to discuss the problem of reef corals and their symbiotes without any reference to Yonge's work on the Barrier Reef, for that upsets the theories in vogue when Hesse wrote.

But we are so grateful to the American translators for what they have achieved that it would be ungracious to dwell on minor defects. Here is a classic work made worthily available to the English student, and all university libraries should place it on their shelves.

D. L. M.

Oyster Biology and Oyster Culture. By J. H. Orton, D.Sc., F.L.S. Based on the Buckland Lectures for 1935. [Pp. 211, with 57 figures.] (London: Edward Arnold & Co., 1937. 5*s.* net.)

No animal which lives in the sea has proved a more fascinating subject of biological research than the oyster. From prehistoric times men of all races and climes have acclaimed its edible qualities. The Romans sought it as far afield as the coast of Kent and were also the pioneers of oyster culture. The increasing demands of the enlarged and wealthier population of the nineteenth century, by causing serious depletion of the prolific natural beds of Europe, led to the re-establishment of culture on a large scale, notably at Arcachon on the Biscay coast of France. To-day oysters of many different species are cultivated in Europe, America, Japan and Australia. The claims of a great industry have stimulated and financed research all over the world.

Prof. Orton has brought together in this delightfully produced little book information from many sources—not least his own extensive researches—on the biology and culture of oysters. The first half contains undoubtedly the best account yet published on the structure and natural history of *Ostrea edulis*, our own "native" oyster. The very numerous illustrations, taken from original papers from diverse sources, add greatly to its value. The only statement to which exception can be taken is that the oyster contains hæmocyanin in the blood. The work of Takatsuki does not support this. The fascinating subject of sex-change and spawning, to which the author has made such important contributions, is especially well treated and data are given for other species.

The concluding section of the book deals with the manifold problems of oyster culture. This will be of especial value to the oyster grower, but no biologist interested in the relations of the animal to the environment can fail to find much of compelling interest in the mass of information which the experience of the oyster grower and the researches of zoologists have collected on the biology of the oyster, the problems of cultivation, protection from enemies and unfavourable conditions, pollution and purification. In every way the most fitting praise for Prof. Orton's book is that Frank Buckland, who made its publication possible, would have been delighted with it.

C. M. Y.

The Nation's Sea-Fish Supply. Being the Buckland Lectures for 1936. By E. Ford, A.R.C.S. [Pp. 112, with frontispiece, 3 plates, and 8 figures.] (London: Edward Arnold & Co., 1937. 3*s.* 6*d.* net.)

Those acquainted with Mr. Ford's long devotion to herring problems may have been surprised by his choice of subject for the Buckland lectures of 1936, but no subject could have been more timely, and no competent writer could have undertaken it with a fuller sense of its importance to the fishing industry

and the nation. Seventy years ago the sea-fisheries of the country were liberated from a multitude of harassing State restrictions, and their colossal development during this period of freedom is known to all. Attempts to reintroduce restrictions have been rejected by Parliament time after time. Then suddenly during the industrial depression a few years ago all this was changed. The great post-war shrinkage of foreign markets, the high costs of production, and the competition of cheap tinned and other foods at home brought all branches of the fishing trade to an economic crisis. Over-production and the prospect of a permanently reduced demand pointed to the necessity of some form of re-organisation, and the trade, lacking initiative and in face of bankruptcy, capitulated to the idea of Governmental control. By the Sea Fishery Act of 1933 all British fishing outside territorial waters was brought under a system of State regulation, and our markets were closed to supplies of fish not conforming to State specifications.

This momentous change is the subject of Mr. Ford's first lecture. "We have broken with the past," he says, "in a tremendous way and now face the future with altered standards of fishing practice, in which State prescription takes the place of personal liberty. We see in this the beginning of a bold experiment, undertaken at a time when constructive action was imperative. It is an experiment in which every aid known to science and industry will be needed to ensure success." In his second and third lectures (or chapters) Mr. Ford deals fully and lucidly with the considerations which underlie two of the Ministry's first regulations, one standardising the size of mesh to be used in the fishermen's nets, the other prescribing the size-limits of the fish to be marketed. These reasons, based upon thirty years' continuous research and experimentation, must be admitted to be substantial, if not in every respect conclusive; but, with Parliament's acceptance of the method of experimental legislation, no one can complain if the responsible authorities cut some argumentative knots with the sharp sword of an Order in Council.

W. G.

Salmon Caught in the Sea—North-west Sutherland, 1936. By G. W. Hartley, B.Sc. Fisheries, Scotland, Salmon Fish., 1937, No. III. [Pp. 21, with 6 figures.] (Edinburgh: H.M. Stationery Office, 1937. 1*s*. net.)

Mr. Hartley was in charge of the Salmon Marking operations carried out by the Fishery Board for Scotland in 1936 at Loch Inchard just south of Cape Wrath, and which have already been reported upon by Mr. Menzies, the Inspector of Salmon Fisheries. Loch Inchard is a grilse fishery, and 80 per cent. of the fish marked were grilse, actually 1006 of the total of 1255.

This paper is a careful and detailed analysis of the fish, their age groups, calculated lengths, condition factors, monthly percentages, etc. The smolt migrations indicated on the scales are of some interest, 69 were two years old at time of first migration, 29 per cent. three years, and 2 per cent. four years. The last is an indication of the northerly region of early growth. The graphical analysis helps to demonstrate the heterogeneous origin of the fish dealt with. Scales showing absorption were infrequent, but the fishing ceased on August 11.

W. L. C.

The Surface Water Drift in the Northern and Middle Areas of the North Sea and in the Faroe-Shetland Channel. Part II, Section 3. By JOHN B. TAIT, B.Sc., Ph.D., F.R.S.E. Fisheries, Scotland, Sci. Invest., 1937, No. 1. [Pp. 60, with 49 figures.] (Edinburgh: H.M. Stationery Office, 1937. 3s. net.)

DURING the five years 1910–14 inclusive, the Fishery Board for Scotland carried out extensive series of experiments with floating bottles designed to drift with the upper waters of the sea. Each bottle was numbered and liberated, at a known time and place, out at sea. It contained a numbered card with directions in various languages asking that the card should be returned with a statement as to when and where it was found. Out of 4825 bottles liberated, 1096 cards were so returned. The plan of the experiments was a familiar one, but this particular series appears to have yielded more detailed information than any other.

The results of the experiments have been studied with untiring energy and great resource by Dr. Tait, who joined the staff of the Fishery Board after the war. His first report, issued in 1930, was a preliminary one on the whole series, and he has since issued three detailed reports dealing respectively with the experiments of the years 1910, 1911 and 1912. The report now under review is the third of these and its special title is, "A cartographical analysis of the results of surface drift-bottle experiments of the year 1912; with a discussion on some hydrographical and biological implications of the drift-bottle results of 1910, 1911 and 1912, including statement of a theory of the upper water circulation of the northern and middle North Sea."

One of the principal difficulties of the analysis is that the details of the system of surface drift-currents change from month to month and are not the same during corresponding months in different years. But Dr. Tait has produced a very interesting chart which gives his conclusions as to the general pattern of the non-tidal movement of surface waters. The variations which occur are in the positions and sizes of the various elements of the pattern and in the speed of the circulation. One of the chief features of this pattern is that water from the Atlantic enters the North Sea by way of the channel between the Faroe and Shetland Islands and then through the western part of the broad channel between the Shetlands and Norway. Contrary to what had previously been widely supposed, practically no water enters the North Sea through the passages between the Shetlands and the mainland of Scotland. The chart shows a large counter-clockwise eddy occupying all the central portion of the northern part of the North Sea, and a number of smaller eddies spread over the whole region of the North Sea north of latitude 54°, and of the Faroe–Shetland waters. Between these eddies there is a system of fairly narrow streams, each consisting of water moving more rapidly than that on either side.

J. P.

A Monograph of the Acanthodriline Earthworms of South Africa. By G. E. PICKFORD, Ph.D. [Pp. 612, with 583 figures.] (Cambridge: W. Heffer & Sons, Ltd., 1937. 25s. net.)

THE Acanthodrilinæ of South Africa are an ancient and probably relict element of the earthworm fauna, and Dr. Pickford has investigated them from a variety of aspects, the morphological and taxonomic, the evolutionary, the distributional and the geological. Certain interesting and curious conclusions emerge. In the matter of their classification, true relationships

are extremely difficult to establish, because the meristic changes and the gradual acquisition and reduction of organs, which constitute the material of the systematist, manifest themselves again and again in wholly unrelated forms. These evolutionary trends are apt to obscure natural relationships, which can, therefore, only be inferred by taking into account the total number of characters common to the various species, on the principle that species having many characters in common are more likely to be related than those that have only a few.

The distribution of these earthworms in South Africa depends upon the average annual rainfall, and areas in which the rainfall is below 20 inches a year form an effective barrier against their dispersion. Cyclical climatic changes, such as are known to have occurred in Quaternary times, accompanied by alternating wet and dry periods and by the consequent expansion and contraction of the Acanthodriline population, have brought about geographical isolation, an important factor in the formation of species. Dr. Pickford believes that the specific and varietal characters are devoid of adaptive significance and finds that the evidence supplied by her studies supports the conclusions of S. Wright. She holds with Wright that both inbreeding in small isolated populations and severe selection in large populations lead to fixation of type and render the population unresponsive to environmental changes, since adaptive changes must wait upon the rare occurrence of favourable mutations. The best conditions for evolutionary change are those to be found either in a medium-sized population mutating in a random and non-adaptive manner but without fixation of type, or in a large population divided into partially isolated sub-groups where rapid inbreeding may lead to the temporary stabilisation of different types without the sacrifice of the pliability of the species as a whole.

Among the Acanthodrilines there are species that inhabit geographically restricted areas and these have relatively constant non-adaptive characters. This may well be the result of close inbreeding. Others have both a fairly wide range and a more or less fixed type, and these have probably had a similar history but have undergone a rapid expansion in response to an increased rainfall. Others again are split up into a number of partially isolated geographical races, presumably the result of local inbreeding.

Dr. Pickford believes that the ancestors of the present Acanthodrilines were brought to South Africa in the Pleistocene by the West Wind Drift, probably from South America, and devotes a chapter to their distribution in relation to geological history. In the systematic section she describes 85 species and a number of sub-species and races, of which a large percentage are new.

Many of her conclusions are rather speculative and open to question, but her monograph is undoubtedly a genuine and important contribution to our knowledge of the group. The proof-reading leaves something to be desired.

C. C. A. M.

Invisible Radiations of Organisms. By Otto Rahn. With an Introduction to the Physics of Radiation, by Sidney W. Barnes. Protoplasma-Monographien, Vol. 9. [Pp. x + 215, with 52 figures.] (Berlin: Gebrüder Borntraeger, 1936. RM.13.20, bound.)

Few questions in biology have excited more controversy than that of mitogenetic radiation, and few can have aroused less interest in this country,

Yet upwards of 600 publications dealing with this subject have appeared since Gurwitsch claimed, in 1923, to have demonstrated the emission from onion roots of radiation which stimulated cell division in other onion roots. A very considerable mass of data has now accumulated which, according to believers in mitogenetic rays, indicates that chemical reactions and living cells emit ultra-violet radiation of wave-lengths between 1900 Å. and 2500 Å., and of very low intensity, and that this radiation brings about an increase in the rate of cell division of living material on which it falls.

Prof. Rahn is a firm believer in the existence of mitogenetic radiation, and in this book he gives a clear statement of the work which has been done on these alleged invisible radiations of organisms. The book opens with a clearly stated summary of the physics of radiation by Dr. S. W. Barnes which will be found very useful for the biologist who is not equipped with a deep knowledge of physics ; if anything, this account perhaps is a trifle too condensed. After a chapter on the sources of radiant energy there follow chapters on the effect of ultra-violet radiation upon cells, methods of observing biological radiation, special characteristics of biological radiations, analysis of the mitogenetic effect, and the significance of biological radiations in biology, medicine and agriculture.

Although the author is definitely putting forward the case for the existence of mitogenetic radiation he states the case very fairly and does not attempt to hide the fact that many workers have failed to obtain the mitogenetic effect, and that, indeed, even the most ardent supporters have sometimes experienced failure in this respect. It is obvious, as the author realises, that in such cases there must be some unknown condition in the experiments.

The book can be recommended to those who want to have a clear account of the evidence for the existence and nature of these alleged invisible radiations from living cells.

W. S.

An Experimental Study of the Problem of Mitogenetic Radiation. By Alexander Hollaender and Walter D. Claus. Bulletin of the National Research Council, No. 100. [Pp. 96, with 20 figures, including 4 plates.] (Washington, D.C. : National Research Council of the National Academy of Sciences, 1937. $1.00.)

This bulletin contains an account of experimental work carried out with the greatest care, all possible precautions being taken against disturbing effects. The authors lay down certain requirements which they consider such work should fulfil. These are, that the experiments should give the possibility of clear-cut results, that they should be arranged so that duplicates and triplicates can be maintained, and so well controlled that repetition would permit an interchange of controls, that they should yield results capable of statistical treatment, and that they should yield the data required by other workers wishing to repeat the work.

Working on these lines over a period of two years the authors were unable to detect any mitogenetic radiation either by the use of biological material as detectors, or by the sensitive physical detectors of such radiation known as photon counters. Having regard to their own findings and to the fact that the detection of mitogenetic radiation is not always repeatable, even by those investigators who have adduced evidence for such radition, the authors conclude that proof of the existence of mitogenetic rays does not rest on a scientific basis.

The bulletin will be read with great interest by all those interested in the problem of possible ultra-violet radiation by living organisms.

W. S.

Evolution and its Modern Critics. By A. Morley Davies, D.Sc. [Pp. xii + 277, with 30 figures, including 4 plates.] (London : Thomas Murby & Co., 1937. 7*s*. 6*d*. net.)

In this delightful little book Dr. Davies accepts the objections to Evolution enunciated by several modern writers as having been put forward quite seriously and thereupon proceeds to discuss them in a sympathetic but judicial spirit. To some this may seem a waste of time ; but one of the strange developments of recent years has been the rapid increase in that section of the general public that owns allegiance to the creational view of the origin of living things. This body of people will only be strengthened in their conviction if such objections to the doctrine of evolution be allowed to go unchallenged.

The serious student will nevertheless find here something more than the mere refutation of an already discredited position ; for the author by a skilful historic treatment, in which he makes free use of fresh quotations, traces the development of evolutionary ideas, and thus lays for the student the foundations of a sound appreciation of the problems as they present themselves to the modern worker.

Naturally, in view of the author's special studies, the evidence is drawn mainly from palæontological sources. This is an excellent fault in these days when there are so many who seem to think that evolutionary problems can be solved only by experimental methods. Nevertheless, the author has not confined his attention exclusively to fossils, but has made some very illuminating excursions into the realm of experimental biology. His illustrative examples, like his quotations, are off the beaten track and are correspondingly more valuable.

The book is of handy size, with numerous well-drawn illustrations. The text is subdivided in a way that leads the eye at once to the themes that are being discussed. Whilst every student of biology and geology should possess a copy of this book, it is worthy of perusal by any person well educated enough to be interested in evolutionary problems. The latter will find his path made easy by a useful glossary of terms at the end of the book.

H. H. S.

Laboratory Studies in Comparative Anatomy. By W. C. Senning, Ph.D. McGraw-Hill Publications in the Zoological Sciences. [Pp. x + 188, with 15 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1937. 10*s*. 6*d*. net.)

This is a practical handbook designed for the guidance of students of vertebrate zoology in their laboratory work. The arrangement and plan of the series of studies bear the stamp of an experienced teacher who not only knows the art and value of stimulating his students to independent observation, but believes, and we fully agree with him, " that more valuable training is to be gained by careful work upon a few, rather than superficial work upon a large number of animals." For this reason, directions are given for the study of the form and structure of only three types, the Shark, Necturus and the Cat. The presentation of this study, however, is so arranged that

in his dissections the student is simultaneously confronted with the same system of each type. For example, the skull of the fish, the amphibian and the mammal are examined seriatim, and their similarities and contrasts noted. Thus the student quickly grasps the significance and importance of phylogeny.

Throughout the studies the bearing of embryology upon definitive structure is frequently emphasised, and short accounts of development are given in their appropriate setting. To quote the author, "The parts of the brain and their arrangement are better understood when viewed from the standpoint of development." The wisdom of these words is characteristic of the plan of the whole book.

A collection of outline drawings is bound in a separate cover as a companion to the dissecting manual, and this is a very valuable part of the work. The author, like other teachers of experience, is convinced of the value of the student making his own drawings as records of his own dissections. The outlines, however, save the student's time, and he is expected, by his own observation and industry, to fill in the necessary detail. The series of drawings is by no means exhaustive, but serves to introduce both teacher and pupil to a method which can be extended as required.

It is regrettable that, in a book of such excellence, we encounter so much spelling and terminology which is not in harmony with that used in this country. Examples of this are found in the following: "pedicel," "sclerotome," "ectal," "ental," "mezal." This cannot but lead to confusion in the mind of the student who is already becoming familiar with other terms expressing the same ideas. This difficulty could be overcome by an English edition, or, better still, we may hope that a second edition, when it is called for, will find biologists in both English-speaking countries conforming to type in their terminology as well as in their anatomy.

In spite of this minor defect, we believe Dr. Senning's manual will find a warm welcome in this country.

J. K.

MEDICINE

Medico-Legal Aspects of the Ruxton Case. By JOHN GLAISTER, M.D., D.Sc., and JAMES COUPER BRASH, M.A., M.D., F.R.C.S., Ed. [Pp. xvi + 284, with 172 figures.] (Edinburgh: E. & S. Livingstone, 1937. 21*s.* net.)

The Medico-Legal Aspect of the Ruxton Case—as presented by Prof. Glaister and Dr. James Couper Brash—is probably without parallel in criminal records. This publication has been undertaken in response to numerous requests from members of medical and legal professions and from officers of police forces at home and abroad. The enquiry commenced with the discovery of human remains in a ravine below the Gardenholme Bridge on the Moffat–Edinburgh road. The collection of numerous remains, some wrapped in newspaper, the date of which offered useful clues, and infected with maggots, were soon found to belong to at least two bodies. It was thought advisable to remove them to Edinburgh University, where the identifying of the bodies was carried out.

The mutilation was very comprehensive and had evidently been performed by someone with a knowledge of anatomy, and moreover its purposive nature suggested medical as well as anatomical knowledge. The ears and nose, eyelids and eyes, lips and also teeth had been removed from the heads during life. The mammæ and flesh from abdomen No. 2 and external genital organs

had been removed, but it was found impossible to be certain that the uterus and appendages belonged to body No. 2. The trunk of body No. 1 was not recovered. Body No. 2 was shown to be that of a female, who was not pregnant, but had borne children. Mutilation of body No. 2 might have been prompted by the idea of preventing identification from finger-prints. The fingers of body No. 1 were intact—? due to lack of time. Mutilation of body No. 1 was not so extensive as that of body No. 2. The mutilation of the left foot of body No. 2 was purposive, though it only served to call attention to the fact that the foot had some peculiarity which it was desired to conceal. Indeed, the thoroughness of the removal of identifying features was of considerable importance in assisting identification.

All the amputations had been performed with a knife, and removed parts included not only features that would have helped identification, but also that which might have disclosed marks of violence or signs of the cause of death.

The maggots were larvæ of *Calliphora erythrocephala*, which need ten to twelve days to hatch out. This was compatible with the remains having been deposited in the ravine about 12 to 20 days before their examination on October 1, and fitted the hypothesis that the parts of the bodies had probably been placed in the ravine during the early hours of the morning of September 16.

Lastly, there was found another most remarkable specimen of a "cyclops eye" belonging to some animal monster, but its origin and real nature were never properly worked through and did not appear to have much bearing on the case.

It may be mentioned that blood testing by blood groups was not undertaken, though this conceivably might have separated Dr. Ruxton's blood from that of the two women and from themselves; but the blood was so widely and irregularly distributed it would seem unlikely that any clear-cut, useful answer would have been obtainable. Apart from this the incriminating evidence was now overwhelming.

The summing up took four hours forty minutes. The Judge stressed the blood-stains on the carpet and especially the stair pads and the condition of the bathroom. He attached no significant relation of the bloodstains to Mrs. Ruxton's miscarriage in 1932. The suggestion by Dr. Ruxton to the charwoman that he would strip the wallpaper from the top staircase himself in his spare time was remarkable for a man with a cut hand. But it was strange that a man with an injured hand had taken to getting up a carpet on a Sunday morning. That the injury to the hand was said to be done with a tin-opener which he had thrown away seemed a very unlikely story. He expressed his opinion of the care and skill and absence of prejudice which had been observed, especially in the fitting of the fragments and reconstruction of the bodies.

P. J.

Perspectives in Biochemistry. Thirty-one Essays presented to Sir Frederick Gowland Hopkins by Past and Present Members of his Laboratory. Edited by J. Needham and D. E. Green. [Pp. x + 361, with 6 plates and other figures.] (Cambridge: at the University Press, 1937. 15*s*. net.)

The conventional Festschrift, composed as it so often is of *ad hoc* contributions of relatively trivial nature, tends to be a nuisance to editors of scientific journals and a weariness to the general reader.

Drs. Needham and Green have conceived a much better idea and one more worthy of the genius of the man whom they desire to honour, namely the collection from his past and present pupils of short essay reviews of subjects in which these individual workers are interested. The result of the editors' labours is the volume under review, which contains thirty-one contributions from past and present members of the laboratory of Sir Frederick Gowland Hopkins.

The plan adopted by the editors not only has the great advantage of excluding routine descriptions of experimental investigations which are out of place in a volume of this kind; it also makes it possible to allow contributors much greater latitude in speculation than is permissible in the modern scientific journals. Both these advantages have been fully exploited and with few exceptions the essays are characterised by a liveliness and an individuality of view which make stimulating and attractive reading.

It is a fitting tribute to the life work of the greatest of the British biochemists that this volume of contributions from his past and present co-workers should cover so wide a variety of subjects. In a review of this length mention of individual articles is precluded, but it should be pointed out that the group of essays dealing with the dynamic aspects of biochemistry, which have always been Sir Frederick Hopkins' own main interest, constitutes an excellent and complete review of the present position; nor are the incursions of biochemistry into other biological sciences and into medicine neglected.

The editors have earned a debt of gratitude from all biochemists in having produced a volume of permanent value and interest, which is at the same time a reflection of the inspiration of a stimulating mind and a tribute of affection to the universally esteemed leader of their science.

C. R. Harington.

Reading, Writing and Speech Problems in Children. By Samuel Torrey Orton, M.D. Thomas W. Salmon Memorial Lectures, Vol. III. [Pp. 215, with 24 figures.] (London: Chapman & Hall, Ltd., 1937. 10*s*. 6*d*. net.)

While Professor of Neurology and Neuropathology at Columbia University, Dr. Orton devoted special attention to the study of language disorders, particularly in children. His work originated in the problems encountered among schoolchildren by a "mobile mental hygiene unit" organised under his direction by the Iowa State Psychopathic Hospital. Later it was continued as a research with aid from the Rockefeller Foundation at the Neurological Institute of New York. The disorders with which he is chiefly concerned in this volume include stuttering and other defects of speech, disabilities in reading and writing, delayed development of speech, left-handedness and abnormal clumsiness in movement, and more specialised disabilities analogous to the various forms of aphasia found among adults. Dr. Orton starts from the general position accepted by medical writers on these subjects. He does not entirely ignore the importance of emotional factors; but at the same time he seeks the main causes of these disabilities rather in physical or anatomical defects than in disturbances of mental function. He regards "language losses in the adult" as "the key to developmental disorders in children." He finds that many of the symptoms found among children closely resemble those found among adult patients suffering from aphasia for which there is

presumably a specific physical cause, and so describes the disorders almost exclusively in physiological terms. Consequently, with many other medical writers, he lays much stress upon the right-handed training of the left-handed child as a cause of serious disorders in speech.

Among psychologists the general tendency is to consider the chief causes of language disorders in children as functional rather than organic; that is to say, the emotional and intellectual disturbances that accompany them are regarded, not as the mere effects, but as the main causes of the symptoms found. The attempt to trace these symptoms to anatomical defects in the brain is still very speculative. Too little is as yet known about the relation between mental processes and cerebral processes for such suggestions to be anything more than tentative hypotheses.

Nevertheless, it is very valuable to have this point of view systematically worked out and stated. Even if it is not accepted, it is nevertheless a theory with which every teacher and school medical officer should be familiar. Dr. Orton's presentation is extremely clear and lucid. The discussion is aided by many illustrations, not only of brain structure and brain localisation, but also of peculiarities of handwriting and the like. There is a glossary of technical terms to assist the reader untrained in physiology and unfamiliar with the numerous technical terms which medical writers have devised to label disabilities in speech, reading, writing, and the like.

C. B.

Bacteriology: A Text-Book of Micro-organisms. By Fred Wilbur Tanner. Third edition. [Pp. xiv + 510, with 151 figures and 1 folding chart.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1937. 17*s.* 6*d.* net.)

The beginner in microbiology will find this a remarkably helpful volume. Professor Tanner has the gift of making microbiology interesting. This book is a product of a philosophical mind and is very readable. It discusses almost every aspect of microbes considered as living organisms. Technique has no place except for a remark (in the preface) on the importance of observations made in the laboratory. The exclusion of references to technique seems to be too complete: a brief account of the Gram stain would make the classificatory descriptions more intelligible. The index refers only incidentally to gelatine, and not at all to agar or staining. Apart from these little matters, the book is admirable. Its comprehensiveness extends to moulds, yeast, and protozoa, as well as bacteria. The sections on what might be called everyday or household bacteriology—foods, water, disease—are written with a refreshing common sense which deserves a wide diffusion. Writers like Prof. Tanner are rare, and it is to be hoped that the author's more detailed book on the bacteriology of food will also appear very soon in a new edition.

H. N.

An Introduction to Abnormal Psychology. By V. E. Fisher. Second edition. [Pp. xii + 533.] (New York and London: Macmillan & Co., Ltd., 1937. 12*s.* 6*d.* net.)

This is something more than merely a revision of Dr. Fisher's work on Abnormal Psychology which was published over eight years ago. No less than seven entirely new chapters have been added to the original text, while three have been altogether omitted. Besides this, the illustrative case histories have been greatly increased in number, and experimental studies

have been introduced. What is perhaps most noteworthy is the Author's approach to the subject of abnormality which, as he tells us in the preface to this revised edition, has been materially altered and extended. It is guided by two leading principles; first, that mental abnormality is to be regarded as a purely relative matter and should be studied in relation to the more normal phenomena from which it is distinguished; second, that abnormalities are best understood and treated when they are looked upon as disorders of the individual rather than as disorders of this or that particular mental process or reaction. The book, accordingly, follows the plan of dealing with the "normal" personality, its variants, and their measurements, in the first place; and then proceeds to the psychotaxes and parataxes, or modes of reaction to difficulties ("always something which tends to block a motive"). These are treated under the heads of Common Modes of Reaction, Psychoneuroses, Borderline Conditions, Functional Psychoses, and Organic Psychoses. Chapters on Sleeping, Dreaming, Hypnotism and Suggestion, Feeblemindedness, and Mental Hygiene follow. There is an excellent glossary and a good index. The book is written primarily for students attending a first course in abnormal psychology, and presupposes no more psychological knowledge than would have been gained in a preliminary general course. But it is written so clearly and readably that even those who have had no general grounding in the subject should be able to understand it and profit by its perusal. It can certainly be recommended as an admirable first text on the subject. F. A.

BIOGRAPHY AND HISTORY OF SCIENCE

Professor David: The Life of Sir Edgeworth David, K.B.E., C.M.G., F.R.S. By M. E. David. [Pp. 320, with 8 plates and 1 map.] (London: Edward Arnold & Co., 1937. 12*s*. 6*d*. net.)

In Australia Prof. David's name is a name to conjure with and it is well to have his life written by one who knew him intimately and who has evidently inherited the sense of humour which was a part of David's charm. The reviewer knew him well: lived "in his pocket" through a year of Antarctic adventure and, again, at Sydney while working up the geological results of the Shackleton Expedition.

He was in a class by himself, whether regarded as professor, colleague or citizen, and men of all ranks in Australia are proud to have counted him among their friends. He lived a very full life. Migrating to Australia as a geological surveyor at the age of twenty-four, he was instrumental in locating and surveying one of her great sources of industrial wealth, the magnificent coal seams of New South Wales. He succeeded to the professorship of Geology at the University of Sydney and built up a truly national school with an international reputation. David's geologists and engineers have worked in most countries where there are mines, and if there is one thing about which they can be persuaded to agree it is in their pride in the fact that they are David's men.

He was equally at home boring for coal in Australia, through coral in the South Seas, or in ice in Antarctica, and made notable contributions to science in all three places. At the age of fifty he took French leave from his University and added 100 per cent. to the scientific value of Shackleton's first expedition to the South. That was to be expected, but even his admirers were surprised when the old man (old as sledge-travellers go) set up a new

record for man-hauling at that age and led his party successfully to the South Magnetic Pole. At sixty we see him in France with an Australian Tunnelling Company, some years later in central Australia with camels and another geologist even older than himself. And through the whole adventure run the triple threads of humour, courtesy and friendship which made the "Professor" unique among his fellows: in the opinion of the generations of his students and the legion of his friends, the greatest Australian who ever lived. Although British born, it is as an Australian that he thought of himself, and as an Australian that he would like to be remembered.

His biographer has told the story well, the better because she has had the wisdom to let the Professor speak for himself whenever possible. The story deserves to be widely read. If it could be given to every Australian lad when he reached the age of sixteen Australia's future prospects would be better than they are. Every Australian should possess it if only so that he may have at hand a witness of how fifty years of the Australian environment can bring out the best in a strong, kindly and good man. Every Welshman will read it with pride because it shows what a fine foundation the Celtic temperament and a Welsh upbringing provide as a base for building upon in a rather harsh new world. To the rest of the world one can commend the book as a fine example of a record of human gentleness, unselfishness and greatness combined, which is well calculated to renew a faith in human nature which is more than ever needed to-day in a world which to most of us, in our saner moments, must appear to be pretty evenly divided between "sheep" and "goats" in the uncomplimentary sense of both terms. David proved once and for all that he was not a "sheep" when he risked his job to follow the gleam in Antarctica. His lack of malice and his thought for others definitely remove him from the "goat" class.

R. E. P.

Recollections of My Life. By SANTIAGO RAMÓN Y CAJAL. Translated by E. HORNE CRAIGIE. Memoirs of the American Philosophical Society, Vol. VIII, Parts I and II. [Pp. xii + 638, with 81 figures, including 21 plates.] (Philadelphia: The American Philosophical Society; London: Humphrey Milford, 1937. 22*s*. 6*d*. net.)

RAMÓN Y CAJAL died October 17, 1934, aged 82 years. He was in many ways a most remarkable man, if not a genius, combining the æsthetic temperament and enthusiasm of an artist with the persistent urge of scientific curiosity and an inexhaustible capacity for work; a long list of 288 of his published books and papers is appended to this memoir.

Owing chiefly to the extraordinary egotism of the autobiographer, perhaps also to his advanced age and his lack of literary style, which is extremely naïve and childish, these two volumes of over 600 pages are disappointing and unsatisfactory. The section dealing with neurological questions will not be understandable to general readers; neurologists and histologists will prefer to consult the original papers; the personal trivialities which comprise the whole of the first volume will only be tedious to neurologists and not very interesting to the general reader; it is conceivable that the original memoirs may appeal to Spanish readers and especially those portions dealing with the author's students and contemporaries.

The American Philosophical Society has performed a kindly duty by publishing the memoirs of this great man. The translator probably had no easy task and had limited powers as an editor. P. J.

A Short History of Chemistry. By J. R. PARTINGTON, M.B.E., D.Sc. [Pp. xiii + 386, with 119 figures.] (London: Macmillan & Co., Ltd., 1937. 7*s.* 6*d.*)

EXPERIENCE teaches that one does not merely buy or possess a book by Prof. Partington; one reads it, and having read it one is conscious of having sat at the feet of Authority, whether the subject be pure, applied or historical chemistry. Although this book is said to be "suited to the requirements of students preparing for degree examinations," as indeed is admirably the case, it is one which many whose studies are wholly unconnected with universities or degrees, and not even mainly concerned with chemistry, will read with both pleasure and profit. It cannot be an easy task to write a satisfactory history of chemistry; matter there is in plenty if one is content to copy or paraphrase the work of others and to accept without enquiry the truth of a statement that has been repeatedly published. This, however, is not the author's way; he has drawn freely, indeed "the greater part of the material," from original sources, and has constantly striven to be "objective, impartial, and free from conventional platitudes." Moreover, his interest in the personal characters of the actors on the chemical stage has enabled him, without overshadowing the scientific aspect of their lives, to present his subject in a manner which makes us seem ourselves to share in its development.

Following a review of early chemistry, including alchemy and the application of chemistry to medicine, an account is given of investigations into combustion, and of the development of the atomic and dualistic theories. The progress of organic chemistry is discussed, due regard being had to the type theories, the theory of valency, and stereochemistry, whilst the later chapters are devoted to the history of physical chemistry, the periodic law, and the structure of the atom.

A. A. E.

MISCELLANEOUS

A Tribe in Transition: A Study in Culture Pattern. By D. N. MAJUMDAR, M.A., Ph.D. [Pp. xx + 216, with 5 plates.] (India, London, New York, Toronto: Longmans, Green & Co., 1937. 10*s.* 6*d.* net.)

THE rapid advance of modern civilisation has had and is still having a profoundly modifying effect on the culture of primitive peoples. Culture contact has, of course, been going on for untold generations, but modern mass production, with its attempt at a uniform pattern, is producing problems different in many ways from what has occurred in the past. A natural concomitant of this has been an increasing desire to study peoples, both primitive and more advanced, and to attempt to give a picture of their present state. In the present case we have an interesting example of collaboration between the descriptive ethnologist and educated members of the tribe. Such a collaboration is naturally more fruitful of accurate results than a mere study by a member of the higher culture of people who are remotely separated from him. The book is essentially descriptive in its aim and, though the material culture is briefly described, the greater part is devoted to the social organisation and to religious beliefs. The evidence has been carefully weighed and it should prove a useful record of the present state of an interesting people.

L. H. D. B.

Human Affairs : An Exposition of what Science can do for Man. Planned and edited by R. B. Cattell, M.A., B.Sc., Ph.D., J. Cohen, M.A., R. M. W. Travers, B.Sc. [Pp. xii + 360, with 16 plates.] (London : Macmillan & Co., Ltd., 1937. 10*s*. 6*d*. net.)

This collection of essays on the relation of science and philosophy to sociology and psychology makes a most interesting, thought-provoking and important book. Unfortunately several of the contributors, especially those concerned with the more psychological and sociological aspects, are not gifted with the art of clear exposition, and like most thinkers in these newer branches seem unable to express their views and arguments without recourse to slogans and paradoxes, hiding their meaning to the lay reader in the obscurity of a jargon of words. On the other hand, the editors, in their selection of authors and in their excellent editorial, have done well. The essays by Prof. Havelock-Ellis, Prof. Haldane, Prof. Katz, and especially that by Prof. McDougall, are very clearly expressed and merit careful perusal. The inclusion of portraits of the authors is probably a mistake ; few things date so rapidly as photographs.

P. J.

Science and Common Sense : An Aristotelian Excursion. By W. R. Thompson, F.R.S. With a preface by Jacques Maritain. [Pp. viii + 234.] (London, New York, Toronto : Longmans, Green & Co., 1937. 7*s*. 6*d*. net.)

This is a most unusual and stimulating book. It contains in surprisingly short compass a mature and penetrating criticism of certain aspects of modern scientific thought, written from the point of view of Aristotelian Thomism, for Dr. Thompson, besides being a distinguished entomologist, is a keen and learned student of that great system of mediæval philosophy, of which the tradition is worthily maintained in these days by Maritain and others. To most of us, brought up in complete ignorance of ancient methods of thought, it will come as a surprise to find how vigorous and powerful the reasoning of Aristotle and Aquinas was, and how helpful it is even now, especially in the biological sciences.

Dr. Thompson's book is so full of meat that it is impossible in a short review to do justice to it. Essentially it is a reasoned protest against the tendency observable in modern scientific thought to disregard logic and observation and substitute mathematical fantasies for reality. " The fundamental truthfulness of the images presented by the senses ; the validity of the ideas the intelligence extracts from the objects of the senses ; the basic reliability of the normal process of the mind—of the rational discourse—as an avenue to an intelligible synthesis of natural phenomena : these are the assumptions upon which science reposes. Discard any one of them and it is inconceivable that by any artifice we can discover the truth about Nature. Common Sense, reduced to these primary elements, *must* therefore be in fundamental accord with Science ; and if in some of their recent flights, men of science have thought to give practical proof of the liberation of Science from the bonds of Common Sense, it is evident that they are entertaining an illusion " (p. 26). Dr. Thompson hunts down this illusion in the fields of mathematical physics and mathematical biology. He points out that the concepts embodied in the theory of relativity are not only contrary to common sense but fundamentally unreasonable. In biology, mathematical

reasoning on ultra-simplified analogies tells us nothing about what actually happens in Nature; only observation and experiment can reveal this. These are the main themes of the long and important chapter on "The Use and Abuse of Mathematics," which is followed by an equally profound one on "The Use and Abuse of Philosophy." In this, Dr. Thompson treats of the whole field of the science and philosophy of living organisms from an Aristotelian standpoint. Particularly interesting to the zoologist are his criticism of the Darwinian theory as essentially philosophical and deductive, his treatment of the problem of vitalism, on which he holds sound Aristotelian views, and his discussion of instinct. The final chapter, on evolution, emphasises the need for a thorough methodological study of the whole problem.

But this is not a book that can be adequately summarised; it is emphatically a book to be read and studied—especially if you disagree with the views therein expressed.

E. S. Russell.

Psychology: The Changing Outlook. By Francis Aveling, M.C., D.Lit., D.Sc., Ph.D. Changing World Library, No. 8. [Pp. viii + 152.] (London: C. A. Watts & Co., Ltd., 1937. 2*s*. 6*d*. net.)

General Psychology. By W. J. H. Sprott, M.A. [Pp. x + 446, with 6 figures.] (London, New York, Toronto: Longmans, Green & Co., 1937. 7*s*. 6*d*.)

We have heard for so long that the science of psychology is in its infancy that it is a pleasure to welcome two books both of which indicate that the young science is growing up. In point of fact it may fairly be claimed that the infancy of psychology ended with the nineteenth century. The movements of the last thirty or forty years have been characterised by the traits of adolescence. But in these two works—though very different in other ways—we see psychology in transition to maturity. It has left behind adolescent dogmatisms, adolescent revolts, and the adolescent desire to prove its forbears always wrong. Both authors practise a discriminating eclecticism, and are willing to find useful contributions to their science in the older psychologies and in all the more prominent of the contemporary schools.

Dr. Aveling, after a rapid survey of the history of psychology from Aristotle to Wundt, passes in review the chief of the contemporary schools of thought—Spearman's nœgenetic system, the psychoanalytic doctrines, the Gestalt, Hormic and Behaviouristic expositions, and concludes this, the chief, section of the book with a balanced and judicial discussion of the main lines of cleavage and agreement. The theoretical review is followed by a brief conspectus of the educational, industrial, medical and criminological application of the science. The book ends with six pages of cautious prophecy. The direction in which the author anticipates a more complete and coherent synthesis to be attained is that of Gestalt psychology; but recognition is accorded to other trends in the suggestion that the nuclear point around which the new synthesis may be expected to crystallise will be provided in further research into the problem of the will.

Mr. Sprott has probably packed into his 446 pp. as much information as has ever before been compressed into a volume of similar size and general character. What is this book? an introduction for the general reader, a student's text-book, or a pocket encyclopædia? The author very nearly

falls between the three stools, but the felicity of his style and his skill in arranging his material enables him to retain his balance to the end. Accordingly the work may be fairly recommended for the triple purpose.

Whilst the general lines of exposition are similar to those of other modern text-books, space is found for much that these other books exclude. Mr. Sprott is interested in the problems of philosophical psychology, in psychical research and in some of the psychological doctrines of Oriental thought. What he has to say on these matters is always to the point, and one could have wished that he had allowed himself more elbow-room for their discussion.

The author is representative of the main body of teachers of psychology in being neutral and eclectic in relation to the various schools. It is a great merit in the work that an attempt has been made to concentrate "on that which all schools must necessarily have in common: the problems which they have to solve." For this reason the work may safely be commended to the student and to the general reader who wishes to begin his studies by a rapid and comprehensive survey of the whole of the contemporary psychological scene.

C. A. M.

Markets and Men : A Study of Artificial Control Schemes in some Primary Industries. By J. W. F. Rowe, M.A. [Pp. x + 259, with 8 plates and 3 maps.] (Cambridge : at the University Press, 1936. 7*s*. 6*d*. net.)

The author presents in the present book a very readable account of the origins of some of the important raw materials. He deals with Coffee, Wheat, Sugar, Cotton, Rubber and Tin. His descriptions of the geographical distribution of these primary products and of the "economics" of their production are necessary to the main purpose of the book, which is to describe in considerable detail the methods of control of these goods into the world's markets. Mr. Rowe is very well equipped for this sort of task, as he has studied on the spot the problems of production of some of these raw materials in what may be described as a world tour. Consequently one feels, when reading the book, that one is reading about things of which the author has a masterly knowledge. In addition, of course, Mr. Rowe is a practical economist, and he is able to place the special problems of these industries in their proper position in the general world economic situation.

The author traces the general movement towards control back to pre-war days, allows for the influence of the upheaval due to the war and especially for the difficulties due to the lack of shipping during and after the war, and proceeds to describe in detail for each industry its special methods of tackling the problem of control.

As a result of his analysis of the progress of the various schemes which have been adopted, be attempts to draw some general conclusions. He considers that restriction of output "is economically unsound as a means of meeting a permanent decline in the demand for the product of a particular industry during times of general prosperity." On the other hand, during periods of depression restriction may be reasonable for a short period until recovery is in sight. His other conclusions as to the value of the various schemes are interesting.

It is true, that one may say that it is easy to be wise after the event. But, it is not everyone who is able to use his wisdom to draw the proper conclusions from the event. The business man critic of the economist may

think that the latter's task appears easy when he reads this book, and he is strongly recommended to do so, because the author has the faculty of making the various problems appear to be facile. But, it is not given to everyone to sift the facts of a case and recognise the little granules of truth remaining.

E. C. RHODES.

Business Statistics. By G. R. DAVIES and DALE YODER. [Pp. viii + 548, with 79 figures.] (New York : John Wiley & Sons, Inc. ; London : Chapman & Hall, Ltd., 1937. 17*s*. 6*d*. net.)

AN important post-war development is the establishment in large business houses of new departments variously described as "intelligence," "economic" or "statistics." These departments keep the executive in touch with the outside world by means of surveys, charts and tables of current economic statistics, and analyse data derived from the individual business. Outside specialists supply clients with information of value from the point of view of selling the product. The Bank of England, acting in co-operation with retailers, publish important tables relating to the value of retail sales month by month.

Such developments as these are of recent origin in England, but in America they are commonplace, and American Universities have had their Schools of Commerce and Departments of Business Administration for some time. The present book was written by the Professor of Business Statistics in the University of Iowa and the Professor of Economics and Business Administration in the University of Minnesota.

The book describes the statistical technique of averaging, measuring variation within a group, measuring trends of time series, calculating seasonal movements, computing index numbers. In addition, correlation is dealt with at great length, and there is a chapter on reliability and significance. The book is intended for students pursuing a course of study in a University institution, and is very well illustrated throughout with examples derived mainly from economic and business data. In addition, each chapter contains exercises for private study. These illustrative examples are very useful, because by the time the student has read through the book and has diligently worked out the examples, he will not only know something about elementary statistical methods, but he will also know something about the kind of practical statistical problem which he is likely to encounter in any business. The book is excellent from that point of view. The mathematical part has been reduced to a minimum.

I was rather surprised to find so little description in the book of the methods of computing the index numbers most used in the United States. I think that anyone who has been through a course such as is indicated in the book would probably be using daily the current indices of prices, production, etc., and he ought to be aware of the detailed making up of such indices. But, perhaps, the authors expect the student to do his own reading on these lines from the excellent bibliographies provided throughout the book.

A word of compliment must be said on account of the splendid printing, the type is easy, the tables are easy and the graphs are easy to read. But we should expect this from Professors of Business Statistics and Administration. The student who expects to go into business in an intelligence department has to realise that he has to "sell" a graph or a table of figures

to an executive officer, just as a cunning salesman sells to a customer a vacuum cleaner. Consequently, his statistics must be put into a palatable form.

E. C. RHODES.

World Prices and the Building Industry. By G. F. WARREN, Ph.D., and F. A. PEARSON, Ph.D. [Pp. viii + 240, with 162 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1937. 17*s.* 6*d.* net.)

THE present book, by two well-known professors at Cornell, is in two parts. In the first part an attempt has been made to compare prices of commodities in a number of different countries. This is admittedly a difficult task and has been undertaken from time to time by others. For instance, in the Bulletins of the London and Cambridge Economic Service there appear, quarterly, comparable indices of this nature for a few countries. The authors, in their appendix, compare their results with those of the London and Cambridge Service. The main difficulty in the preparation of indices of the kind dealt with here is making a choice of commodity prices which are to enter into the computations. The authors select 40 basic commodities, 18 foods and 22 materials. The data generally refer to each month of the years from 1910 to 1936 and indices were calculated for fourteen countries, United States, England, France, Spain, Canada, Netherlands, Belgium, Italy, Germany, Bulgaria, Australia, New Zealand, Sweden and Finland. In addition, a World Price Index was prepared from the data for the first seven of these countries.

To some extent, of course, the indices obtained are bound to be influenced by the particular choice of commodities, but the broad conclusions reached by the authors will be the same whatever variations are introduced by a slightly different choice of data. Similarly, criticisms may be levelled at the authors because they have computed world indices, based on data referring to seven particular countries. Again, the broad conclusions reached will be the same, even if some variation were introduced into the method of calculation, since the commodities chosen are those which are of importance in the economy of all countries included.

The indices have been computed on both currency and gold basis. The world price level (gold) declined to a minimum in 1934 and has subsequently risen. The level in 1934 was 64 per cent. of pre-war.

The divergencies between gold and currency prices for the various countries are displayed very effectively in graphical form. The general trends of gold prices in the various countries are compared. Movements of prices of important commodities such as cotton are traced.

In the second part, the subject is the variations in the activity in the building industry over long periods of years in different countries. There appear to have been regular cycles in building. The analysis deals with the various factors which influence the construction of buildings. The whole of this part consists of a very interesting account of this industry over a long period.

Of great interest also is the Appendix, with accounts of the comparative methods of construction of various indices.

The book is an important piece of work which does great credit to the energy of its distinguished authors. E. C. RHODES.

The Criminals We Deserve. By H. T. F. RHODES. [Pp. xii + 257, with 8 plates.] (London: Methuen & Co., Ltd., 1937. 7*s*. 6*d*. net.)

CRIME is one of our increasingly costly economic problems. Not only is crime increasing in quantity, but needs to be attacked by increasingly more elaborate methods. The increase in juvenile crime is especially remarkable, a rise of 50 per cent. between 1933 and 1935 seems almost incredible. The Chief Constable of Liverpool in 1935 observed 4,700 extra offences committed by juveniles, or nearly 13 a day! It has been said that the great mass of crime is based on poverty, and it cannot be disputed that slum life—overcrowding and its consequent unsocial immoral habits, to which has been added education, which trains these slum-dwellers to expect something better from the world than they are able to attain—is perhaps one of the basic origins of the problem. But the poor are not always criminals, crime is found to be equally distributed throughout the classes. In this excellent book crime is considered as individual crime and organised crime (a more recent and more dangerous form of law-breaking). Individual crime, generally less difficult to trace, may arise from hunger, covetousness, curiosity, the spirit of adventure—and especially occurs in those "twisted in the making"—a psychic origin. Organised crime—gangsterism—is big business undertaking, is more dangerous to society and costly for detection.

The author illustrates some of the chief types of crime by short accounts of well-known crimes and criminals—murder, forgery, drug traffic, incendiarism, white slavery, swindling tricksters, all of which make interesting reading—incredible for their audacity. Some of the methods employed in tracing these criminals are described. Dealing with treatment—punishment of criminals—he considers prevention is better than cure, and concludes that the existing social system founded on unrestricted competition and profit-making is the basis of crime and must be liquidated, and points to Soviet Russia as the first example of such a society. Our punishment no less than our criminals are symptoms of decline in a system which is quickly outliving its usefulness; it creates more criminals than it cures, by confirming in them that anti-sociality which is an amplified function of its own defects and failure. We not only create but we perpetuate in our penal system the kind of criminal we deserve.

P. J.

The Microtomist's Vade Mecum (Bolles Lee). Edited by J. BRONTË GATENBY, M.A., Ph.D., D.Phil., D.Sc., and THEOPHILUS S. PAINTER, A.B., A.M., Ph.D. Tenth edition. [Pp. xii + 784, with frontispiece and 11 figures.] (London: J. & A. Churchill, Ltd., 1937. 30*s*. net.)

IN the tenth edition of Lee, Prof. Painter takes the place of Dr. E. V. Cowdry as co-editor with Prof. Gatenby. The book has, of course, been thoroughly revised and modernised, and while the rearrangement has led to the omission of two sections which appeared previously, it will be felt widely that their loss is amply compensated by the inclusion of a section on Botanical technique by Dr. D. G. Catcheside. This section is comprehensive, occupying some 100 pages, and, apart from general techniques, includes sections on such subjects as cytological and palæobotanical methods as well as on micro-chemical tests. Botanists have always found Lee a valuable reference book, but with the inclusion of this section it should prove as indispensable to

botanists as to zoologists, and, to quote from the preface, "It is certain that the botanist and zoologist will benefit by looking over various chapters describing the techniques in the other sister science. . . ." Lee now becomes, in the widest sense, the Microtomist's Vade Mecum.

This edition includes new chapters on the frozen section technique and also on vital staining, while Dr. Conn has revised and brought up to date the late Professor Bayliss's article on staining.

Revision of the book has not been accomplished without the addition of another 74 pages above those of the ninth edition, although the inclusion of the section on botanical methods more than accounts for this increase. Nevertheless, it is difficult to avoid the feeling that the book is becoming a little unwieldy for the busy worker. It is not suggested that the work could be abridged without detriment, but it may prove possible, perhaps in the next edition, to include brief summaries of the more useful techniques in at least some of the chapters.

The editors point out a difference between the later editions and those handled by Lee, in that Lee himself tried out each method mentioned, while in recent editions this has, for obvious reasons, not proved practicable. Such a course as Lee adopted is ideal, and while the ideal may not have been attained, the editors and their collaborators have carried out their task in a manner which lacks nothing in thoroughness.

F. W. J.

Textile Design and Colour. By W. WATSON, F.T.I. Fourth edition. [Pp. xii + 492, with 438 figures.] (London, New York, Toronto: Longmans, Green & Co., 1937. 21*s*. net.)

IT is a strange fact that the number of text-books in English devoted to the technical processes of the textile industries is out of all proportion to the magnitude of those industries. The small number that are in current use mostly rank as classics, and in the special domain of the ornamentation of cloth by the art of the weaver few so well deserve this recognition as Watson's *Textile Design and Colour* and his companion volume *Advanced Textile Design*.

The present volume is essentially for textile men, since it presupposes a knowledge of the terms used to describe the parts of the loom. The most useful section to the general reader will probably be Appendix I, pp. 351–425, which in dictionary form describes the most important types of yarns, modes of interlacing them (" weaves ") and fabrics.

Chapter X deals with " Colour Theories and Phenomena " and reflects the chaotic condition of our nomenclature and treatment of this subject from which bodies like the Optical Society of America are gradually extricating us. Bearing in mind that the book is intended for textile students, the reviewer thinks that this chapter should be very greatly simplified and illustrated by diagrams in colour. The present verbal treatment leaves the reader with such sentences as the following (from p. 138): "A tint is therefore a tone which is lighter, and a shade a tone which is darker, than the normal colour." "A ' mode ' shade is a broken colour in which a certain hue predominates over a pure grey."

The author calls special attention in his preface to Appendix II, which describes the manufacture of rayon. This is out of place in the book.

SCIENCE PROGRESS, however, is not really the place to review the book,

for it does not claim to treat of the *Science* of Textile Design. Such a science is in its infancy but badly needs development. Fabrics are not produced solely for appearance sake, but an increasing volume of trade is done in cloths that are chosen for their mechanical properties. In this field, textile design provides some tough problems for the physicist.

J. C. WITHERS.

Zero to Eighty. By AKKAD PSEUDOMAN (*alias* E. F. NORTHRUP). [Pp. xvi + 283, with 15 plates and 14 figures.] (Princeton, N.J.: Scientific Publishing Co., 1937. $3.50.)

SURELY the most extraordinary book that ever fell to the lot of a reviewer for this Journal. Though in the tradition of Jules Verne and H. G. Wells, its breezy style is of the best American talking film. Let the following extract (p. 79) testify to the author's judgment of what is needed in modern scientific fiction: "I read aloud the typed matter—a terrible message. 'We hold your gal to ransom, we want $500,000 and no chiseling. Follow instructions exactly. . . . Do as we tell you and no tricks mind you. If you pull off any tricks you will never see your gal again. We will feed her dope and use her as we please.'"

So much for the style. As for the matter, it is a fictitious account of a trip by rocket to the stratosphere (0° C. to 80° C.) and the moon in 1954, based on some laboratory experiments carried out by the author in which he projected metal cylinders from coils along which electromagnetic phase waves passed.

As such, of course, it invites comparison with Mr. Barbicane's famous experiment in 186–, but whereas Verne has a way of lulling our incredulity by convincing—but probably false—mathematics introduced judiciously into the text, his disciple follows present-day procedure with technical supplements (17 of them) ranging from "The Laws of Motion" to "The Load-ship as a Satellite to the Moon." This is the weakness of the book, for while the author can write a good thriller as long as he keeps science in the background, he spoils it by his lessons and examples in fourth-form mathematics at the end. What are we to say to an author who shows us (p. 244) how to calculate the height to which a projectile would rise with a given vertical muzzle velocity, and then naïvely adds that in practice (owing to atmospheric friction) one must divide this answer by about five?

E. G. R.

BOOKS RECEIVED

(Publishers are requested to notify prices.)

The Nature of Variable Stars. By Paul W. Merrill, Astronomer in the Mount Wilson Observatory, Carnegie Institution of Washington. New York and London: Macmillan & Co., Ltd., 1938. (Pp. x + 134, with 2 plates and 12 figures.) 8*s*. 6*d*. net.

The Pageant of the Heavens. By Frederick Warren Grover, Ph.D., Professor in Union College. London, New York, Toronto: Longmans, Green & Co., 1937. (Pp. xx + 157, with 13 figures.) 12*s*. 6*d*. net.

The Climate of the British Isles. Being an Introductory Study of the Official Records, for Students and General Readers. By E. G. Bilham, A.R.C.Sc., D.I.C., B.Sc., F.R.Met.Soc., Superintendent of British Climatology and the British Rainfall Organization, Meteorological Office, Air Ministry. London: Macmillan & Co., Ltd., 1938. (Pp. xx + 347, with 101 figures.) 21*s*. net.

A Textbook of Physics. By Louis Bevier Spinney, Professor of Physics, Iowa State College. Fifth edition. New York and London: Macmillan & Co., Ltd., 1937. (Pp. xiv + 721, with 481 figures.) 16*s*. net.

Modern Physics. A Second Course in College Physics. By G. E. M. Jauncey, D.Sc., Professor of Physics, Washington University, St. Louis. Second edition. New York: D. van Nostrand Co., Inc.; London: Chapman & Hall, Ltd., 1937. (Pp. xviii + 602, with 241 figures.) 22*s*. net.

The Evolution of Physics. The Growth of Ideas from the Early Concepts to Relativity and Quanta. By Albert Einstein and Leopold Infeld. The Cambridge Library of Modern Science. Cambridge: at the University Press, 1938. (Pp. x + 319, with 3 plates and 75 figures.) 8*s*. 6*d*. net.

The Elements of Quantum Mechanics. By Saul Dushman, Ph.D., Assistant Director, Research Laboratory, General Electric Co., Schenectady, N.Y. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. (Pp. xiv + 452, with 82 figures.) 25*s*. net.

Spectroscopy in Science and Industry. Proceedings of the Fifth Summer Conference on Spectroscopy and its Applications, held at the Massachusetts Institute of Technology, July 19–22, 1937. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. (Pp. viii + 134, with 60 figures.) 15*s*. net.

Photoelements and Their Application. By Dr. Bruno Lange, Consulting Engineer, Berlin-Dahlem, formerly Research Physicist with Kaiser Wilhelm Institute, Berlin. Translated by Ancel St. John, Ph.D., New

York. New York: Reinhold Publishing Corporation; London: Chapman & Hall, Ltd., 1938. (Pp. 297, with 67 figures.) 27*s*. 6*d*. net.

The Fine Structure of Matter. The Bearing of Recent Work on Crystal Structure, Polarization and Line Spectra. Being Vol. II of a Comprehensive Treatise of Atomic and Molecular Structure. Part II: Molecular Polarization. Part III: The Quantum Theory and Line Spectra. By C. H. Douglas Clark, D.Sc., A.R.C.S., A.I.C., D.I.C., Assistant Lecturer in Inorganic Chemistry in the University of Leeds. London: Chapman & Hall, Ltd., 1938. (Part II: pp. lxxii + 242, with 35 figures. Part III: pp. lxxii + 185, with 29 figures.) 15*s*. net each part.

These Amazing Electrons. By Raymond F. Yates. New York and London: Macmillan & Co., Ltd., 1937. (Pp. x + 326, with 46 plates and 63 figures.) 16*s*. net.

Direct and Alternating Current Potentiometer Measurements. By D. C. Gall, F.Inst.P. With a Foreword by S. Parker Smith, D.Sc., M.I.E.E., A.M.Inst.C.E. Vol. IV of a Series of Monographs on Electrical Engineering under the Editorship of H. P. Young. London: Chapman & Hall, Ltd., 1938. (Pp. xiv + 231, with 109 figures, including 12 plates.) 15*s*. net.

Automatic Protection of A. C. Circuits. By G. W. Stubbings, B.Sc., F.Inst.P., A.M.I.E.E. Second edition. London: Chapman & Hall, Ltd., 1938. (Pp. viii + 311, with 210 figures.) 15*s*. net.

A Text-Book on Crystal Physics. By W. A. Wooster, M.A., Ph.D., Lecturer in the Department of Mineralogy and Petrology, University of Cambridge. Cambridge: at the University Press, 1938. (Pp. xxii + 295, with 108 figures.) 15*s*. net.

Airplane Structures. Vol. I. By Alfred S. Niles, A.B., S.B., Professor of Aeronautic Engineering, Leland Stanford Jr. University, and Joseph S. Newell, S.B., Associate Professor of Aeronautical Structural Engineering, Massachusetts Institute of Technology. Second edition. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. (Pp. xvi + 451, with 184 figures.) 25*s*. net.

The Principles and Practice of Surveying. By Charles B. Breed, Professor of Railway and Highway Transportation, and George L. Hosmer, late Professor of Geodesy, Massachusetts Institute of Technology. Vol. I: Elementary Surveying, seventh edition. Vol. II: Higher Surveying, fifth edition. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. Vol. I (pp. xxiv + 717, with 217 figures) 20*s*. net; Vol. II (pp. xxii + 674, with 247 figures) 17*s*. 6*d*. net.

Kinetic Theory of Gases. With an Introduction to Statistical Mechanics. By Earle H. Kennard, Professor of Physics, Cornell University. International Series in Physics. New York and London: McGraw-Hill Publishing Co., Ltd., 1938. (Pp. xiv + 483, with 94 figures.) 30*s*. net.

Dipole Moments. Their Measurement and Application in Chemistry. By R. J. W. Le Fèvre, D.Sc., Ph.D., F.I.C., Lecturer in Chemistry, University College, London. Methuen's Monographs on Physical Subjects. London: Methuen & Co., Ltd., 1938. (Pp. vi + 110, with 28 figures.) 3*s*. 6*d*. net.

Numerical Problems in Advanced Physical Chemistry. By J. H. Wolfenden, Fellow of Exeter College, Oxford, University Demonstrator and Lecturer in Chemistry. (Oxford : at the Clarendon Press ; London : Humphrey Milford, 1938. (Pp. xx + 227.) 7*s*. 6*d*. net.

Organic Chemistry. An Advanced Treatise. Vols. I and II. Henry Gilman, Editor-in-Chief. New York : John Wiley & Sons, Inc. ; London : Chapman & Hall, Ltd., 1938. (Pp. lvi + 1890, with numerous figures.) 37*s*. 6*d*. net each volume.

Principles of Organic Chemistry. By H. P. Starck, M.A., Head of the Science Department, the Technical College, Kingston-on-Thames. London and Glasgow : Blackie & Son, Ltd., 1938. (Pp. viii + 664, with 58 figures.) 12*s*. 6*d*. net.

A Brief Introduction to the Use of Beilstein's Handbuch der organischen Chemie. By Ernest Hamlin Huntress, Ph.D., Associate Professor of Organic Chemistry, Massachusetts Institute of Technology. Second edition. New York : John Wiley & Sons, Inc. ; London : Chapman & Hall, Ltd., 1938. (Pp. x + 44.) 5*s*. net.

Asphalts and Allied Substances. Their Occurrence, Modes of Production, Uses in the Arts and Methods of Testing. By Herbert Abraham, President, The Ruberoid Co. ; President, Asphalt Shingle and Roofing Industry. Fourth edition. New York : D. van Nostrand Co., Inc. ; London : Chapman & Hall, Ltd., 1938. (Pp. xxiv + 1491, with 333 figures.) 60*s*. net.

Rancidity in Edible Fats. By C. H. Lea, B.Sc., Ph.D., Low Temperature Research Station, Cambridge. Department of Scientific and Industrial Research, Food Investigation, Special Report No. 46. London : H.M. Stationery Office, 1938. (Pp. vi + 230, with 38 figures.) 3*s*. 6*d*. net.

Solvents. By Thos. H. Durrans, D.Sc., F.I.C., Director of the Research Laboratories of A. Boake, Roberts & Co., Ltd., London. Vol. IV of a Series of Monographs on Applied Chemistry under the Editorship of E. Howard Tripp, Ph.D. Fourth edition. London : Chapman & Hall, Ltd., 1938. (Pp. xviii + 238, with 4 figures.) 15*s*. net.

The Chemistry and Technology of Rubber Latex. By C. Falconer Flint, Ph.D., D.I.C., A.I.C., A.R.C.S., B.Sc., formerly of the Rubber Research Institute of Malaya and now on the staff of Imperial Chemical Industries, Ltd. With a Foreword by Lt.-Col. B. J. Eaton, O.B.E., F.I.C., F.I.R.I. Based on Georges Génin's "Chimie et Technologie du Latex de Caoutchouc." London : Chapman & Hall, Ltd., 1938. (Pp. xx + 715, with frontispiece and 146 figures, including 36 plates.) 42*s*. net.

The Action of Hydrogen upon Coal. Part III : The Development of a Small-Scale Liquid-Phase Continuous Plant. By N. Booth, Ph.D., B.Sc., A.I.C., F. A. Williams, Ph.D., M.Sc., A.I.C., and J. G. King, Ph.D., B.Sc., A.R.T.C., F.I.C. Department of Scientific and Industrial Research, Fuel Research, Technical Paper No. 44. London : H.M. Stationery Office, 1938. (Pp. vi + 27, with 5 figures, including 2 plates.) 9*d*. net.

The Structure of Steel Simply Explained. By Eric N. Simons, and Edwin Gregory, Ph.D., M.Sc., F.I.C., Chief Metallurgist, Park Gate Iron and Steel Co., Ltd. With an Introduction by F. C. Lea, O.B.E., D.Sc.,

M.I.Mech.E., M.Inst.C.E. London and Glasgow: Blackie & Son, Ltd., 1938. (Pp. xii + 115, with 45 figures, including 7 plates.) *3s. 6d.* net.

Semi-Micro Qualitative Analysis. By Paul Arthur, Ph.D., Assistant Professor of Analytical Chemistry, and Otto M. Smith, Ph.D., Professor of Chemistry, Oklahoma Agricultural and Mechanical College. New York and London: McGraw-Hill Publishing Co., Ltd., 1938. (Pp. xii + 198, with frontispiece and 10 figures.) *12s.* net.

Micromethods of Quantitative Organic Elementary Analysis. By Joseph B. Niederl, Ph.D., Associate Professor of Chemistry, and Victor Niederl, Teaching Fellow, New York University, Washington Square College. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. (Pp. xvi + 271, with 53 figures.) *15s.* net.

Outlines of Methods of Chemical Analysis. By G. E. F. Lundell, Ph.D., Chief Chemist, and James Irvin Hoffman, Ph.D., Chemist, National Bureau of Standards. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. (Pp. xii + 250, with 115 tables.) *15s.* net.

An Intermediate Course of Volumetric Analysis. By Gordon Edward Watts, M.A., Ph.D., B.Sc., F.I.C., Vice-Principal and Head of the Chemistry Department, Brighton Technical College, and Clifford Chew, M.Sc.Tech., F.I.C., Principal of the Storey Institute Technical College, Lancaster. London and Glasgow: Blackie & Son, Ltd., 1938. (Pp. viii + 224, with 10 figures.) *3s. 6d.* net.

Intermediate Readings in Chemical and Technical German. Edited by John Theodore Fotos, Associate Professor of Modern Languages, and R. Norris Shreve, Professor of Chemical Engineering, Purdue University. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. (Pp. xliv + 219.) *9s. 6d.* net.

Outlines of Geology. Being a Combination of "Outlines of Physical Geology," by Chester R. Longwell, Professor of Geology, Adolph Knopf, Silliman Professor of Geology, and Richard F. Flint, Associate Professor of Geology, in Yale University, and "Outlines of Historical Geology," third edition, by Charles Schuchert, Professor Emeritus of Paleontology, and Carl O. Dunbar, Professor of Paleontology and Stratigraphy, in Yale University. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1937. (Pp. vi + 356, with frontispiece and 297 figures, and pp. vi + 241, with frontispiece and 151 figures.) *20s.* net.

The Petrology of the Sedimentary Rocks. By F. H. Hatch, O.B.E., Ph.D., Past-President of the Inst. of Mining and Metallurgy and of the Geol. Soc. of S. Africa, and R. H. Rastall, M.A., Sc.D., University Lecturer in Mineralogy and Petrology, Cambridge. Third edition, revised by Maurice Black, M.A., Lecturer in Natural Science and Fellow of Trinity College, Cambridge, and Demonstrator in Geology in the University of Cambridge. London: George Allen & Unwin, Ltd., 1938. (Pp. 383, with 75 figures.) *15s.* net.

An Introduction to Geology. By A. E. Trueman, D.Sc., F.G.S., Professor of Geology in the University of Glasgow. London: Thomas Murby & Co., 1938. (Pp. x + 258, with 133 figures.) *4s.* net.

Physiography of Eastern United States. By Nevin M. Fenneman, Professor of Geology, University of Cincinnati. New York and London: McGraw-Hill Publishing Co., Ltd., 1938. (Pp. xiv + 714, with 197 figures and 6 folding plates.) 36*s*. net.

Comprehensive Index of the Publications of the American Association of Petroleum Geologists, 1917–1936. By Daisy Winifred Heath. Tulsa, Oklahoma: The American Association of Petroleum Geologists; London: Thomas Murby & Co., 1937. (Pp. viii + 382.) $3.00.

Water-Divining. New Facts and Theories. By Theodore Besterman. London: Methuen & Co., Ltd., 1938. (Pp. xii + 207, with 2 plates and 7 figures.) 7*s*. 6*d*. net.

Sixty Years of Botany in Britain (1875–1935). Impressions of an Eye-Witness. By F. O. Bower, Sc.D., LL.D., F.R.S., Emeritus Professor of Botany in the University of Glasgow. London: Macmillan & Co., Ltd., 1938. (Pp. xii + 112, with 14 plates.) 10*s*. 6*d*. net.

Plant Ecology. By John E. Weaver, Professor of Plant Ecology, University of Nebraska, and Frederic E. Clements, Ecologist, Division of Plant Biology, Carnegie Institution of Washington. Second edition. McGraw-Hill Publications in the Botanical Sciences. New York and London: McGraw-Hill Publishing Co., Ltd., 1938. (Pp. xxii + 601, with frontispiece and 271 figures.) 30*s*. net.

Agricultural Analysis. A Handbook of Methods, excluding those for Soils. By C. Harold Wright, M.A., F.I.C., formerly Senior Agricultural Chemist, Nigeria. London: Thomas Murby & Co., 1938. (Pp. x + 343, with 8 figures.) 16*s*. net.

Theory and Practice in the Use of Fertilizers. By Firman E. Bear, Ph.D., Science Editor, The Country Home Magazine. Second edition. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. (Pp. x + 360, with 63 figures.) 20*s*. net.

Timber. Its Structure and Properties. By H. E. Desch, P.A.S.I., B.Sc., M.A. London: Macmillan & Co., Ltd., 1938. (Pp. xxii + 169, with 90 figures, including 27 plates.) 12*s*. 6*d*. net.

Forest Pathology. By John Shaw Boyce, M.A., M.F., Ph.D., Professor of Forest Pathology, Yale University. American Forestry Series. New York and London: McGraw-Hill Publishing Co., Ltd., 1938. (Pp. x + 600, with 216 figures.) 30*s*. net.

Tropical Fruits and Vegetables: An Account of their Storage and Transport. By C. W. Wardlaw. Low Temperature Research Station, Memoir No. 7. Trinidad: Imperial College of Tropical Agriculture, 1937. (Pp. xii + 224.) 4*s*. (post free).

Farm and Garden Seeds. By S. P. Mercer, B.Sc.(Agric.), N.D.A., Professor of Agricultural Botany in the Queen's University of Belfast. With a Section on the Seeds Act, 1920, by A. W. Monro, C.B., Ministry of Agriculture and Fisheries. Agricultural and Horticultural Handbooks. London: Crosby Lockwood & Son, Ltd., 1938. (Pp. 205, with 14 plates and 4 figures.) 10*s*. 6*d*. net.

Dairy Bacteriology. By Bernard W. Hammer, Ph.D., Professor of Dairy Bacteriology, Iowa State College, and Chief in Dairy Bacteriology,

Iowa Agricultural Experiment Station. Second edition. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. (Pp. xiv + 482, with 45 figures.) 25*s*. net.

Genmutation. I: Allgemeiner Teil. Von Hans Stubbe. Handbuch der Vererbungswissenschaft, herausgegeben von E. Baur und M. Hartmann, Band IIF. Berlin: Verlag von Gebrüder Borntraeger, 1938. (Pp. iv + 429, with 90 figures.) RM.60.—(Subscription price RM.48.)

Lac Cultivation in India. By P. M. Glover, B.Sc., Entomologist, Indian Lac Research Institute. Being a second edition of "A Practical Manual of Lac Cultivation," by P. M. Glover. Namkum, Ranchi, Bihar, India: The Indian Lac Research Institute, 1937. (Pp. viii + 147, with 25 plates, including 2 coloured plates.) Rs.2/–.

Bird Flocks and the Breeding Cycle. A Contribution to the Study of Avian Sociality. By F. Fraser Darling, Ph.D., F.R.S.E. Cambridge: at the University Press, 1938. (Pp. x + 124, with frontispiece and 1 figure.) 6*s*. net.

The Measurement of Linkage in Heredity. By K. Mather, Ph.D., Lecturer in the Galton Laboratory, University College, London. Methuen's Monographs on Biological Subjects. London: Methuen & Co., Ltd., 1938. (Pp. x + 132.) 4*s*. 6*d*. net.

Physiological Genetics. By Richard Goldschmidt, Ph.D., M.D., D.Sc., Professor of Zoology, University of California. McGraw-Hill Publications in the Zoological Sciences. New York and London: McGraw-Hill Publishing Co., Ltd., 1938. (Pp. x + 375, with 54 figures.) 24*s*. net.

The Human Value of Biology. By Johan Hjort. Cambridge, Mass.: Harvard University Press; London: Humphrey Milford, 1938. (Pp. xii + 241, with 29 figures, including 2 plates.) 10*s*. 6*d*. net.

Bioclimatics. A Science of Life and Climate Relations. By Andrew Delmar Hopkins, formerly principal entomologist, Division of Bioclimatics, Bureau of Entomology and Plant Quarantine. U.S. Department of Agriculture, Miscellaneous Publication No. 280. Washington: U.S. Government Printing Office, 1938. (Pp. iv + 188, with 55 figures.)

Proceedings of the Fourth International Locust Conference, Cairo, April 22, 1936. Cairo: Government Press, Bulâq, 1937.

The Movements of Salmon Marked in the Sea. II: The West Coast of Sutherland in 1937. By W. J. M. Menzies, F.R.S.E., Inspector of Salmon Fisheries of Scotland. Fishery Board for Scotland, Salmon Fisheries, 1938, No. 1. Edinburgh: H.M. Stationery Office, 1938. (Pp. 9, with 1 map.) 1*s*. net.

Political Arithmetic. A Symposium of Population Studies. Edited by Lancelot Hogben, F.R.S. London: George Allen & Unwin, Ltd., 1938. (Pp. 531, with figures and maps.) 30*s*. net.

The Essentials of Human Embryology. By Gideon S. Dodds, Ph.D., Professor of Histology and Embryology, School of Medicine, West Virginia University, Morgantown, West Virginia. Second edition. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. (Pp. x + 316, with 182 figures.) 20*s*. net.

The Adrenal Cortex and Intersexuality. By L. R. Broster, Clifford Allen, H. W. C. Vines, Jocelyn Patterson, Alan W. Greenwood, G. F. Marrian, G. C. Butler. With a Foreword by Sir Walter Langdon-Brown. London: Chapman & Hall, Ltd., 1938. (Pp. xii + 245, with 23 plates.) 15*s*. net.

Studies on the Physiology of the Eye. By J. Grandson Byrne. Reissue with Supplement and new Index. London: H. K. Lewis & Co., Ltd., 1938. (Pp. xii + 440, with 48 figures.) 40*s*. net.

Fundamentals of Biochemistry, with Laboratory Experiments. By Carl L. A. Schmidt, M.S., Ph.D., Professor of Biochemistry, and Frank Worthington Allen, Ph.D., Instructor in Biochemistry, University of California. New York and London: McGraw-Hill Publishing Co., Ltd., 1938. (Pp. xvi + 388, with 29 figures.) 18*s*. net.

The Cause of Cancer. By David Brownlie, B.Sc., F.C.S. London: Chapman & Hall, Ltd., 1938. (Pp. viii + 208.) 7*s*. 6*d*. net.

Manual of Psychiatry and Mental Hygiene. By Aaron J. Rosanoff, M.D., Los Angeles, California. Seventh edition. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. (Pp. xviii + 1091, with 87 figures, including 1 plate.) 37*s*. 6*d*. net.

Papers on Psycho-Analysis. By Ernest Jones, M.D., M.R.C.P., President of the International Psycho-Analytical Association and of the British Psycho-Analytical Society. Fourth edition. London: Baillière, Tindall & Cox, 1938. (Pp. x + 643.) 25*s*. net.

Science and Psychical Phenomena. By G. N. M. Tyrrell. London: Methuen & Co., Ltd., 1938. (Pp. xvi + 379.) 12*s*. 6*d*. net.

The Doctrine of Signatures. A Defence of Theory in Medicine. By Scott Buchanan. International Library of Psychology, Philosophy and Scientific Method. London: Kegan Paul, Trench, Trubner & Co., Ltd., 1938. (Pp. xvi + 205.) 7*s*. 6*d*. net.

Elements of Statistical Method. By Albert E. Waugh, Professor of Economics, Connecticut State College. New York and London: McGraw-Hill Publishing Co., Ltd., 1938. (Pp. xvi + 381, with 43 figures.) 21*s*. net.

Lectures and Conferences on Mathematical Statistics. Delivered by J. Neyman at the Graduate School of the United States Department of Agriculture in April 1937. Revised and supplemented by the author with the editorial assistance of W. Edwards Deming. Washington: The Graduate School of the United States Department of Agriculture, 1938. (Pp. viii + 163.) $1.25.

On the Statistical Theory of Errors. By W. Edwards Deming and Raymond T. Birge. Reprinted from *Reviews of Modern Physics*, Vol. 6, 119–161, July 1934, with additional notes dated 1937. Washington: The Graduate School of the United States Department of Agriculture, 1938. (Pp. 49, with 15 figures.) 35 cents.

Isaac Newton 1642–1727. By J. W. N. Sullivan. With a Memoir of the Author by Charles Singer. London: Macmillan & Co., Ltd., 1938. (Pp. xx + 275.) 8*s*. 6*d*. net.

Living Again. An Autobiography. By Felix Riesenberg. London: Methuen & Co., Ltd., 1938. (Pp. x + 310.) 10*s*. 6*d*. net.

The Universe of Science. By H. Levy, Professor of Mathematics at the Imperial College of Science, University of London. Revised and expanded. The Thinker's Library, No. 67. London: C. A. Watts & Co., Ltd., 1938. (Pp. xiv + 242.) 1*s.* net.

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SCIENCE PROGRESS

REACTIONS IN THE SOLID STATE

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There are numerous reactions in inorganic chemistry which depend on the decomposition of solid bodies by heat. Many of the simple gases are prepared by this method ; thus carbon dioxide is prepared from carbonates, oxygen from potassium permanganate or potassium chlorate, and nitrogen from sodium azide or ammonium bichromate. Also, use is made of the decomposition of solids in the explosives industry where the main products are chemical compounds whose function is that of furnishing the pressures needed in blasting and in the propulsion of projectiles. As an example of a photochemical decomposition of widespread importance, we have the action of light on silver bromide, which is the basis of the photographic plate. Solid reactions are not limited, however, to chemical decompositions occurring with the liberation of gases, for interaction between chemical substances can occur in the solid state. Many catalysts used in chemical industry are chemical compounds manufactured by heating two or more solid substances together. Also, the corrosion of metals, which in the first place is a reaction between the metal and oxygen, depends on reactions occurring in the oxide layer. In order to understand the mechanisms of such processes, many researches have been carried out, with a view to discovering the underlying principles, and an account of some of the more important of these is given in the present article.

Classification of the Reactions of Solids

The classification of reactions involving solids is a matter of considerable difficulty and any classification made at the present time is bound to be provisional. For the purpose of this article, it is proposed to adopt a classification suggested by Roginski, which discriminates between solid reactions on the basis of the steps in the reaction which involve either the formation or destruction of

a crystalline lattice. The reason for this choice is to be found in the fact that the crystallographic processes which occur in solid reactions often determine the character of the phenomena which are observed, and this is especially true for endothermic processes. A somewhat extended and modified form of Roginski's classification is given below.

I. *Reactions involving no Crystallographic Steps.*—These are reactions which take place without change of solid phase, and are divisible into two types, (*a*) reactions in which the products form solid solutions with the reactants, and (*b*) reactions which are virtually chemisorption occurring on the surfaces of the solid. The former includes (1) those minute changes which are brought about when the alkali halides are acted on by light, and (2) reactions whereby a gas either enters or leaves a crystalline lattice, examples being the dehydration of zeolites, and the reaction of oxygen with Fe_2O_3 or Cu_2O. An essential characteristic of this type is a high mobility of some ion, atom, or molecule through the lattice, and the presence of empty spaces which they can fill; (*b*) is included since chemisorption plays a large part in certain reversible solid reactions, such as, for example, the combination of CO_2 with oxides.

II. *Reactions involving one Crystallisation Step.*—These include the combination of gases to give solids and also the formation of solids by combination of substances present in solution. Since in this class nuclei of a new crystalline phase must be formed, there is frequently considerable supersaturation in the gaseous or liquid phase.

III. *Reactions involving the Destruction of a Crystalline Lattice.*—Examples are the decomposition of HgO, NH_3. NI_3 and KN_3, in which the solids are completely converted into volatile products.

IV. *Reactions which involve both the Creation and Destruction of Lattices.*—This forms a very large class and includes the following types:

$$(a)\ A_{\text{solid}} \rightarrow B_{\text{solid}} + \text{gas},$$

examples being the decomposition of some explosives, and the action of light on the photographic plate.

$$(b)\ A_{\text{solid}} \rightarrow B_{\text{solid}} + C_{\text{solid}},$$

such as the decomposition of silver acetylide, and

$$(c)\ A_{\text{solid}} + B_{\text{solid}} \rightarrow C_{\text{solid}},$$

such as the formation of spinels from mixed oxides.

In addition to this classification it is necessary to bear in mind, that exothermic and endothermic changes may present different features, especially in the classes III and IV. Roginski has laid

down the generalisation that, for changes where the energy liberated, $Q + E$, is small or negative, the factors controlling the formation or disappearance of the lattice will take control and the reactions will be limited to a regular interface between two solid phases. Such reactions occur in the dehydration of hydrates, where $Q + E$ is negative, whereas if $Q + E$ is large and positive, the energy liberated may be sufficient to enable the reaction to penetrate the lattice in an irregular fashion, giving rise to a highly disperse distribution of a second solid phase in the first.

Also, in exothermic processes, chain reactions become possible, and when the chain length becomes infinite, the reaction is propagated linearly at a high speed throughout the solid. This linear propagation may pass into a detonation wave, where the velocity of propagation exceeds that of sound.

This classification should be borne in mind, but since it is impossible in the short space of this article to work systematically through each section, only some of the more important facts and points at issue will be dealt with. It is clear, on glancing through the classification, that (*a*) the nature of the electronic, ionic, atomic or molecular mobility through solids, (*b*) the production and the growth of nuclei, (*c*) the activation energy of solid reactions, and (*d*) the conditions under which the solid reaction can pass into deflagration or detonation, will be worth detailed attention. It is also necessary to remember that many of the properties of solid reactions are "structure" sensitive, *i.e.* they depend on the irregularities of the lattices of "real" crystals.

Diffusion in Solids

The movement of atoms, ions, or molecules within crystals may occur (*a*) by passage of the particles along the internal surfaces of the crystals, *e.g.* along Smekal cracks, (*b*) by passage interstitially within the normal lattice and (*c*) as a result of the presence of vacant places within the lattice and the movement of adjacent particles into the vacant positions. There is little doubt that in "real" crystals, movement along Smekal cracks does occur, and also that in addition, either process (*b*) or (*c*) or both, can take place as well. Measurement of the electrical conductivity of salts has established that part of the conductivity is along the cracks, and part through the lattice.

The question of diffusion is linked with that of the nature of the disorder that can occur in the crystalline lattice, and it is found that in ionic crystals the smaller ions show the greatest amount of disorder. In AgI, X-ray measurements have shown that the

anions give a perfect anion lattice, whereas the smaller cations are extremely disordered. In this case, the disorder is limited to one kind of ion, and is of the type known as Frenkel disorder. In order to produce a hole within the lattice, one ion moves to an interstitial position within the lattice, as in Fig. 1A.

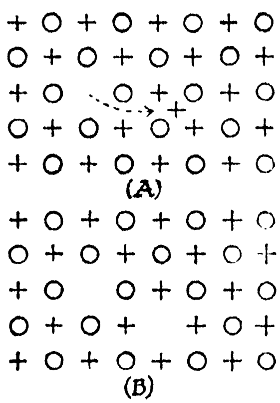


FIG. 1.—Frenkel and Schottky disorder in crystals.

A second type of disorder (Schottky disorder) occurs when for each lattice point vacated by a cation there is a corresponding hole vacated by an anion, the removed ions being used to add to the lattice on the external surface of the crystal (Fig. 1B). These types of order are regarded as being responsible both for ionic conductivity and for diffusion in "ideal" crystals. In general, the degree of disorder increases with temperature, and for any given temperature the concentration of the holes possesses an equilibrium value.

In numerous cases, crystals can be prepared which possess an excess or defect of one ion. Thus, on heating zinc oxide, oxygen evaporates, leaving zinc atoms in solid solution in the zinc. Cuprous oxide takes up oxygen which passes into solid solution. In such cases there must be cavities within the crystal due to the defect of some constituent, and in consequence there are considerable opportunities for diffusion and electrical conductivity by means of ionic movements. The study of the adsorption of gases on certain solids is made difficult by the occurrence of such movements.

Reactions of Type I.—A few examples will be given of the reactions of type I which can occur in crystals as a result of movements of ions. (*a*) When a crystal of an alkali halide is heated in the vapour of the metal, the crystal becomes coloured to an extent which depends on the pressure and heat of solution of the metal, and on the temperature of the solid. The coloration is considered to be due to the presence of neutralised cations within the lattice and according to Hilsch and Pohl, they arise in the following manner. Alkali atoms are adsorbed on the external surface of the crystal, and these atoms lose electrons thermally which pass into the solid neutralising alkali ions in their path. This diffusion of electrons sets up a space charge near the surface of the solid, which in turn causes an electrolytic movement of positive ions away from the surface and, or, movement of negative ions towards the surface, this movement serving to neutralise the space charge. When

equilibrium has been reached, neutralised cations are present, uniformly distributed throughout the lattice, and these possess a characteristic adsorption band, usually in the visible.

(*b*) Alkali halides containing alkali metal, when heated in hydrogen or deuterium, give rise to solid solutions of KH or KD in the halide by the diffusion of hydrogen or deuterium into places previously occupied by the halide ions, the crystals becoming decolorised during the process. Such solid solutions under the action of $\lambda = 228$ mμ at room temperatures give metal atoms according to the equation, $MH + h\nu = M + H$, the hydrogen atoms diffusing away. The metal atoms form colour centres with identical properties to those produced by the thermally diffusing electrons in (*a*).

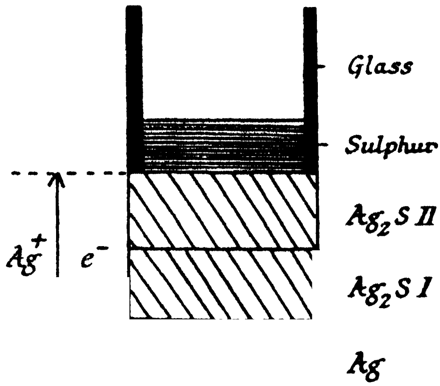


FIG. 2.—Reaction between silver and sulphur.

(*c*) With the following arrangement, Wagner has carried out the reaction, $2Ag + S \rightarrow Ag_2S$.

A series of cylinders of liquid sulphur, Ag_2S, and Ag, are taken in the above order. It is found that the upper boundary of Ag_2S II moves upward into the sulphur due to a movement of silver ions from the silver plate, moving through the Ag_2S crystals. It is shown that this movement of silver ions is accompanied by electrons. The course of the reaction can be followed by weighing the cylinders, Ag_2S increasing, and the Ag plate decreasing, in weight. Similar movements of ions through an oxide layer formed in the oxidation of metals is important in determining the velocity of corrosion.

THE CREATION AND GROWTH OF NUCLEI

The work of Tammann has brought to light the important rôles played by (*a*) the rate of formation of nuclei, and (*b*) the rate of nuclear growth in determining the velocity with which a liquid undergoes crystallisation. He has shown that the formation of a crystalline nucleus in a liquid, just below its melting point, is a relatively infrequent occurrence, and this is a consequence of the fact that small nuclei possess higher vapour pressures than matter in bulk, and hence when surrounded by a liquid with normal properties must melt at lower temperatures than the true

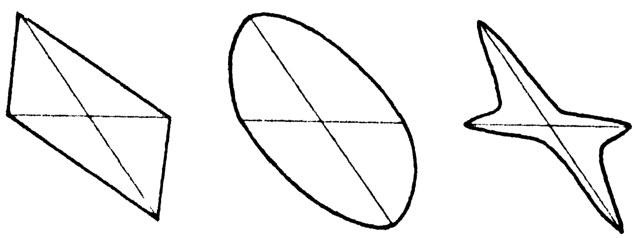
melting point. Since all nuclei must have small beginnings, there is but a small chance at the melting point that a nucleus will grow to a size sufficient to be stable in contact with the melt. In fact, the formation of nuclei is possible only because of the Smoluchowski fluctuations in the liquid. As the temperature is lowered, the probability of nuclei formation at first increases and then decreases, the decrease being due to a slowing down of the molecular movements. Similar considerations, no doubt, rule in other reversible phase transformations, small nuclei being unstable relatively to large crystals. Thus, in the formation of calcium carbonate from lime and carbon dioxide at pressures far above the equilibrium values, small nuclei are produced which, as Zawadski and Breitsnajder have shown, possess dissociation pressures higher than the true equilibrium pressures. Consequently, small nuclei of calcium carbonate are unstable in the neighbourhood of the equilibrium pressures, and hence there is but a small probability of their formation under such conditions. It is frequently observed in solid reactions that nuclei form the more readily the further the system is removed from equilibrium.

There must, however, be other reasons for the slowness of nuclei formation, for it is found to be slow even in cases where there is no reverse process to take into account, as is true for irreversible processes and for dissociation processes which are brought about in a hard vacuum. There is considerable evidence, which is supported on theoretical grounds (see the work of Volmer, Kassel and Stransky), that the first stage in the formation of a nucleus requires considerably more activation energy than the subsequent stages of its growth. Also, perfect crystals of hydrates can be maintained in a hard vacuum for many hours without the appearance of nuclei, which indicates a high activation energy for their formation on the surface of a perfect lattice. Nuclei are, however, very readily formed at places where the lattice structure has been disorganised by impurities or mechanical damage, *i.e.* at places where the activation energy for nuclear formation has been lowered. As a consequence of this, there is a greater tendency for nuclei to form on the corners and edges of crystals, since such places are the more easily damaged.

The Shapes of Nuclei.—Volmer, Kassel, and Stransky have shown that the growth of crystals in contact with the vapour or solution occurs by way of two dimensional nuclei, the crystallisation occurring in a series of repeatable steps. A nucleus forms at some point on a surface of the crystal, grows away from the surface up to a certain limiting thickness and then spreads as a thin layer over the whole surface. This process is repeated as long as the lattice

is being built up, with the result that on observing the face of a growing crystal under the microscope, it is seen to consist of a number of repeatable steps. Zawadski and Breitsnajder regard this as the normal process in the decomposition and formation of carbonates, the nuclei being two-dimensional in character and spreading over the surface of a crystal block. In the decomposition of hydrates, however, the nuclei are not of this type, being three-dimensional in most cases and where they appear to be two-dimensional, lattice penetration by the nuclei occurs.

The nuclei formed on the surface of hydrates are of three main types (see Fig. 3, which shows the shapes of nuclei associated with two axes of growth): (*a*) the nuclei possess straight edges, and potassium hydrogen oxalate hemihydrate is an example of this type; (*b*) the nuclei are circles or ellipses, and good examples are chrome alum and the haloes penetrating the crystal in the case of



(*a*) (*b*) (*c*)

FIG. 3.—Shapes of nuclei.

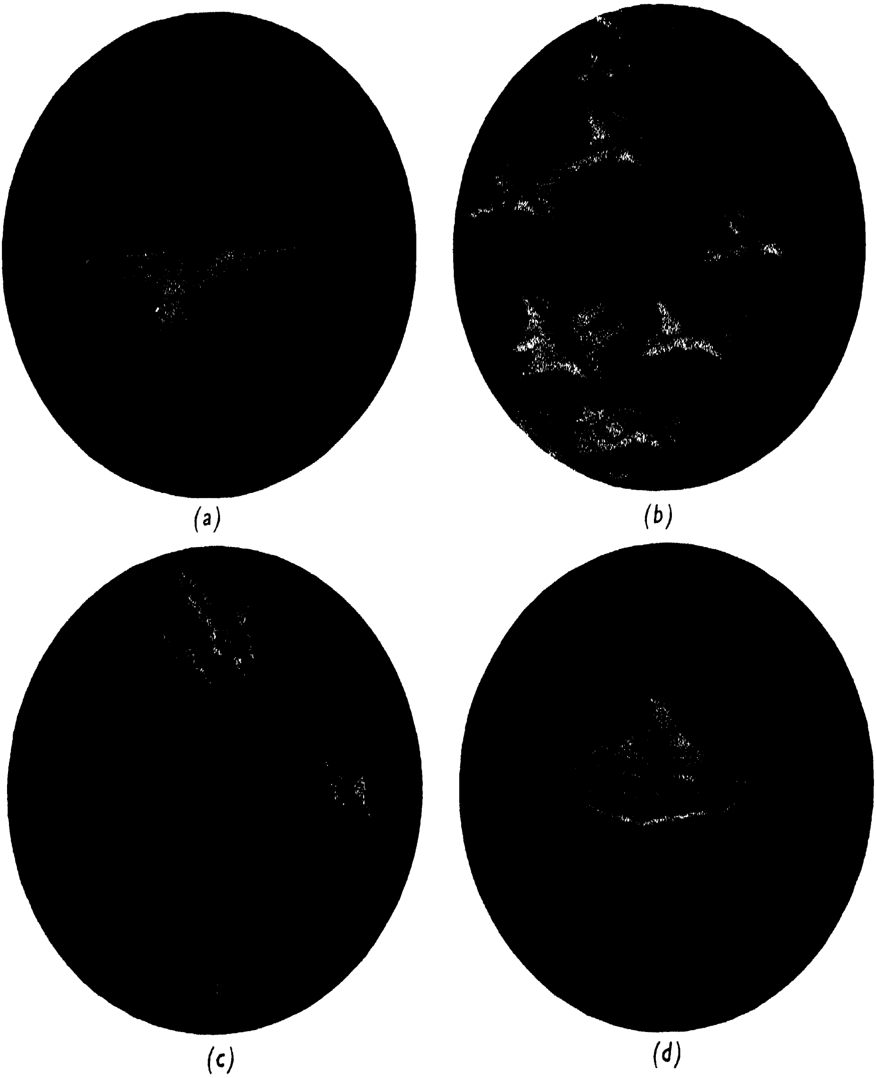
copper sulphate pentahydrate and nickel sulphate heptahydrate; (*c*) the nuclei possess horns indicating exceptional growth along special crystal planes, and the majority of the nuclei on the faces of crystals of copper sulphate pentahydrate are of this type. Photographs of some hydrate nuceli are given in Plates I and II. (*a*) is a horned nucleus formed on the 110 face of $CuSO_4 5H_2O$; the two axes of the nucleus lie on the surface, but in addition there is a halo which penetrates the crystal lattice at 56° to the surface. (*b*) shows a number of nuclei on the 110 face but photographed under such conditions that the halo is not visible. (*c*) are nuclei on the 100 face with two axes nearly at right angles, and no halo is showing. (*d*) is a nucleus on the 021 face of copper sulphate, with a marked major axis and a solitary horn; there is inhibition of the growth of one of the horns, probably due to the halo formed immediately beneath, which is faintly visible on the photograph. (*e*) is a spherical nucleus formed on the 111 face of chrome alum. The product on the edge of the nucleus is opaque but in the interior it has contracted

and undergone a process of crystallisation, causing a series of radial cracks. (*f*) shows nuclei on the 110 face of nickel sulphate heptahydrate. The sharp edge lies in the plane of the surface and the half disc penetrates the surface at an angle of 45°. This is a good example of a two-dimensional nucleus. (*g*) and (*h*) are nuclei on the 111 face of common alum and are in marked contrast with those of chrome alum with which this substance is isomorphous. The shape of these nuclei depend on temperature, and approach the spherical shape at low temperatures.

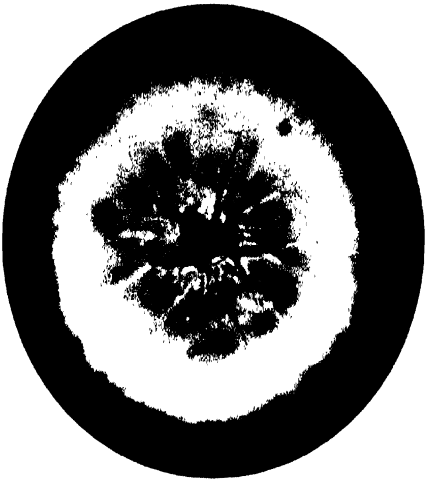
The factors controlling the shapes of the nuclei are only imperfectly understood. There is evidence that the activation energy of growth varies with the crystal direction and this no doubt plays some part in determining the shape. It is not, however, the whole story, for the rate of growth depends on such factors as the hindrance to escape of the water molecules by the product, which was named by Topley "impedance," and the production of strains within the lattice as a result of a tendency to contraction of the nucleus. Since the structure of the collapsed lattice of which the product consists will depend to some extent on the crystal direction of the interface at which the reacton occurs, the rate of escape of water from an interface and the type of strain produced will be very largely unpredictable in the case of nuclei with a complex shape. Copper sulphate pentahydrate has been most thoroughly studied and here although there is a tendency for nuclear growth to take place predominantly along certain crystal planes, notably the 010 plane, numerous irregularities are observed. Thus, in the case of nuclei on the *t*, *s*′, and *t*′ faces, one of the horns is missing (see Plate I,*d*,) and Pike has recently shown that wide variations can occur in the angle between the directions of the horns, the angle being constant for the nuclei occurring on one part of a crystal face, and this changing abruptly to another value on an adjacent area, or on another crystal. The angle is thus a structure-sensitive property.

Reproducibility, always a difficult matter in solid reactions, becomes very difficult to attain in the case of the horned nuclei, and this is most probably due to the irregularity of the strains set up. With nuclei of types (*a*) and (*b*), Fig. 3, the difficulties in the way of accurate measurements are not so great and activation energies can be determnied with fair accuracy.

In exothermic changes, there have been but few accurate measurements on nuclei. In fact, in many such processes, no nuclei can be observed under a microscope at any stage of the growth. On heating a crystal of mercury fulminate, it becomes yellow

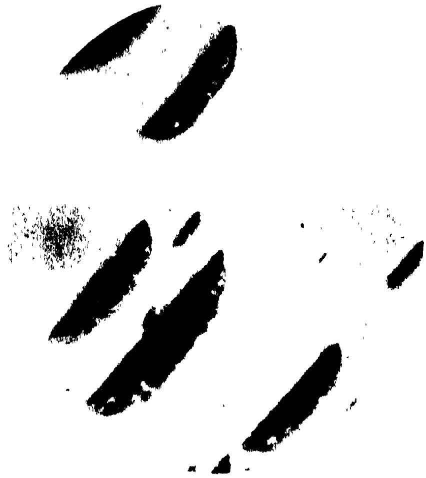


Nuclei formed in the Dehydration of $CuSO_4 5H_2O$.

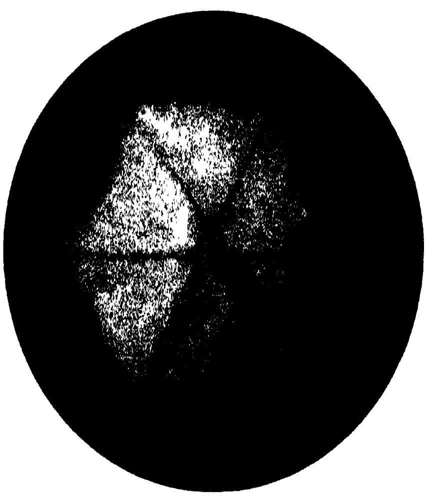


(e)

Nickel Sulphate Heptahydrate.



(f)



(g)
Common Alum.

(h)
Common Alum.

OTHER DEHYDRATION NUCLEI.

throughout in the very early stage of the reaction. The yellow colour commences on the external surface and then spreads inwards. The crystal lattice is penetrated by the reaction in a diffuse fashion, possibly by the diffusion into the interior of the solid of some autocatalyst of the reaction. A similar effect occurs in the decomposition of potassium azide ; individual crystal blocks become blue in colour and then rapidly decompose. It should, however, be emphasised that not all exothermic changes behave in this way. For example, in α-lead azide, the state of affairs is similar to that met with in the decomposition of hydrates and carbonates, metallic lead being formed on the external surfaces of the crystals. On the other hand, in β-lead azide, the nuclei appear to be much more diffuse, penetrating the body of the crystal in an irregular fashion. Since in all these processes $Q + E$ is large, it is doubtful if Roginski's generalisation (p. 211) can be regarded as sound.

The Increase in Number of Nuclei with Time.—It is generally held in work on the photographic plate that the number of centres at which nuclei can form is strictly limited, and this also appears to be true for the thermal decomposition of solids. It would be expected, therefore, that the rate of nuclei formation would depend on the number of possible developable centres and also on the activation energy of nuclei formation. If this activation energy were constant for each centre, the rate of formation of nuclei should obey the equation of the first order, giving approximately a linear increase with time in the early stages of the reaction. Experiments on $CuSO_4 5H_2O$ and chrome alum show that this is approximately true, but that in the case of $NiSO_4 7H_2O$ the number of nuclei instead of increasing proportionately to the time increases as the square of the time. Kornfeld finds similar results in the case of nucleation of strained metals, the number of nuclei increasing as the time or as the square of the time. On the whole, the experiments on nuclei formation are in agreement with the view that the activation energy of nuclear formation is not constant but varies from one centre of dislocation to another.

Rate of Growth.—The reactions of type IV occur within the interface between the reactant and its product, and the latter exerts some positive catalytic effect on the rate of growth. This is very considerable in the decomposition of potassium azide, where the rate of reaction is several powers of ten slower in hard vacuum than when the crystals are covered with a layer of metallic potassium.

The rate of movement of the interface across the crystal face has been shown in a number of examples to be constant at constant temperature for any one crystal when the nuclei are above a certain

size (approximately 10^{-2} to 10^{-3} cm. in diameter). The rate of growth is, however, a structure-sensitive property, as is shown by the variation in this " constant " rate from one crystal to another. Nuclei formed on scratches grow at a quicker rate in the beginning than later on, possibly due to the wide extent of the dislocation caused by the scratch, which lowers the activation energy over a wide area. For an unscratched surface, which is free from dust, etc., there is an induction period before the nuclei become visible which is much greater than would be expected if the nuclei started at $t = 0$ and increased in size at the normal rate. The induction period is most probably due to (*a*) the difficulty in decomposing the first few molecules, and to (*b*) an exponential rate of growth of the nuclei.

In the case of carefully prepared crystals of chrome alum, the nuclei grow more slowly in the initial stages, and as Cooper has shown, the rate of increase of diameter obeys an exponential law. Recently, Maggs has measured the rate of growth of the spherical barium nuclei formed in the decomposition of barium azide and found also that the exponential law holds. The cause of the exponential rate of increase in diameter is not known with any certainty.

The Molecular Structure of the Nuclei.—Microscopical examination of nuclei shows that the material of which they are composed undergoes slow changes with time (see nuclei on chrome alum, Plate II). These changes are probably crystallisation processes, for X-ray examination of the product in certain cases, *e.g.* copper sulphate pentahydrate and manganous oxalate dihydrate, has shown that when first separated it has no detectable crystalline structure. The view generally held is that in the case of hydrates the product has at first a zeolitic structure, which then collapses and finally crystallises. The rapidity with which crystallisation occurs varies from case to case, and when crystallisation is slow, anomalous effects may be observed such as the Topley-Smith effect.

The effect observed by Topley and Smith with manganous oxalate bihydrate and recently confirmed by Volmer is no doubt caused by the slow crystallisation of the product. These authors have measured the rate of decomposition of the bihydrate in the presence of varying pressures of water vapour (Fig. 4). The main features of this curve are explained by Volmer as due to a slow rate of crystallisation of the solid product in hard vacuum, and its acceleration by water vapour, a surmise which was confirmed by X-ray analysis. At low pressures of water vapour, the solid is in an amorphous state showing no crystal structure with X-rays, but as the pressure of water vapour increases this solid undergoes

crystallisation. The curve given in the figure is divisible into two portions. The first fall in rate occurs at the interface between the crystalline hydrate and the amorphous product and the second fall at the interface between the crystalline hydrate and the crystalline product. The initial fall is due to the effect of water vapour in retarding the rate of dissociation at the first interface and the second fall to a similar action of water vapour on the second interface. The rise which occurs after the minimum is ascribed to slow crystallisation. The Topley-Smith effect has so far been observed

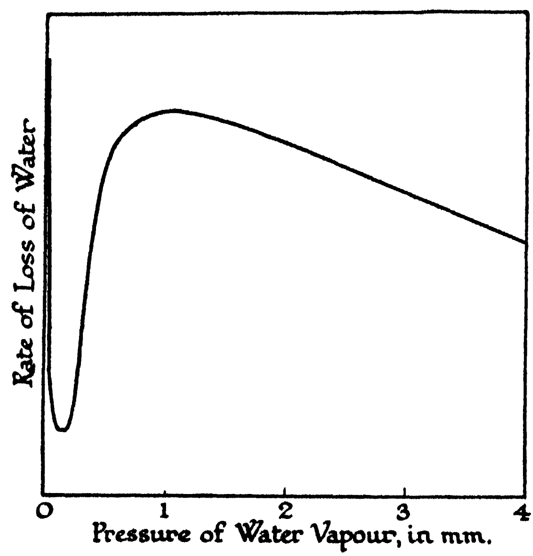


FIG. 4.—Rates of dehydration of manganous oxalate bi-hydrate.

only with manganous oxalate bihydrate, and can be expected only when the rate of crystallisation of the product is abnormally slow.

Experiments by Colvin and Hume have shown that in the decomposition of copper pentahydrate, the presence of water vapour facilitates the crystallisation and stabilisation of the intermediate hydrate, $CuSO_4 3H_2O$. In this connection, a curious result is obtained in the dehydration of $CuSO_4 5H_2O$, the monohydrate apparently separating out as a first product below 50° whereas the trihydrate is produced above this temperature. This is explained as due to the formation of amorphous trihydrate as an intermediate product at low temperatures, but which is only stabilised by crystallisation at the higher temperatures. It is tempting to explain other phenomena in terms of crystallisation processes. Thus, Spencer

and Topley observed that water vapour catalyses the decomposition of silver carbonate, which could be explained as due to a reduction of the activation energy of the reaction as a consequence of the catalysis of the crystallisation of the product by water vapour. Also, Topley and Smith have observed the catalysis of the dehydration of copper sulphate by hydrogen chloride, which may be due to a similar cause.

Activation Energies of Solid Reactions.—These are measured in various ways from the temperature coefficient of (*a*) the rates of growth of nuclei, (*b*) the induction periods in solid reactions, and (*c*) the overall rate of reaction, and where checks can be made between the various methods, the results are usually in agreement. There are considerable difficulties in the way of accurate measurements. In the first place, the energy liberated or adsorbed in the reaction is not easily dispersed, especially in work in high vacuum. Also, in reversible processes it is not easy to eliminate the back reaction, especially if the gaseous products have to diffuse through a layer of product (Impedance).

The activation energies so far measured range from 0 to 60 K. cals./gm.mol. and in general those changes which occur at measurable speeds at low temperatures have lower activation energies than those occurring at higher temperatures. There are, however, noteworthy exceptions; thus, the high explosives with activation energies in the neighbourhood of 50 K. cals. decompose over the range 100–200° C., whereas the alkaline earth azides which decompose over the same range of temperatures possess activation energies of 18–20 K. cal. The differences are due to the variation in the B factor in the Arrhenius equation, reaction rate $= B.e^{-E/RT}$. In reactions in gaseous and liquid systems, B includes the collision number Z, and certain probabilities that reaction will occur on collision, but in solids B has quite a different meaning. According to the theory of Polanyi and Wigner for surface and solid reactions, $B = \nu.N$ for each sq. cm. of reacting surface, where ν is the frequency of activation, and N is the number of molecules per sq. cm. of surface. The rate per sq. cm. of interface $= \nu.N.e^{-E/RT}$ and this gives a reasonable fit for certain solid reactions if ν is taken as the frequency of vibration of the solid lattice $\sim 10^{-13}$ sec. It is, however, essential in tests of this equation that the boundary between the solid and its product be sharp and easily estimated or measured. Agreement within one or two powers of ten has been obtained for such diverse substances as silver carbonate, copper sulphate pentahydrate, and lead and potassium azides. These can be regarded as "normal" cases of solid decomposition, where there is a reason-

ably sharp interface and where the rate of molecular activation is the maximum possible.

Exothermic Changes.—A table of activation energies for exothermic changes is given below.

Solid.	Activation Energy (K. cal.).	Range of Temp. (°).	Investigator.
Sodium azide, NaN_3 * . .	34·4	240–275	Garner and Marke
Potassium azide, KN_3 * .	36·1	222–255	,, ,, ,,
Calcium azide, CaN_6 . .	18–19	80–110	Marke
Strontium azide, SrN_6 . .	18	100–130	Maggs
Barium azide, BaN_6 . .	21	100–130	Harvey
Lead azide, αPbN_6 . . .	38	220–260	Garner and Gomm
Lead azide, βPbN_6 . . .	(37)	200–270	,, ,, ,,
Nitrogen iodide, NI_3NH_3 .	18	-30– -10	Meldrum
Mercury fulminate, $Hg(CNO)_2$	32·2	100–115	Garner and Hailes
Lead styphnate, $Pb(C_6H(NO_2)_3O_2)$ . .	(40)	225–255	Hailes
S-trinitro-triazo-benzene, $C_6(N_3)_3(NO_2)_3$ . . .	32·0	20–100	Roginski and Andreev
Trotyl, $C_6H_2(NO_2)_3$ . .	53·0	about 137·5	Roginski (*Phys. Z. Sowjet Union*, 1932, 1, 649)
Picric acid, $C_6H_2(OH)(NO_2)_3$ . . .	58·0	,, 153	Roginski
Tetryl, $C_6H_2(NO_2)_3.NCH_3NO_2$.	53·0	,, 117·5	,,
Nitroglycerin (liquid), $C_3H_5(NO_3)_3$	50	150–190	,,

* Measured when the surface is covered with a layer of alkali metal.

It is not easy to decide from this table which are the " normal " cases. Assuming that lead and the alkali azides are normal with activation energies of 34–38 K. cal. for the range 220–270°, then it is probable that the alkaline earth azides with values 18–20 K. cal. at 100° are also normal, although it has not yet been possible to measure the area of the interface to test this point. This means that all of the azides and lead styphnate possess rates of reaction in approximate agreement with the Polanyi-Wigner equation. The principal exceptions are mercury fulminate, nitrogen iodide, and the high explosives, for which the reaction proceeds at a measurable rate at lower temperatures than would be expected.

The abnormality of mercury fulminate is readily explained, since the reaction penetrates the lattice, and the interface at which reaction occurs soon becomes very large. In all solid reactions, the interface acts as a catalyst, so that the rapid rates of decomposition of fulminate can be stated as due to the production of an autocatalyst which rapidly penetrates the lattice. The high explosives, as Roginski has shown, partly from his own work and that of Farmer,

give B values of 10^{18} to 10^{28} instead of those for normal gas reactions of 10^{12} to 10^{14}. In these cases also, the crystals become coloured, and in certain cases liquefy, so that here also we have penetration of the lattice. As Hinshelwood has shown, the autocatalyst in the case of tetryl is picric acid and the lattice diffusion of this substance would account for the abnormality observed.

Endothermic Changes.—In solid reactions where heat is absorbed, in so far as these have been measured, there is qualitative agreement with the Polanyi-Wigner equation. These reactions are reversible processes, and the main interest in this section lies in the manner in which the activation energy is affected by the state of aggregation of the product at the interface, and also in the magnitude of the activation energy of the back reaction.

The activation energies in the case of hydrates and carbonates are of the same order as the heats of dissociation and this means that the activation energies of the reverse processes, *e.g.* the addition of H_2O and CO_2 to the lattice of the dehydrated product, are either zero or possess a small value. The reverse reaction is thus analogous to the condensation of a vapour on the surface of a liquid. Topley and Smith have made an accurate estimation for the reaction,

$$CuSO_4 5H_2O \longrightarrow CuSO_4 H_2O + 4H_2O,$$

and Cooper, Colvin, and Hume for the reaction

$$CuSO_4 3H_2O \longrightarrow CuSO_4 H_2O + 2H_2O,$$

finding $E = 18{\cdot}25$ and $15{\cdot}6$ K. cal. respectively. From the induction period in nuclear growth, Bright finds 16 K. cal., whereas from the work of Carpenter and Jette, the heat of dissociation is 12·6 K. cal. This discrepancy between the activation energies and heat of dissociation may be due to the finely divided nature of the product which separates in the rate measurements, and which must possess a higher internal energy than a coarsely crystalline material. In other words, the lack of agreement is due to the fact that the activation energies have not been measured for the systems in a state of equilibrium. This is the only case for hydrates where there has been a thorough investigation from all points of view. Zawadski and Breitsnajder, however, have taken these matters into account in an analysis of the results for carbonates, and have come to the conclusion that when the systems are truly in equilibrium the activation energy is equal to the energy of dissociation and that the addition of carbon dioxide to oxides occurs without activation energy. As an example, there may be mentioned the decomposition of silver carbonate which gives for E, 20·4 K. cal. and for Q, 20 K. cal. This view that the adsorption of carbon dioxide by oxides occurs

without appreciable activation energy is supported by measurements of the velocity of desorption of adsorbed carbon dioxide.

THE PRESSURE-TIME CURVES

The pressure-time curves are usually sigmoid in shape, showing in certain cases, *e.g.*, mercury fulminate, marked induction periods. We can distinguish three types of curve which will be illustrated by examples. In Fig. 5 are curves for single crystals of lead azide, lead styphnate, and mercury fulminate, which show the range obtained. The nature of the curve, however, depends on the state of division of the solid. Thus, if a large number of small crystals of lead azide be taken, or if the fulminate be ground before decomposition, curves of the intermediate type shown by lead styphnate are obtained in both cases. With hydrates all three types are found, depending on the method of handling of the crystals. An unscratched crystal gives a marked induction period, whereas if its surface be thoroughly scratched it gives a curve similar to that of lead azide. It would appear, therefore, that an induction period is a common property of all solid reactions, but that it can easily be caused to disappear by adventitious circumstances.

In the case of nuclei possessing a sharp interface, when these are above a certain critical size, the rate of reaction is found to be proportional to the area of the interface between the reactant and its product, and hence to the volume of the nuclei to the two-thirds power, the so-called law of typochemical reactions. Colvin and Hume have had considerable success in calculating the form of the sigmoid curves on the basis of simple assumptions regarding the increase in number of the nuclei with time and with the aid of the two-thirds power law. There are, however, abnormalities in the early stages of the reactions which are made evident by an exponential increase of the pressure with time. There are several causes which can operate to produce this result. In some cases, *e.g.* mercury fulminate, the interface of the nuclei is diffuse, and in others the nuclei increase in size in an exponential fashion, as Maggs has recently shown to be true for barium azide. There is also a third factor which may be operative, namely, that the numbers of the nuclei increase exponentially with time. It is practically impossible to separate out these three factors from pressure measurements alone, and there is no certainty that the inferences drawn are correct unless measurements can be made on the nuclei themselves.

From the somewhat scanty data available, it may be concluded that there are two main types of solid reaction, (*a*) those occurring

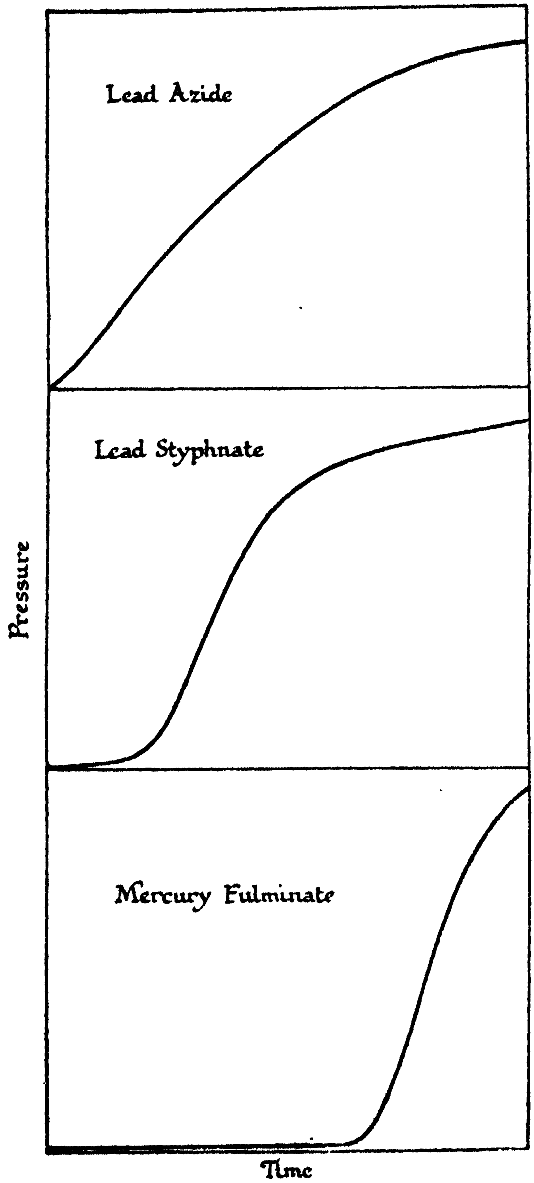


FIG. 5.—Typical pressure-time curves for solid decomposition.

at a fairly sharp interface, and (*b*) those taking place in a diffuse fashion throughout the solid lattice. In both types the pressure increases exponentially with the time in the early stages of the reaction. It is worth while examining in some detail the cases of

mercury fulminate and high explosives where the reaction appears to belong to the second category.

Mercury Fulminate.—There is a relatively long induction period followed by an acceleration of the reaction, the rate increasing as the twentieth power of the time. The reaction possesses a diffuse interface, and rapidly spreads throughout the solid lattice. The spread of the reaction obeys an exponential law in the early stages, *viz.* $\log p = kt + \text{const.}$ This can be accounted for formally in terms of a branching chain mechanism, although there is little experimental evidence on which to base a detailed theory. The branching may occur like that of a tree, among the discontinuities of the lattice, or it may follow as a result of the diffusion of an autocatalyst say, mercury ions, from the reaction zone into the body of the lattice, there to start up fresh reaction centres. If N_0 be the number of centres started thermally and k a branching coefficient, then

(1) $\dfrac{dN}{dt} = N_0 + kN_t$ and hence

(2) $\log N_t = kt + \text{const.}$,

and since $N_t \propto$ to dp/dt,

(3) $\log dp/dt = kt + \text{const.}$, and $\log p = kt + \text{const.}$,

the relationships found experimentally.

The branching coefficient k is but slightly affected by grinding the crystals, which indicates that the branching is probably occurring in the body of the lattice, and this is in support of the theory of autocatalyst spreading. Therefore, the temperature coefficient of this reaction may be that of the mobility of some ion through the crystal lattice.

High Explosives.—Roginski and his collaborators have brought to light similarities between the decomposition of nitroglycerine and solid high explosives. At low temperatures, the decompositions are autocatalytic, the autocatalysts probably being one of the end products of the reaction, *e.g.* picric acid for tetryl and NO_2 for nitroglycerine. The autocatalytic reaction obeys the Semenoff equations for branching chain reactions. At high temperatures the reactions are monomolecular with a very high B value in the equation, $k = B.e^{-A/RT}$. The high B value of 10^{20} is unexplained, it being doubtful if the monomolecular change is a chain reaction. There are thus two mechanisms of the reaction, the autocatalytic reaction occurring at the lower temperatures because its activation energy is less than that of the monomolecular reaction. Thus, for tetryl, A_{mono} is 52, and A_{auto} is 18 K. cal. The occurrence of both these reaction types for both liquid and solid explosives shows that the

lattice affords little effective hindrance to the mobility of the autocatalyst and the escape of gases, so there must be very considerable mobility in the solid lattice. The autocatalytic decomposition of high explosives is very similar to that occurring in mercury fulminate, and it is probable that the causes are the same in both cases, *viz.* the high mobility of some end or intermediate product in the solid state.

The Deflagration and Detonation of Solids

Solids, the decomposition of which occurs with the liberation of heat, can be made to detonate given suitable conditions. Sensitive explosives require only the application of heat, whereas the less sensitive can only be detonated by the application of a powerful shock. Even such an intractable material as a mixture of ammonium sulphate and ammonium nitrate can be made to undergo detonation, as was shown by the disastrous explosion which occurred at Oppau shortly after the Great War. Although many explosives cannot be detonated by the application of heat alone, it is generally found that they can be caused to ignite or deflagrate if the temperature be raised above a critical value, *i.e.* the normal thermal decomposition passes into a deflagration at an ignition temperature. The deflagration is a linear propagation of the reaction through the solid. A good example for demonstration purposes is provided by crystals of barium azide which on being placed on a hot plate become ignited at one end and are propagated like rockets through the air. If the speed of deflagration be sufficiently rapid, then a pressure pulse is generated which keeps in step with the chemical change, and the deflagration passes into detonation. Most high explosives, *e.g.* picric acid, deflagrate on heating and the rapid decomposition does not pass into detonation since the pressure pulse is not built up. In order to cause the detonation of such substances it is necessary to create a pressure pulse artificially as by the application of a shock, or by means of an initiating explosive. On the other hand, the brisant initiating substances, like lead azide, do detonate on the application of heat; in these cases, the velocity of the reaction can reach such a magnitude that it travels at the speed of the pressure pulse created by the chemical decomposition.

The transition of the normal thermal decomposition into deflagration, and the passage of deflagration into detonation, are matters of considerable interest at the present time, and although the conclusions reached as to the mechanisms of these processes are still very tentative, it is worth while to see what progress has been made in these fields.

Deflagration.—There are two main theories of the cause of ignition. The first, which is very ancient, is that deflagration arises when the heat generated by the reaction is too great to be conducted away to the surroundings, and the second, which is a modern invention, is based on the chain theory of reaction, ignition occurring when the length of the reaction chain becomes infinite. It is difficult to decide between these two alternatives, for both theories give similar mathematical relationships. It is probable, however, that ignition can arise by either mechanism according to circumstances, as has been found for the ignition of explosive mixtures of gases, where the inception of flame in hydrogen and oxygen is ascribed to the chain lengths becoming infinite, whilst the origin of flame in methane and oxygen is said to be due to thermal causes. It is of interest to examine a few cases where the ignition cannot be due to the latter.

In the case of mercury fulminate, crystals can be caused to ignite in a hard vacuum, under such conditions that the rise of temperature of the crystal due to self heating is negligible. The ignition arises abruptly out of the thermal decomposition without warning, and its occurrence obeys the laws of chance. If the ignition were due to a rise in temperature of the crystal as a whole there should be some evidence on the pressure-time curves of an acceleration of the reaction immediately before ignition, but none is found. Deflagration must be due to some event occurring during the decomposition, which has a strictly local origin. Similar results are obtained for lead azide, but here the matter can be pushed further since detonation arises from a sharp interface between metallic lead and the azide, the area of which can be measured. The lowest ignition temperature for lead azide in vacuum is 290° C., and at this temperature the number of molecules decomposing per second per sq. cm. of interface is $1{\cdot}1 \times 10^{16}$, and this means that about 30 layers of lead azide are decomposed per second. Detonation arises out of the thermal decomposition on an average about once in 20 seconds. Now if we consider what local events can arise, which have the same probability as that of detonation, we find that the chance of two adjacent azide molecules decomposing within 10^{-13} of a second is of the right order to be a possible cause of detonation, whereas the chance that three molecules so decompose occurs too rarely to be an effective cause. For two molecules to exert a "simultaneous" effect on neighbouring molecules, they must decompose within a period of 10^{-13} sec., since the period of vibration of the lattice is approximately 10^{-13} times per sec., and vibrational energy is transmitted from lattice point to lattice

point with this velocity. It is also of interest in this connection that the detonation wave passes from one lattice point to the next within the same period of time. Deflagration could therefore arise if two adjacent molecules decompose within 10^{-13} sec., provided that the energy set free by the decomposition were sufficient to activate all the adjacent molecules. The first step, as in nuclei formation generally, always requires the greatest energy, and this probably applies to deflagration so that, once started, the reaction would spread indefinitely. The energy set free in the decomposition of a gm. mol. of lead azide, $Q + E$, is 288 K. cal., and the activation energy is 38 K. cal., so that it is clear that there is sufficient energy available to activate 7–8 surrounding molecules. The initiation of deflagration wave by this means is thus a possibility.

This theory of the origin of ignition of explosives receives support from the work of Kallmann on the effect of rapidly moving atomic and molecular projectiles on explosives. α-particles and electrons which possess a very small diameter do not give rise to detonation but cause the molecules to decompose singly. However, the bombardment by ions of hydrogen, argon, and mercury, which possess a diameter large enough to activate a group of neighbouring molecules, is effective. Also, Taylor and Weale conclude from their experiments on the initiation of detonation by shock, that the origin of the detonation wave is the decomposition of a few molecules within a small volume, as a result of the friction which occurs, there being insufficient heat generated to raise the explosive to the ignition temperature.

The Detonation Wave.—The pressure pulse in the detonation of explosives traverses the solid with velocities ranging from 1500 to 10,000 metres per sec. The wave passes each molecular layer in the solid in $10^{-12} - 10^{-13}$ sec., and since it is scarcely conceivable that a molecular layer will decompose and hand on its energy to the next layer within 10^{-13} sec., it is clear that the wave front will possess a thickness of many molecular layers. The actual thickness has not been determined experimentally, but examination of solids in which the detonation wave has been extinguished shows that the width is very narrow, certainly less than 1 mm.

In the study of detonation by shock, Taylor and Weale have shown that the heat generated by the falling weight is inadequate to raise the temperature by more than a few degrees and hence the onset of detonation cannot be due to purely thermal causes. They ascribe the origin of detonation to the grinding of crystal surfaces over one another which sets up locally the activation of a few neighbouring molecules. They have measured the critical stress in

the case of mercury fulminate and find it 7 tons/cm.^{2}, acting over a time interval of at least 10^{-5} sec. As regards the detonation wave itself, they have come to the conclusion that there is a critical value of the stress set up which must be exceeded in order to ensure the passage of detonation through the solid. This raises many interesting questions concerning the action of this stress in facilitating chemical reactions in the solid. The stresses produced by detonation are very high, being 1000 tons/cm.^{2} in certain cases and such stresses are of the same order as the restoring forces in molecules. Thus, the chemical reactions in the detonation wave may be conceived as brought about by the applications of pressure alone.[1] As the detonation wave traverses the solid, the atoms of the molecules in the wave front undergo a continuous change of position under the action of the stress, and if this stress exceeds a critical value, they pass into new molecular configurations. The effect of the stress may thus be regarded as reducing the energy of activation of the reaction to zero.

In the case of thermal decomposition, we have seen that this passes into deflagration at a critical temperature, and that in the more sensitive explosives deflagration may pass in turn into detonation. In the light of what has been said above, it would appear that detonation will arise on heating a solid only if the linear propagation of the reaction is capable of producing a sufficiently high stress in the igniting layers.

Many of the topics covered by this article have been the subject of a discussion under the auspices of the Faraday Society at Bristol in April 1938, and references to original work can be found in the publication of this discussion.

[1] Eggert has shown that in the case of nitrogen iodide, the application of a gas pressure of 3000 atmospheres will cause this substance to detonate.

PLANTS IN RELATION TO THE HUMAN ENVIRONMENT[1]

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The relationships of organisms to the surroundings in which they grow and the reciprocal effects of the physical environment and of the plants and animals upon one another is the subject of study and experiment embraced in that department of knowledge, or rather aspect thereof, which we term Ecology. In this complex of living organisms and physical surroundings the human being is an all-important member. It is, therefore, perhaps not inappropriate that a plant ecologist should deal with this subject, since plants constitute so fundamental a part of our surroundings and are directly, or indirectly, essential to our continued existence. All aspects of applied biology come within the purview of the ecologist and not least those which are concerned with the hygiene, in its widest sense, of the human race. Human welfare is, in fact, a branch of applied ecology.

The consideration of communities of micro-organisms as they actually occur, comparable to the communities of higher plants, with their mutual reactions upon one another, has already yielded results that the study of individual organisms as separate entities could not alone have attained. But, important as the lower plants, such as the Bacteria and Fungi, undoubtedly are with regard to human affairs, both as individuals and as communities, it is my purpose to restrict my remarks to a consideration of some of the reactions between the more complex green plants and man, which have, I think, been too little stressed hitherto.

All of us, and all the lower animals, depend upon green plants either directly or indirectly for their energy supply. We either have our food second hand, as when we eat an herbivorous animal such as a cow, or even more indirectly if we eat the flesh of a carnivorous animal, or we obtain our food first hand from the plant,

[1] The Chadwick Lecture, delivered June 9, 1938.

as when we eat bread or a banana. But our ideas of food and food values have been revolutionised during the past two decades. The old idea that our needs are satisfied by a certain number of calories soon gave place to a recognition of the need for a properly balanced diet. More recent years have revealed the presence of the vitamins, which, though found in such small amounts, play so fundamental a rôle in the proper nutrition of our bodies. The diversity of these accessory food substances and their varying degree of development, even in different varieties of the same species or in different parts of the same organ, have placed a new emphasis upon the nature of the food we eat. Indeed, with respect to those members of the alphabet of vitamins that can be artificially prepared, there is even a risk of another lack of balance from the concept that because a little is good a lot is better.

Quite recently our attention has been drawn to the necessity for healthy plant growth of the so-called micro-nutrients. The need of minute traces of boron for the growth of sugar beet, the need of zinc for the healthy growth of the grape fruit, of minute traces of copper, manganese and iron for many plants, are but a few of the instances of essential micronutrients that have been demonstrated. Indeed, some evidence has been furnished to show that a large number of the rarer metals present in most, though by no means all, soils, are, in extremely minute quantities, actually beneficial to plant growth. Now, the plant not only absorbs these substances, but often accumulates them in its body, so that the quantity in the plant may be much greater than that in the soil itself. An instance of this is afforded by the remarkable accumulation of Selenium salts in certain American species of *Astragalus*, resulting in these plants acquiring a very poisonous character with fatal results to browsing animals.

But the particular point that I would wish to stress is that these micronutrients, transferred from the soil to the plant, may have an important influence upon the nutritive value of the plant material, not merely for animals but for man himself. This has already been spectacularly demonstrated for sheep in Australia, where it has been shown that the fatal disease known as Sheep Staggers is due to deficiency of cobalt in the herbage, which in turn is an outcome of a deficiency in the soil. So, too, the Coast Disease, of sheep fed on the calcareous dune pastures, appears to be due to a combination of deficiences in respect to both cobalt and copper. It would seem as though the views of the homeopathist with regard to the efficacy of small doses were here at least valid.

It must be emphasised that we are only at the beginning of our

study of the micronutrients, and that what applies in so startling a manner to cobalt deficiency in sheep may well obtain in greater or less degree with regard to some of the micronutrients required by man; and a more meticulous examination of the staples of diet is necessary before we can rightly assess their value, not so much perhaps for the mere maintenance of life, but for the support of vigorous health.

The indispensability of green plants to man's nutritional environment is fully recognised. So too is their importance as sources of raw materials, such as coal, timber, rubber, textiles, etc., for maintaining us in comfort with respect to our physical surroundings. But the use of plant materials as a source of heat, for the construction of our houses, or for the manufacture of our clothes, constitutes, as it were, so many protective devices interposed between us and the conditions by which we are surrounded. There is, however, a further aspect with which it is my purpose to treat, namely the effect of plants in modifying the physical environment itself to the benefit or detriment of our health.

Man is often the dominant, in the sense of the controlling, factor in his environment, and it is important that we should apprehend the effect on the welfare of the race of those changes which he himself brings about. With the object of adumbrating the influence of plant life, we will here consider the relation to human welfare not of the individual plant but of the aggregates of these as they affect man's physical and mental well being.

The general effect of plants on our surrounding circumstances is naturally an outcome of the main changes which their mass effect produces on the environment. The different behaviour of plants and animals with respect to their relative influence upon their gaseous environment has at times been exaggerated, at others minimised. Animals from their motile habits utilise a very large amount of energy and, in consequence, one of the pronounced effects of their presence upon the atmosphere around is to increase the content of carbon dioxide, as an outcome of their high rate of respiration. The organic food which the animal consumes is almost entirely utilised in the production of energy to drive the machinery of its body, and this food is oxidised with the liberation of a considerable volume of carbon dioxide. Green plants, on the other hand, being sedentary organisms, consume far less energy in respiration, but, from their possession of the mixture of green pigments which we term chlorophyll, are able to absorb the external energy provided by light and so to utilise a large volume of carbon dioxide, which is built up into complex carbohydrates, with the simultaneous

liberation of oxygen in an amount approximately equivalent to that of the carbon dioxide absorbed.

It was a too façile generalisation of these facts that led the examination candidate to conclude that if a rabbit and a cabbage were placed together in an hermetically sealed chamber they would live for ever, but the reciprocal influence of plants and animals upon the gaseous content of the atmosphere around them is a feature of great significance in the economy of nature, for this relationship of their gaseous exchanges ensure that these gases, if one may employ a financial simile, are maintained in current circulation and not accumulated in a deposit account.

A plant in full sunshine is actively respiring as well as manufacturing food, and it is therefore utilising oxygen and producing carbon dioxide in the one process at the same time as it is using carbon dioxide and producing oxgyen in the other. The total effect of the plant on the gases around is hence the balance between these two processes.

During the daytime and in the open the green plant may easily be producing oxygen and taking in carbon dioxide at twice the rate at which the reverse processes are proceeding. We might perhaps therefore expect that, in a dense growth of vegetation such as a woodland or a well-stocked garden, the atmosphere would exhibit an enrichment of the oxygen content. Actually, however, there is little, if any, change in composition, since the multitudinous microorganisms in the soil itself are producing carbon dioxide in their respiration, which often more than counterbalances the absorption of this gas by the green plant. Indeed, the carbon dioxide content of the air in a dense beechwood, when there is no wind, may actually be appreciably in excess of the 0·03 per cent. normally present.

Numerous determinations have, however, shown that, despite the factors tending towards disparity, the composition of the atmospheric air in various localities is remarkably constant.

But a word may not be out of place with reference to a matter that was at one time the subject of some controversy, namely the influence of plants and particularly of flowers in living-rooms. To appreciate the position it must be realised that the capacity of the green plant to make food at a faster rate than it is consumed in respiration is usually dependent upon the light intensity. Now, owing to the compensatory action of our eyes in dull light, we are led to imagine the intensity of illumination in a living-room is much higher than it actually is. In a living-room or bedroom the light intensity is often not more than one-fiftieth of that in the open, although it is of course lighter nearer the window and darker as

we recede from it. It may thus easily be the case that, even during the daytime on a bright day, a green plant in a living-room is barely receiving enough radiant energy to enable it to manufacture food fast enough to keep pace with the losses due to the utilisation of this food during respiration. In other words there will be little or no increase of oxygen in the air and little or no decrease of CO_2. There is a light intensity at which the building up processes and the breaking down processes just balance, known as the compensation point, and experiments have shown that this intensity varies considerably between different plants. It is very low for the aspidistra, which is the reason why this plant will flourish even in the gloom of an apartment window. With non-green parts of plants, which includes the flowers (except for their sepals and stalks), there is of course respiration but no photosynthesis, so that these increase the CO_2 content of the air and diminish the oxygen content, the more so that the rate of respiration of floral organs is often twice that of leaves and may be very much higher.

Modern physiological research has shown that a slight increase of the CO_2 content of the air may promote deep breathing, but even so the old belief that it is desirable to remove plants from a sick-room after dusk might seem to be theoretically justified, though it is very doubtful whether their influence is sufficiently marked to be significant. The slight increase of humidity they bring about might even be beneficial. Still, it is well to be on the safe side. It can be assumed that, except in still air or enclosed spaces, the effect of plants on the composition of the atmosphere is usually negligible.

The influence of plants on humidity is, however, very marked. The surface of the leaves is continually giving off water vapour and when plants are growing in the mass the amount of water emitted in this way is very considerable. It has, for example, been calculated that a beech wood during the course of a single season gives off water vapour equivalent to about 8½ inches of rain. The loss from an oak wood is probably rather less, being of the order of about 5 inches. Vegetation thus plays a not inconsiderable part in maintaining the humidity of the atmosphere.

Greenburg in a communication to the American Medical Association in 1919 adduced evidence to show that deaths from lobar pneumonia were fewer when the humidity was high than when it was low, and, apart from particular instances such as this, general experience affords evidence that prolonged periods of drought are deleterious to health, probably both from the direct effects of low humidity and from the indirect consequences of this condition such

as the prevalence of dust. Thus we see the plant world in the rôle of a regulator of the atmospheric humidity and, since plants lose more water when the air is dry than when it is moist, the presence of vegetation is an important safeguard, tending to minimise the risk of extreme aridity of the atmosphere which would be prejudicial to health.

Of the radiant energy that falls upon the surface of the foliage, and which in its absence would contribute to the heating of the soil surface and the supernatant air, more than 50 per cent. is probably utilised in evaporating water from the plant. Thus vegetation is seen in another rôle as a regulator of the temperature of the lower layers of air. Indeed, it has been shown that the temperatures in woodlands are both lower in summer and higher in winter than in the open, whilst extremes of temperature are most marked where the soil surface is devoid of vegetation.

Sir Napier Shaw brought forward evidence that low temperatures were deleterious to influenza patients, whilst Huntington showed that both low and high temperatures were inimical and less favourable to human welfare than a medium temperature. The optimum has been assessed at about 64° F. and this temperature is more beneficial if the air be moist. The ancient belief that country air is beneficial may well be due to the fact that there the vegetation provides a safeguard against those extremes of low humidity and high or low temperature which so often obtain in cities and which are so unfavourable to man. At the same time vegetation tends to check abrupt changes, so that these play their full effect as stimuli to the human body without involving the suddenness of adjustment that mitigates their value.

But another and more important aspect of vegetation is its rôle as a conserver of our water supplies. The rain that falls upon the surface of the land is largely determined by physical conditions of the atmosphere beyond these islands, so that the local conditions of surface play but a minor part in respect to precipitation. But the presence or absence of vegetation and the nature of the plant covering do materially affect the fate of the rain, dew, mist or snow that furnish our water supplies.

Of this total precipitation some evaporates directly into the air again, some is held by the soil, as by a sponge, and some passes through or over the soil to find its way into the rivers and ultimately into the oceans to repeat the circuit of its transport.

The greater the amount which is retained by the soil, the less will flow down the surface, and so there will be a diminution of erosion. The more sponge-like the soil, the slower will be the

drainage, and so the water that falls at a given time may take weeks or months to find its way into the streams. It has, for instance, been shown that the flow of the Hertfordshire Bourne is determined by the rainfall more than a month previous. Anything, therefore, that tends to hold back the water will result in a higher average water table and a more prolonged flow. It will be a safeguard alike against flood or drought.

Such a safeguard is provided by vegetation. The most retentive component of the soil is the organic material provided by the decaying leaves, and of all types of vegetation which provide a copious and continually renewed supply of humus none is more effective than woodland. Rather less efficacious is grassland. Cultivated crops still less so and bare soil least of all. The disastrous floods in the south of France which resulted from the extensive felling of forests in hilly country have been surpassed by those in the United States. The absence of the natural regulators of water supply permit the incident rain to make its way unhindered to the rivers, but this same loss of water brings in its train excessive erosion. With the advent of the dry season and the absence of any reservoir in the soil, the floods are inevitably followed by drought, and the eroded and bare surfaces are liable to further removal by the action of wind.

So we find that in America the extensive floods were but the prelude to equally disastrous droughts and to the production of the so-called "dust bowl." It has been calculated that in this way an area equivalent to nearly twice that of the whole land area of Great Britain and Ireland has been more or less ruined. The droughts and floods that have been experienced of recent years in the catchment area of the Thames are the same phenomenon on a smaller scale. If we are to have due regard for the future comfort and health of the population it is essential that our planning of the utilisation of our land surface should have due regard to the capacity of that surface to regulate our water supplies and duly conserve them.

Some idea of the capacity of forest soils to conserve moisture can be gained from a comparison of the proportion of rainfall retained by such a soil before and after the surface has been burned and the sponge-like humus thereby destroyed. It was found in two such instances that the retentive capacity was in this manner reduced in the one locality to less than a third and in another to one-fiftieth. A close turf of grass may prevent erosion almost as effectively as woodland, though the latter is more effective as a retainer of water, but land under cultivated crops is only about a tenth as effective for preventing water loss.

When we consider that probably all the heather clad moorlands of the west and north and the heathlands of the south and east of England were once woodland, that forests once clothed the Weald of Kent and the plains of Worcester—to mention but two areas that were well wooded until comparatively recent times—we realise that if conditions are not worse than they actually are it is due to luck rather than to judgment. The need for insurance against a national emergency in respect to timber has dictated our too long-delayed policy of national afforestation, but we may well ask whether this should not be combined with one directed towards the maintenance of healthier conditions ?

In the words of Professor C. Wissler, "Perhaps when the new Bible of Science is written one may read of man as the prodigal son of Mother Nature spending his heritage in riotous living, but at last reduced to the husks upon a barren waste of his own making."

If we are to escape from this we must take thought for both to-day and to-morrow regarding the implications of the changes that our activities bring about.

So far I have dealt with the more direct influence of vegetation upon our welfare, but there is another, more subtle, but none the less important, aspect of plant life, namely its amenity value.

This rôle is recognised by the existence of those properties vested in the National Trust for their value as Nature reserves which have been acquired by public and private enterprise. But it is too little realised that the recreational value of these properties, which depends upon the maintenance of the diverse flora and fauna that they support to-day, will only persist under a system of informed control that is fully alive to the natural trends of change and the requirements individually and collectively of the plants and animals concerned. To some it is the flowers that are a source of refreshment and recreation, to others it is the birds, to some perhaps the insects. But, whether it be the animal or plant life, or the combination of both, that provides balm to the jaded nerves of the urban population, it is upon the vegetation that these features depend. Active and not passive control can alone ensure that downland will not become woodland or that the feet of many visitors do not destroy those very features for which an area is prized.

For this reason the acquisition of a small number of large areas is far more to be desired than many small ones. For, with increasing facilities of transport, the obstacle that distance imposed is fast being removed, whilst a large area not only permits of greater diversity of flora and fauna, but reduces materially the difficulties of both maintenance and control.

It is in the best interests of the health of the community, mental as well as physical, that the heritage of the ages which is still available in the flora and fauna of to-day should be preserved to us and our successors in suitable chosen National Parks before their acquisition is too costly or the wild life of these islands becomes further impoverished.

To some degree, but I venture to think a smaller one, the cult of the garden plays a similar rôle in the life of many members of the community that wild country does in that of others. Horticultural pursuits certainly play no small part in maintaining the health of many members of the community, and it is therefore perhaps not out of place that I should here voice a protest against that tendency towards mass production in gardening, which tends to detract in no small degree from the recreational value of the products of the craft. I refer to the all too frequent tendency towards that imitation which results in the multiplicity of Blue Lobelias, Red Pelargoniums, etc., or the vain repetition of the same kinds of plants planted in the same kind of way in garden after garden of the same villa area. It is, I think, true to say that almost any garden possesses interest if it has individuality, but this unfortunate tendency to grow exactly the same plants as your neighbours is the very negation of personal expression and results in a monotony that must be as psychologically deleterious as the identity of house plan with which such gardens are so often associated. But this regrettable uniformity is by no means confined to the smaller type of garden, which can exhibit an individuality surpassed by none. The large garden, too, not infrequently suffers from a lay-out that, even though expensive, is as unmistakably "stock pattern." Amongst all those plants which you personally prefer find out by experiment which you can grow successfully in your garden and make a speciality of these, but never persist in stocking your garden with hospital cases that you keep alive by sheer skill. To grow the plants which look as though they enjoyed growing is the surest way to horticultural success and mental satisfaction.

Finally, I would add something as to wayside planting. Everyone will probably concede the amenity value of a well-planted road, but if we are to obtain the greatest benefits from this feature there are several important points to be borne in mind.

First, I would urge that the wayside planting must have due regard to vistas which the road user can enjoy, which implies that in general the planting of roadsides on hill crests which afford beautiful prospects is to be deprecated. As regards choice of trees or shrubs, there is a wide range available for planting in the more

sophisticated neighbourhood of towns, but in the open country the planting of native species suitable and preferably characteristic of the area through which the road passes is desirable. The variety of native trees is not inconsiderable and it adds great charm to one's journey if the same species do not continually reappear, but are associated with their appropriate areas. The Scots Fir and small-leaved birch by the roads of Perthshire would not appear half so charming if they were planted everywhere else. So too we like to see the Cornish Elm in Cornwall and the Huntingdon Elm in the Eastern counties or the stately beech trees by the roads of Buckinghamshire. Such attention to the local character of the country through which the road is passing will avoid that uniformity of treatment that leads to monotony and which is known to be conducive to bad driving. For the same reason a single species should not be employed for long continuous stretches and irregularity of vertical and horizontal contour should be aimed at.

For narrow roads the smaller-leaved species are to be preferred, whilst large-leaved species or species whose leaves, like those of the beech, are liable to packing when wet, and thus liable to cause skidding, should be utilised only where the roadside verge is of sufficient width to obviate such risk.

Plants both individually and collectively can contribute in no small degree to human health and happiness, but if they are to be utilised to the best advantage we must consider alike their influence on us and ours on them.

THE PRODUCTION AND PROPERTIES OF NEUTRONS

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HISTORICAL INTRODUCTION

SOON after the electrical theory of the constitution of matter had been generally accepted by men of science, the idea of an atomic or sub-atomic particle having no resultant charge began to be entertained by many workers. At first only the negative electron possessed the dignity of an experimentally isolated entity, but, because matter in bulk is normally uncharged, a positive counterpart of the electron was obviously required. To suggest the existence of a (possibly complex) neutral particle was then merely to go one step farther with the general scheme. Such a particle was finally discovered thirty-five years after the electron was isolated—and just about half that time after the identity of the positive counterpart of the electron had been commonly recognised—and many distinct suggestions regarding neutral particles had meanwhile been made. In the present introduction some of these suggestions will be examined for their historical significance and their general interest —and it will be noted, incidentally, that the term "neutron" was current in general scientific literature long before its final appropriation in respect of a particle the discovery of which belongs strictly to the year 1932.

Concerning almost the first of these suggestions—and probably the first use of the word neutron—it will suffice to quote from the English translation of the fourth (1903) German edition of Nernst's *Theoretical Chemistry*. Nernst's translator wrote : "It is a question of much importance whether a compound of the positive and negative electrons (neutron, an electrically neutral massless molecule) really exists ; we shall assume that neutrons are everywhere present like the luminiferous ether, and may regard the space filled by these molecules as weightless, non-conducting, but electrically polarisable, that is, as possessing the properties which optics assumes for the

luminiferous ether." Merely to remove any possible misunderstanding it might be added that the word neutron occurs in the original German; it was, therefore, taken over directly in the English version.

The next group of suggestions represents some early attempts to understand the nature of the X-rays and the γ-rays from radioactive substances. Reviewing various hypotheses in 1904, Rutherford wrote (*Radioactivity*, p. 146): "It is also possible that the γ-rays may consist of uncharged particles projected with great velocity," and, as reasonable support for this suggestion, added, "a small uncharged particle moving through matter would probably not be absorbed as rapidly as a charged particle of the same mass and velocity." Here, whilst there is no evidence to show that Rutherford really considered this possibility very seriously at all, it is interesting to note that even at this early stage he had evidently formed a very definite idea of the mode of absorption of fast particles in passing through matter, recognising the great penetrating power which a fast-moving neutron might be expected to possess. Rather later W. H. Bragg put forward a somewhat similar neutral-doublet hypothesis in an attempt to visualise the processes occurring in the production and absorption of X-rays. Two quotations from *Studies in Radioactivity* (1912) are sufficient to indicate the differences and affinities between this idea and that of Rutherford, just mentioned. Bragg wrote (p. 192): "The great bulk of X-ray phenomena are just what we should expect if we thought the electron able to neutralise its electric charge without alterations to any other of its properties or qualities"—and again (p. 191): "As an X-ray, the entity, being neutral, passes through atoms freely and carries its store of energy from point to point without loss." It is hardly necessary to add that both hypotheses have long since ceased to possess any relic of their original utility.

Suggestions of the third group belong to the period of the nuclear atom model, after the hydrogen nucleus, or proton, had already been recognised as the structural counterpart of the negative electron (Rutherford, *Phil. Mag.*, [vi] **27**, 488, 1914). These suggestions, therefore, involve heavy rather than light neutrons, and are concerned chiefly with attempts to formulate descriptive theories of nuclear structure rather than to describe the properties of radiations. Thus, in the period 1915–30, neutral particles of atomic mass 1, 2, 3 and 4 units were postulated by Rutherford (1920, 1927), Harkins (1920, 1921), Meitner (1921), Ono (1926) and others, for reasons of this kind. As later events have proved, the most successful of these predictions was made by Rutherford in 1920. Rutherford

wrote (*Proc. Roy. Soc.*, A, **97**, 396, 1920): "Under some conditions, however, it may be possible for an electron to combine much more closely with the H nucleus, forming a kind of neutral doublet. Such an atom would have very novel properties. Its external field would be practically zero, except very close to the nucleus, and in consequence it should be able to move freely through matter . . . it should enter readily into the structure of atoms, and may either unite with the nucleus or be disintegrated by its intense field. . . . The existence of such atoms seems almost necessary to explain the building up of the nuclei of heavy elements; for unless we suppose the production of charged particles of very high velocities it is difficult to see how any positively charged particle can reach the nucleus of a heavy atom against its intense repulsive field." Without much exaggeration it may be said that the neutron of 1932 was found with precisely these properties. Before leaving the above quotation, it is perhaps worth while to draw particular attention to the last remark which it contains. Five years earlier, Rutherford had written (*Popular Sci. Monthly*, **87**, 141, 1915): "On the point of view outlined in these lectures, the building up of a new atom will require the addition to the atomic nucleus of either the nucleus of hydrogen or of helium, or a combination of these nuclei. On present data, this is only possible if the hydrogen or helium atom is shot into the atom with such great speed that it passes close to the nucleus." It is not unlikely that the original hypothesis of the massive neutron arose first, at some intermediate time, in the consideration which is common to these quotations.

After hypotheses, the next step should invariably be experimental. It is characteristic of Rutherford that in this case, as with any less speculative suggestion, he immediately set on foot investigations designed to test his ideas. On his suggestion both Glasson (*Phil. Mag.*, [vi] **42**, 596, 1921) and Roberts (*Proc. Roy. Soc.*, A, **102**, 72, 1922) carried out experiments in an attempt to detect the production of neutrons, by combination of protons and electrons, in an electric discharge through hydrogen. Each of these experiments yielded a negative result; Glasson was unable to detect any penetrating component in the hydrogen canal rays by ionisation methods, and Roberts showed that if sub-atomic energy were released in the discharge (energy of combination of proton and electron) this was less than 0·5 per cent. of the electrical energy which was dissipated as heat. In spite of these results—and positive results would in fact have been very surprising—Rutherford did not abandon the idea of the neutron as a structural unit for nuclear theories. He referred to it again in papers written in 1924 (*J. Franklin Inst*, **198**,

725, 1924), 1925 (*Phil. Mag.*, [vi] **50**, 889, 1925), 1927 (*Phil. Mag.*, [vii] **4**, 580, 1927) and in *Radiations from Radioactive Substances* in 1930 (p. 523). In 1929 an apparently anomalous result in an experiment on the disintegration of aluminium by α particles (Rutherford and Chadwick, *Proc. Camb. Phil. Soc.*, **25**, 186, 1929) was followed up "in order to test . . . whether the particles of very long range might possibly be neutrons" (p. 189). It was found that they were not. Soon afterwards many more investigators began to take count of the hypothetical neutron in theoretical discussions (*e.g.* Langer and Rosen (1931), and Urey (1931)). Finally, three years later, the recognition of another artificial disintegration anomaly opened the way to Chadwick's discovery: in the case of beryllium, Chadwick showed that disintegration particles were emitted having all the properties of neutrons. We shall return to the details of this discovery presently; meanwhile, brief reference must be made to another more recent neutral particle hypothesis to which we shall also be forced to recur at a later stage in the discussion. In 1931 Pauli re-introduced the idea of a neutron of small mass in order to be able to retain the principles of conservation of energy and momentum in the description of radioactive β-particle emission. Experimental evidence in favour of this particle—now referred to as the neutrino—is slowly accumulating: at present, however, no more definite pronouncement can be made. Certainly the neutrino hypothesis is amongst the longer-lived unproved notions of modern physics.

The Discovery of the Neutron

In view of what has already been said it will scarcely appear as surprising that the radiation which was first shown to consist of neutrons should initially have been thought of as γ-radiation. In its broad outlines the history of re-interpretation in this case is well enough known, if it is recorded here, therefore, it is with particular emphasis on one aspect often insufficiently appreciated—the complication introduced by the circumstance that γ-rays as well as neutrons happened to be present in the complex radiation originally investigated.

In 1930 Bothe and Becker (*Naturwiss*, **18**, 705, 1930; *Z. Physik*, **66**, 289, 1930) used a point counter to study the possible production of γ-radiation when light elements are bombarded by α-particles. The anomaly with aluminium which Rutherford and Chadwick had investigated in the previous year had been one of a number of indications that nuclear γ-rays might possibly be emitted from certain light elements which, under α-particle bombardment,

suffered artificial disintegration with emission of protons. Bothe and Becker found evidence for such γ-radiation with boron, magnesium and aluminium—and also with lithium and beryllium, two elements from which protons had not been observed. They investigated the penetrating power of the beryllium radiation, and found it more penetrating than the γ-rays from any known radioactive product. This interesting result attracted much attention, and Curie (*Comptes rendus*, **193**, 1412, 1931) and Joliot (*Comptes rendus*, **193**, 1415, 1931) carried out independent investigations of the radiations from beryllium and boron, respectively. These investigators used an ionisation chamber at atmospheric pressure and found in each case that the radiations produced under α-particle bombardment were more penetrating than the hardest radioactive γ-rays. Absorption coefficients even smaller than those obtained by Bothe and Becker were deduced from these experiments. It is now clear that insufficient importance was attached to this numerical discrepancy: it did not occur to anyone to suggest that the point counter and the ionisation chamber might possibly be registering chiefly the effects of different radiations. We now know that such was in fact the case. About this time, also, Webster (*Proc. Roy. Soc.*, A, **136**, 428, 1932) made a thorough survey of the penetrating radiation from light elements, using both a tube counter and a high-pressure ionisation chamber. If corresponding results had been of the same accuracy in these experiments (actually the tube counter was used only in preliminary work with a weak source of α-particles) an important discrepancy would not again have been missed. As it was, Webster discovered another result equally difficult of explanation. He found (in the ionisation chamber experiments), with beryllium and boron, a difference in penetrating power between radiations emitted in different directions. In each case the radiation emitted in the direction of motion of the α-particles used in the bombardment was found to be harder than that emitted in the opposite direction. Clearly this result is difficult to understand of a quantum radiation. Then, further investigations of Curie and Joliot (*Comptes rendus*, **194**, 273, 1932) produced a most unexpected observation. Influenced perhaps by the consideration that in penetrating power the beryllium radiation approached more nearly than did any other radiation of terrestrial origin the penetrating cosmic radiation—and by certain results in that field of investigation then of topical interest (see Millikan and Anderson, *Phys. Rev.*, **40**, 325, 1932)—Curie and Joliot made an intensive search for possible disintegrations which might be attributed to the radiation from beryllium. No such effects were

observed, but they found that protons of long range were projected from all hydrogenous materials. Similar results were obtained with the penetrating radiation from boron—and differences in range of the protons observed in the two cases showed that characteristic nuclear radiations were involved. Whilst recognising the great difficulty in understanding theoretically the large magnitude of the effect, Curie and Joliot suggested tentatively that the protons which they had observed originated in a Compton scattering process suffered by an electromagnetic radiation of high quantum energy.

As soon as these results had been announced, Chadwick (*Nature*, **129**, 312, 1932) investigated the possibility that the radiation from beryllium might be able to set in motion the nuclei of atoms heavier than hydrogen. Using an ionisation chamber and proportional amplifier, he detected the recoil atoms of each of the seven lightest elements and also of argon. Moreover, his experimental method enabled him to make a reasonable estimate of the relative energies of projection of these nuclei. It was found that the rate of decrease of recoil energy with increasing mass was considerably less rapid than was consistent with the quantum-scattering hypothesis. Chadwick pointed out that all the energies were in good agreement with those calculated on a neutral particle hypothesis assuming elastic collisions, if the mass of the neutral particle was about unity on the atomic scale—and, furthermore, that this hypothesis was able to explain naturally the directional asymmetries which had been observed. This new suggestion was accorded immediate and wide acceptance. Very soon it was established that not only from beryllium and lithium, but also from several elements from which protons had previously been observed, neutrons were emitted, along with γ-rays, under α-particle bombardment (see Chadwick, *Proc. Roy. Soc.*, A, **136**, 692, 1932; **142**, 1, 1933; Becker and Bothe, *Z. Physik*, **76**, 421, 1932). Since then examples of the production of neutrons in transformations produced by protons, by deuterons, by γ-rays, as well as by other neutrons as bombarding particles, have rapidly multiplied. Here, however, the later history of this aspect of the matter does not further concern us.

The "Exact" Mass of the Neutron

As long as it was imagined that the neutron was a stable combination of a proton and an electron, it was natural to suppose that its mass was slightly less than the mass of the hydrogen atom. Various classical theory calculations were made on this basis at an early stage (see, for example, Braunbek, *Z. Physik*, **77**, 534, 1932, also, in this connection, Harkins and Wilson, *Proc Nat. Acad. Sci.*

1, 276, 1915) and the estimate which Chadwick first derived empirically from a consideration of the conservation of mass-energy in the disintegrations of boron appeared favourable to this assumption. If, as we now know, the contrary result has since proved to be correct—the neutron mass is in fact greater than the mass of the hydrogen atom—this neither invalidates Chadwick's method of calculation nor confirms the original high value put forward by Curie and Joliot (*Comptes rendus*, **197**, 237, 1933). Chadwick's method depended upon the use of the accepted values of atomic masses, which have been considerably modified in recent years (see *Reports on Progress in Physics*, **2**, 77, 1936 ; **3**, 69, 1937), that of Curie and Joliot was based upon a false assumption concerning the nuclear transformations involved. What at one time provided a lively controversy now retains, therefore, nothing but historical interest, and we may proceed at once to indicate the method by which our present more trustworthy knowledge has been obtained. This method combines an accurate determination of a small mass difference by means of the mass spectrograph with an estimate of the binding energy of the deuteron deduced from the results of photo-disintegration experiments.

In 1934 Chadwick and Goldhaber (*Nature*, **134**, 237, 1934) first showed that irradiation of heavy hydrogen with the hard γ-rays of thorium active deposit resulted in the liberation of protons and neutrons throughout the gas. These particles come from the disintegration of the heavy hydrogen nucleus by γ-rays of $2{\cdot}62 \times 10^6$ e.V. energy : if, therefore, we can determine the kinetic energy of the particles, we can calculate, by subtraction, the minimum energy which is necessary to effect the disintegration. This energy represents the difference in mass between the complex nucleus, the deuteron, and its constituent particles, proton and neutron. Chadwick, Feather and Bretscher (*Proc. Roy. Soc.*, A, **163**, 366, 1937) have deduced the mean kinetic energy of the protons from measurements of range in the expansion chamber. Because of the near equality of mass of proton and neutron—and the small momentum of the quantum—the mean energy of the neutrons is the same within allowable limits. On the basis of these results (concerning which see also Bethe, *Phys. Rev.*, **53**, 313, 1938), if symbols represent atomic masses, as is the usual practice, we may write

$$H^1 + n^1 - H^2 = 0{\cdot}00238 \pm 0{\cdot}00007 \text{ mass units.}$$

The mass spectrographic data concern the small difference in mass between the singly charged ions of molecular hydrogen and those of atomic deuterium, respectively. Here the determinations of

Aston (*Proc. Roy. Soc.*, A, **163**, 391, 1937), and Bainbridge and Jordan (*Phys. Rev.*, **51**, 384, 1937) are in excellent agreement. We may write

$$2H^1 - H^2 = 0{\cdot}00152 \pm 0{\cdot}00001 \text{ mass units.}$$

Subtracting again, we obtain

$$n^1 - H^1 = 0{\cdot}00086 \pm 0{\cdot}00007 \text{ mass units,}$$

for the difference required. It will be observed that this result is independent of an accurate knowledge of the mass of any atomic species. In addition, although it has been obtained by considering one case of disintegration only, it may be said that none of the well-established disintegration data throws serious doubt on its accuracy : if the mass of the hydrogen atom is 1·00812, that of the neutron is 1·00898 ± 0·00007 on the atomic scale.

Neutrons as Structural Units in Nuclei

It may be taken as the uncontested result of numerous experiments that the resultant charge on any nucleus is an integral multiple of the charge on the proton, and that the mass of the nucleus is represented quite closely by a whole number on the atomic scale ($O^{16} = 16$). Moreover, it is regarded as axiomatic that for any except possibly the lightest nuclei a complex structure must be assumed. This being the case, it is clearly consistent to picture all nuclei as constituted entirely of protons and neutrons—for it will always be possible to arrange for the correct nuclear charge, whilst still obtaining a positive binding energy for the system.

Theoretically, the neutron-proton model has two great advantages in comparison with any other hypothetical scheme. In the first place, the number of ultimate particles in any nucleus is less than would be obtained on any other assumption, and, secondly, electrons, in particular, are not required as structural units. We still have to describe the process of electron emission in β-disintegration, but we are not continually embarrassed by the difficulties which, on any quantum mechanical treatment, attend every attempt to formulate a theory of the motion of a particle of such small mass as the electron in so restricted a space as the nucleus provides (see Rasetti, *Elements of Nuclear Physics*, 1937, p. 172). In addition, we escape a direct conflict with experiment, regarding the type of statistics applicable to certain light nuclei (see Bethe and Bacher, *Rev. Mod. Phys.*, **8**, 82, 1936, § 4).

Before investigating further the properties of this nuclear model, however, it will be interesting to consider first the concept of the "fundamental" or "elementary" particle (frequently, though

loosely, used in discussions of this kind) and to enquire under what circumstances, if at all, the conceptual entities proton and neutron may be thought of as belonging to this class. Two types of criterion may be suggested in pursuing this enquiry. On the one hand, we may approach the matter empirically and regard any particle as elementary concerning which there is no need to suppose that it is ever transformed into two or more entities of a different kind—any such classification, however, is continually liable to modification as new modes of transformation are discovered—or we may attempt some more formal definition. We may say, perhaps, that an elementary particle is one for which some particular type of wave equation—say Dirac's relativistic wave equation—is valid. Whichever definition is adopted, clearly the electron must be regarded as an elementary particle : at first sight, however, neither neutron nor proton can be accepted as elementary, on either basis. Thus, the fact of β- (i.e. electron and positron) disintegration requires the transformation of neutrons and protons in complex nuclei—and the size of the proton, as determined in scattering experiments, is already so large that Dirac's equation must be inapplicable, having regard to the mass of the particle (see Gamow, *Structure of Atomic Nuclei and Nuclear Transformations*, 1937, p. 58). Also, the magnitudes of the magnetic moments of proton and neutron raise similar difficulties (see Bethe and Bacher, *ibid*, § 45). Even as early as 1932 Carlson and Oppenheimer (*Phys. Rev.*, **41**, 763, 1932) had concluded that the properties of the newly discovered neutron were not those of an elementary particle according to our second definition. Having recognised these general results, the next step in any systematic discussion of nuclear structure is a consideration of the forces which operate between the various constituent particles. Because here we are concerned primarily with the fact that neutrons occur as constituent particles in positively charged nuclei, we shall limit our consideration almost exclusively to the nature of one of these forces—that between neutron and proton.

There are three important aspects of the question of the neutron-proton force which may now be distinguished : there is the problem of the interaction of the free particles, determining the scattering of neutrons in gaseous hydrogen, there is the problem of the formation, dissociation, and possible stationary states of the deuteron, the permanent combination of proton and neutron, and, finally, if current views are correct, there is the problem of β-disintegration, in which we assume that the transformation of one particle into the other is involved. It is just because the probability of β-disintegration does not depend markedly upon the charge or mass of the

β-active nucleus, when a given amount of energy is available, that it is possible to regard this last simply as a problem in neutron-proton interaction. It so happens that in this case the first step in the discussion of the whole question has proved to be the important one. In 1932, within a few months of the discovery of the neutron, Heisenberg (*Z. Physik*, **77**, 1, 1932) put forward the suggestion that the force between neutron and proton is an " exchange " force—a force having similar characteristics to the valency force in an elementary diatomic molecular ion. Forces of this type are not envisaged in classical theory ; in wave mechanics they arise whenever two particles are of such a nature and are so situated that back and forth exchange of charge or identity is possible between them. If, for example, it were possible that back and forth exchange of an electron could take place with a proton and a neutron in close proximity, an " exchange " force of attraction or repulsion would be expected to operate. There is no particular distance-dependence characteristic of exchange forces, but they exhibit saturation properties similar to the saturation of valencies in chemical binding. If certain exchange conditions in the neutron-proton case are so chosen that saturation is reached when two protons and two neutrons are in close association (see Majorana, *Z. Physik*, **82**, 137, 1933)—that is when a subsidiary α-unit has been formed—a general explanation of a number of regularities in isotope statistics and nuclear mass defects is naturally obtained. Calculations employing non-exchange (" ordinary ") forces between neutron and proton have been made by Wigner (*Phys. Rev.*, **43**, 252, 1933) and by Massey and Buckingham (*Proc. Roy. Soc.*, A, **163**, 281, 1937), as well as by other workers. The possibility that calculations on such a basis may suffice to describe the interaction of these two particles must, of course, be borne in mind in reviewing the experimental material.

In experiments on the scattering of neutrons by protons two main features are important—the angular distribution of the scattered neutrons (or of the protons projected in the process of scattering) and the total cross-section per proton effective for the process. It is important that each of these features should be studied in its dependence on the energy of the incident neutrons, over a wide energy range, if any crucial test of alternative theories is to be made on the results of scattering experiments. Unfortunately it is not at present easy to obtain mono-kinetic neutrons except for one energy only (about $2{\cdot}5 \times 10^6$ e.V.)—although, in the restricted range below 100 e.V. energy, various more or less closely defined energy groups may be studied by indirect means (*v. inf.*). For neutrons of $2{\cdot}5 \times 10^6$ e.V. energy the angular distribution has been

investigated very thoroughly by Dee and Gilbert (*Proc. Roy. Soc.*, A, **163**, 265, 1937), using the expansion chamber. Since their results can be explained on the basis of a neutron-proton force of almost any type, so long as it is a short-range force, they add little to any interpretative scheme which postulates such forces. Previously short-range forces had generally been adopted for theoretical discussions, though with less justification (see Thomas, *Phys. Rev.*, **47**, 903, 1935). Similarly, information regarding the energy dependence of the total cross-section for the scattering of neutrons by protons at first provided no straightforward differentiation between various types of force—although it yielded instead new knowledge concerning the stationary states of the deuteron. The large scattering cross-section for neutrons of very small energy could only be interpreted in terms of a (real or virtual) stationary state of energy very close to the dissociation energy of this nucleus. However, this is itself a result which any theory of the forces must eventually explain. Recently, the scattering of slow neutrons has been investigated in liquid para-hydrogen, as well as in the high-temperature equilibrium mixture ("normal" hydrogen) (see Dunning, Manley, Hoge and Brickwedde, *Phys. Rev.*, **52**, 1076, 1937). Very different molecular cross-sections have been found, showing opposite types of energy dependence, for the two modifications. From the numerical data it has been inferred that the above-mentioned excited state of the deuteron is a virtual state, and from the general qualitative results of the experiment it has been concluded that the neutron-proton force is certainly dependent on the relative spin directions of the particles. Whilst, previously, this possibility had often been considered—it was implicit in Heisenberg's original theory—this scattering experiment provides the first definite proof (see, however, Casimir, *Physica*, **3**, 936, 1936).

After the scattering experiments have been fully considered, our knowledge of the forces is very little further advanced by discussing experimental results concerning either the photo-disintegration of the deuteron or the reverse process, the radiative capture of neutrons by protons. Theoretically, on each set of results alone it should also be possible to decide between a real and a virtual excited state, but with less certainty than before. Again, if no other more cogent reasons for the assumption were forthcoming, merely from the observed angular distribution of the protons in the process of photo-disintegration (see Chadwick, Feather and Bretscher, *Proc. Roy. Soc.*, A, **163**, 366, 1937) we might conclude that the ground state of the deuteron was of zero orbital momentum (S state). This, of course, is the usual, and the simplest, assumption :

the ground state is taken as a ^{3}S state and the excited state which we have been considering as a ^{1}S state of the system.

It has already been stated that a third aspect of the question of the neutron-proton force is to be found in the problem of β-disintegration. In 1934 Fermi (*Z. Physik*, **88**, 161, 1934) developed in considerable detail a suggestion which in its essentials was being commonly discussed at that time, namely that β-particle (negative electron) disintegration involves the transformation of a neutron into a proton within the radioactive nucleus and the emission of two particles, not one, from the nucleus. Emission of the second particle, the neutrino, was postulated so that the mechanical conservation principles and currently accepted rules concerning the statistics of nuclei could be retained. Fermi's development of this idea followed the general methods of radiation theory: electron and neutrino were supposed "created" in the process of emission much as a quantum is created in the de-excitation of an excited atom—and neutron and proton were regarded as different inner quantum states of a single heavy particle. In a sense, it will be observed, the way was clear for re-admitting this ideal particle into the category of elementary particles already discussed. Finally, when it was necessary to explain the phenomenon of positron emission, this was done merely by introducing complete symmetry into the theory. It was not long before it was realised that in the mechanism proposed for β-disintegration the possibility of a definite exchange process between neutron and proton was explicitly admitted. The attempt was then made to unify the theories of the neutron-proton exchange force and of β-disintegration. Numerically, this attempt encountered most serious difficulties (see Nordsieck, *Phys. Rev.*, **46**, 234, 1934): the exchange force associated with the phenomenon of β-disintegration (exchange of an electron and a neutrino) was too weak, by a factor of roughly 10^{12}, to explain nuclear binding energies and scattering, so long as it were assumed to have the experimentally determined range of action. Until very recently only highly speculative suggestions have been made regarding means of removing this enormous discrepancy. However, one of these speculative suggestions, due to Yukawa, now bids fair to become the basis of the accepted explanation. In 1935 Yukawa (*Proc. Phys. Math. Soc. Japan*, **17**, 48, 1935) suggested that the main interaction between proton and neutron is connected with the exchange (virtual emission and re-absorption) of a charged particle of mass about 100 times the electron mass—and with spin quantum number which is integral, not half-integral as for the other particles concerned. This assumption

regarding the spin of the particle avoided the necessity of postulating the exchange of two, at that time unknown, particles between neutron and proton. During 1937 evidence began to accumulate that positive and negative particles of about this mass have a transient existence in the penetrating cosmic radiation (see, for example, Street and Stevenson, *Phys. Rev.*, **52**, 1003, 1937 ; Williams and Pickup, *Nature*, **141**, 684, 836, 1938). Naturally the result has been a very great renewal of interest in the problem of the exchange force between neutron and proton. Theoretically, several possibilities were open, but these have rapidly been reduced in comparison with the experimental material already discussed. Bhabha (*Proc. Roy. Soc.*, A, **166**, 501, 1938) has shown that one particular variant of the theory (with a particle of unit spin) gives precisely the type of exchange force best suited to describe the scattering results, and also predicts, without further hypothesis, a ^{3}S specification for the ground state of the deuteron. This seems a very promising start for the new theory. From the experimental side, however, it is clear that, in the spontaneous disintegration of nuclei, the emission of a heavy electron (as the new particle is at present not very happily named) cannot possibly compete with ordinary β-disintegration until the amount of intra-nuclear energy available is very much larger than it has ever been found to be. It is most unlikely that such an amount of energy should be available, except during the collision of a very energetic particle or quantum with a nucleus.

Since present results appear to show that quite strong forces operate, within the nucleus, between pairs of neutrons (and between pairs of protons), it has frequently been suggested in recent months that a new neutral particle of intermediate mass should also be postulated. So far, however, this suggestion remains entirely speculative.

Nuclear Transformations produced by Neutrons

It so happens, because of the negligible interaction between neutrons and electrons (see Dee, *Proc. Roy. Soc.*, A, **136**, 727, 1932) and the short range of the forces between neutrons and nuclei, that, for a high energy neutron traversing matter, the process of absorption by means of a disintegration or other inelastic nuclear collision possesses quite a high probability (of the order of $\frac{1}{10}$ to 1), for most substances. This explains the relative ease with which these processes were discovered at an early stage (see Feather, *Proc. Roy. Soc.*, A, **136**, 709, 1932). For most of the gases which could conveniently be used in the expansion chamber, an appreciable fraction

of the heavy particle tracks obtained on irradiation with "fast" neutrons proved to be the paired tracks of the charged products of disintegration. The experiments of Fermi and his colleagues (*La Ricerca Scientifica*, 5(2), 282, 1934) first showed that, when the energy of a neutron has finally been reduced to a very small (thermal) value in elastic collisions, it is almost certain to be captured by an atomic nucleus, whatever substance it traverses. It would appear that, in this last phase, the only possible competition arises from the spontaneous β-disintegration of the neutron (after which a proton remains)—the half-value period of this transformation being probably of the order of a few hours. For the most part, Fermi's experiments were carried out by observing the induced radioactivity of the species formed by neutron capture—or, occasionally, by observing the γ-radiation emitted during the capture process. In order to produce beams of neutrons of thermal energies the method of transmission through hydrogenous organic material was employed (disintegration processes do not occur with hydrogen and are rather infrequent with carbon nuclei, and the rough equality of the masses of neutron and proton ensures a large transference of energy at each collision between the two). This method has since become standard and has been very widely used. Moreover, because most important results have been obtained in these transformation experiments with slow neutrons, the rest of this section will be devoted entirely to discussing them (see Moon, *Reports on Progress in Physics*, **4**, 198, 1938).

Except with the light elements lithium, boron and nitrogen, so far as is at present known all transformations produced by slow neutron irradiation proceed by capture of the neutron and emission of radiation. One of the most striking and earliest recognised features of this phenomenon is the wide range in efficiency of capture exhibited by different elements. For some nuclei the cross-section effective for slow neutron capture is of the same order as the geometrical target area of the nucleus, for a few others it is about a thousand times as great. Moreover, no regularity of mass or charge is apparent by which the highly efficient or the poor absorbers may be grouped together. When this result was considered along with the fact that the cross-sections of nuclei for the scattering of slow neutrons show no such wide variations, being of the order of the geometrical cross-sections in every case (as an example of a recent experiment, see Goldhaber and Briggs, *Proc. Roy. Soc.*, A, **162**, 127, 1937), the inadequacy of all early theories of the phenomenon (*cf.* Bethe, *Phys. Rev.*, **47**, 747, 1935) was immediately evident. These theories predicted a roughly constant ratio for the

efficiencies of capture and scattering for all nuclei. Bohr (*Nature*, **137**, 344, 1936) and Breit and Wigner (*Phys. Rev.*, **49**, 519, 1936), independently—and from somewhat different viewpoints, gave the first acceptable explanation of the experimental results. For this explanation attention is fixed on the complex system (nucleus + neutron) temporarily formed at the moment of collision—and its success depends essentially on the assumption that in all cases, for an energy quite close to the total energy of the system so formed (say within a few hundred electron volts—or, occasionally, when the capture cross-section is greatest, within a few electron volts of that energy), there should exist a stationary state of the system having a relatively long life-time (say 10^{-13} to 10^{-15} sec.). Because with slow neutrons the system (nucleus + neutron) must generally be formed with an energy of excitation of several million electron volts, this assumption requires that the average spacing of the energy levels should not be greater than about 100 e.V. at this energy, for any moderately heavy nucleus. On Bohr's nuclear model, such a requirement is naturally fulfilled. On account of the short range of the forces and the close packing of the system, excess energy is almost always shared in a type of collective motion in which all the particles in the nucleus are involved. Except for small energies of excitation, then, a multiplicity of closely spaced energy levels must result. It was not long before there was direct experimental evidence for the existence of sharp energy levels of the type which was required (see, for example, Amaldi and Fermi, *Phys. Rev.*, **50**, 899, 1936). Following up certain anomalies previously reported by Moon and Tillman, as also by Bjerge and Westcott, Amaldi and Fermi and, independently, Szilard, found a very pronounced selective absorption of medium-slow neutrons in various elements. Estimates of the peak energies and effective widths of the absorption bands have now been made in a number of cases. It may definitely be said that this particular experimental domain is developing precisely along the lines predicted by the theory of Bohr.

The method of estimating slow neutron absorption energies is sufficiently interesting in itself to merit brief description. It depends upon the existence of a strongly absorbing substance for which the absorption cross-section varies monotonically—and in a known manner—with the energy of the neutrons. For the strongly absorbing light elements lithium and boron (absorption in which results in disintegration, not in radiative capture of the neutron) a cross-section inversely proportional to the neutron velocity is generally predicted by theory. Over the lowest velocity range

this has also been unambiguously verified by direct experiment (Rasetti and others, *Phys. Rev.*, **49**, 104, 1936). So long as the $1/v$ law is correct, clearly the decrease in intensity of a beam of slow neutrons, in passing through a given absorber, should not be altered by any relative motion of the neutron source and the absorber. Within the accuracy of the experiment no alteration was found with an absorbing screen of boron, when an over-all difference of velocity with respect to the source of about 2×10^4 cm./sec. was employed. The energies of slow neutron groups, therefore, are regularly determined by absorption measurements with boron, no serious inconsistencies having as yet been discovered in extrapolating the $1/v$ absorption law as far as required. For the standardisation of the method, knowledge of a single neutron energy is necessary : for this purpose it is general to suppose that the neutrons emerging from a sufficient thickness of hydrogenous material have already reached thermal equilibrium with the material. Again, there appears to be a large volume of independent experimental support for this supposition.

Investigations of the γ-radiations emitted when slow neutrons are captured are of great importance, but also of considerable technical difficulty. On the basis of the neutron-proton model, the total energy radiated is the increase in nuclear binding energy between one nucleus and the next ; apart from theory its determination makes possible an accurate estimate of the difference in mass between neighbouring isotopes. Already, much has been done in this field and a general survey made of the effective energies of the capture radiations for most elements. But interpretation is difficult because of an interesting effect. It is now clear (see, for example, Aoki, *Proc. Phys. Math. Soc. Japan*, **19**, 799, 1937) that the emission of neutron-capture radiation is frequently a cascade process in which several quanta are successively emitted from a single nucleus. A further study of this phenomenon, even if it emphasises the difficulties in calculating mass differences from the data, as above suggested, will certainly also provide a large amount of additional information of great interest.

The Magnetic Moment of the Neutron

Interest in the theoretical discussion of the magnetic moment of the neutron has recently been considerably revived by the success of the first direct experimental determinations of this constant. Even if these experiments hardly do more than provide an order of magnitude result, this in itself is important in the present state of theory. Previously, estimates of the neutron moment had been

based entirely upon the measured values of the mechanical and magnetic moments of proton and deuteron—and upon assumptions regarding the specification of the ground state of the latter nucleus. If this state were assumed to be a triplet S state, and if neutron, as well as proton, were assigned a spin quantum number of $\frac{1}{2}$, then the magnetic moment of the neutron could be obtained by simple subtraction: $\mu\ (n) = \mu\ (H^2) - \mu\ (H^1)$. Precisely on this basis a neutron moment of $-2{\cdot}0 \pm 0{\cdot}2$ nuclear magnetons was generally accepted. The new experiments have confirmed this result as regards sign—and, numerically, at least as regards order of magnitude (see Powers and others, *Phys. Rev.*, **52**, 38, 1937; Frisch, von Halban and Koch, *Phys. Rev.*, **53**, 719, 1938).

The successful experimental method, which was first suggested by Bloch in 1936 (*Phys. Rev.*, **50**, 259, 1936), involves the polarisation of a beam of slow neutrons by scattering in a ferromagnetic material. The extent of the polarisation (in the actual experiments of the order of 2 per cent.) provides an indication of the magnitude of the neutron moment, the sense of rotation of the polarisation direction in an auxiliary magnetic field enables the sign of the moment to be deduced. Because of the definite result of the experiments there can no longer be any question of attempts to explain existing data on the assumption of zero magnetic moment for the neutron.

An interesting suggestion concerning the origin of the magnetic moments of neutron and proton has been worked out by Fröhlich, Heitler and Kemmer (*Proc. Roy. Soc.*, A, **166**, 154, 1938) on the basis of the theory of the heavy electron, already referred to. Using plausible assumptions to limit the divergence of the integrals occurring in the calculations, values of the correct order of magnitude and correct sign have been obtained in each case. For these calculations the magnetic moment of proton or neutron is assumed to arise on account of the virtual emission and re-absorption of a heavy (positive or negative) electron from the particle in the free state. In its essential features this suggestion is the same as that put forward by Wick (*Att. Acad. Lincei*, **21**, 170, 1935) on the theory of β-disintegration—only now the heavy electron replaces the electron-neutrino pair of the earlier theory. On either assumption, to a first approximation, the "additional" moments of proton and neutron would be expected to be equal and opposite: "intrinsic" moments would be one nuclear magneton and zero, respectively. It is generally favourable to these suggestions that the difference in the absolute values of the magnetic moments of the two particles is, in fact, not very different from one nuclear magneton.

THE IDENTIFICATION AND CORRELATION OF COAL SEAMS

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THE rocks known as the Coal Measures consist chiefly of clays, shales and mudstones, often grey in colour; associated with them are coarser deposits, including beds of sandstone sometimes hundreds of feet in thickness. In some areas in Britain the total thickness of the Coal Measures exceeds 8000 feet; of this thickness the coal seams form a very small proportion, and as only certain seams are economically workable the search for a particular coal in a great mass of rather monotonous sediments calls for the most accurate information regarding both the succession of rocks and the peculiarities of individual seams.

These difficulties are most serious in the early development of a coalfield or of an unexplored part of a coalfield, and in other areas where the strata are highly disturbed, as for instance, by faults. In the former case, the sinking of a pit or a trial boring reveals the succession of strata beneath, and it may show the presence of various coals at different depths; the identity of the coals may be easily determined if one has some peculiarity such as unusual thickness. But it is possible that sinking for a thousand feet will yield evidence only of thin seams of similar appearance, and from the evidence available it may be uncertain whether the seams sought are to be found at greater depths or whether they are represented in the area by thin and unworkable seams, so that further exploration is useless.

In the case of areas where the rocks are highly disturbed, the difficulty of identification of seams may be much increased and may give rise to problems of great urgency on many occasions during the working of a single colliery. For in such a case, the coal may be worked up to a fault, so that the seam terminates abruptly along a given line. The driving of galleries in different directions in the rock beyond the fault may reveal worthless coals (as at X in Fig. 1) but not the seam which has been lost; if the

position of the worthless coal in the sequence is known, so that the position of the worked coal can be estimated, the problem of economic mining is greatly simplified. In the absence of any criteria for distinguishing the seams, however, much exploration has frequently been necessary to decide whether the workable coal can be profitably looked for in the region beyond the fault; if the fault is of great throw the coal may be too deep for mining from the existing shaft. The structure illustrated in Fig. 1 of course suggests a very simple problem, but it must be remembered that the mining engineer may

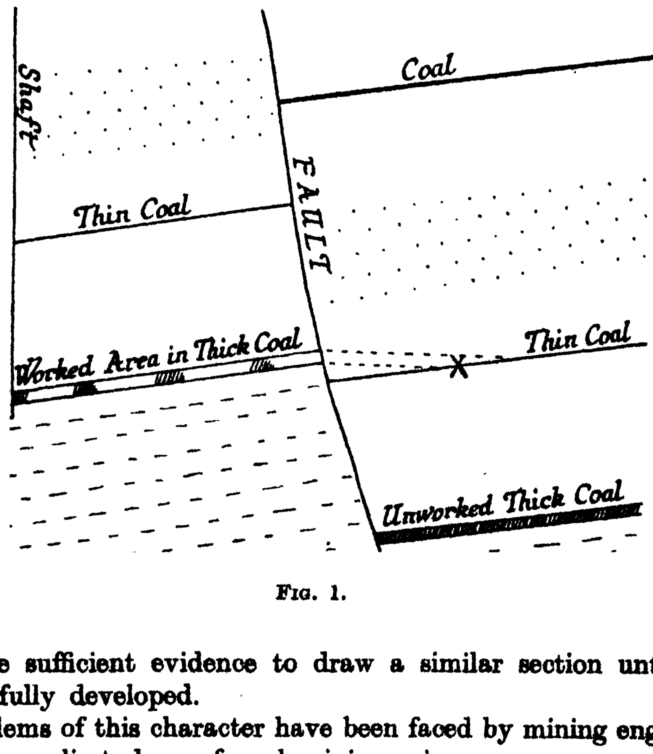

FIG. 1.

not have sufficient evidence to draw a similar section until the mine is fully developed.

Problems of this character have been faced by mining engineers since the earliest days of coal mining; in some cases geologists have been able to collaborate, and the officers of the Geological Survey in particular have devoted great attention to the interpretation of the structures of most of the coalfields and to the nature of the faults affecting them. The identification and correlation of seams, however, have presented peculiar difficulties, and for many years the data available for accurate work on the Coal Measures has been less satisfactory than that provided for workers on the Chalk, the Jurassic and the Lower Palæozoic rocks, which in economic importance cannot be compared with the Coal Measures.

These difficulties in the correlation of the Coal Measures have arisen mainly from the nature of the sediments and the fossils found in them. For as has already been suggested, the sediments are on the whole of uniform character, showing a fairly regular rhythmic succession of coal, shale or mudstone, sandstone and fire-clay ; the recognition of even the general position in the sequence is often impossible by a consideration of the rocks alone.

The utilisation of the fossils found in the Coal Measures has presented difficulties for many years, and while other rock groups possessing a similar uniformity of lithological types (such as the Chalk and the Carboniferous Limestone) have been divided into zones by means of their fossils, the zoning of the Coal Measures has only been accomplished within quite recent years. The mining engineer has thus been compelled to rely mainly on other criteria, such as the thickness and character of seams and associated sediments.

The nature of this evidence may most usefully be discussed first.

Thicknesses and Character of Associated Strata

The irregular distribution of the coal seams within a great thickness of other sediments often makes it possible to compare sections of the Coal Measures in adjoining areas and to make correlation of some of the individual seams. This is usually done by drawing to the same scale sections showing the sequence proved in neighbouring collieries or boreholes : by matching the thick seams, or similar groups of two or three closely placed seams, it is often possible to demonstrate the similarity of the distribution of the coals so that correlation of the two areas can be made (Fig. 2).

This method is greatly extended by introducing into the sections some details concerning the rocks other than the coals : the presence of thick beds of sandstone at corresponding positions helps to confirm conclusions already formed, while the occurrence of bands of peculiar lithology, such as white quartzites, red shales, conglomerates or thin beds of limestone, supplies even more valuable information. In some cases the positions of the fireclays are of great assistance in correlation. This results from the fact that fireclays generally represent the "seat-earths" or fossil soils on which the coal-forming vegetation grew ; most fireclays are succeeded by a coal seam, though possibly of insignificant thickness. Since many seams vary greatly in thickness, the absence of any record of a coal at a particular level is not so significant if a fireclay is observed there. In some continental countries (*e.g.* Belgium) the

positions of fireclays are regarded as of great importance in comparing sections of adjoining areas. This method depends on the fact that the formation of the Coal Measures was marked by rhythmic changes in depth of water and other conditions, affecting considerable areas simultaneously ; a coal seam presumably marks a period (perhaps a very long period) when the water was exceedingly shallow and plant growth was extensive, while the succeeding strata must

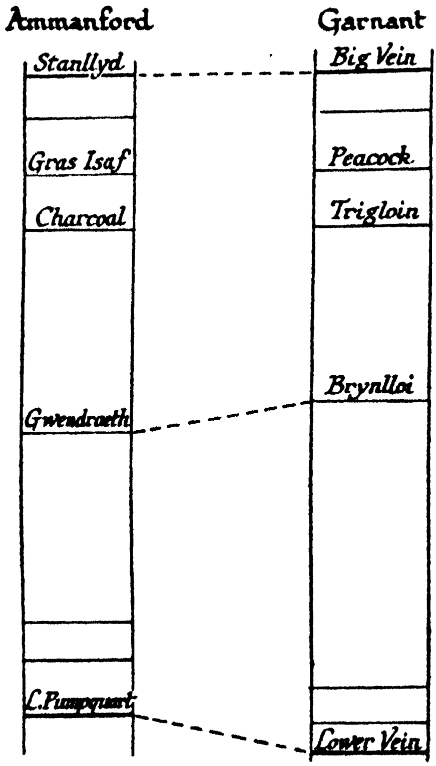

FIG. 2.

mark a subsidence causing a deepening of the water. These changes often affected wide areas and thus the corresponding rocks can be correlated.

But while comparisons can be made in these ways between neighbouring areas, each step in the correlation may be open to some doubt, and correlations extended for great distances on this basis cannot be regarded as other than provisional. Yet this method has been used with conspicuous success, as by the officers of the Geological Survey in their work in South Wales, where a general

correlation, made over many hundreds of square miles, has only been modified in detail since other information has been available. Using such criteria also Professor G. Hickling made a correlation of the Lancashire coalfield; Dr. D. A. Wray achieved results of even wider application, with some assistance from palæontological data, in making a correlation of the coalfields of Yorkshire, Lancashire and North Staffordshire.

It is clear, however, that comparisons of this type can only be made where an accurate section of a considerable thickness of the strata at any locality is available: if only a portion of the section is known the problem is naturally more difficult, and while many engineers and miners become so familiar with the characters of individual coal seams and the strata immediately in contact with them that they can recognise them at places some distance away, the lateral variation in these characters often makes identification impossible.

The Fossils of the Coal Measures

Some of the fossils of the Coal Measures are familiar objects, which are extremely abundant in many localities. They have been freely collected and illustrated, and it is surprising that their utilisation in zoning was so long delayed. This delay was partly due to the belief that the various species characteristic of the Coal Measures have long vertical ranges, and that since they extend through very great thicknesses of strata they cannot be employed in the recognition of separate subdivisions of the formation. This belief has been shown to be partly erroneous: certain species are now known to extend through a comparatively small thickness of rocks, and while some species (and in some cases, the commonest species) have more extended ranges, the fossils are of much more value in correlation than was once supposed.

The Coal Measures yield great numbers of fossil animals as well as the more familiar plants. The vertebrates are rarely of use in correlation, although occasionally a band rich in fish remains forms a distinctive and well-marked horizon traceable over a wide area. By far the most useful fossil animals are the "mussels," lamellibranchs which are believed to represent fresh-water (or, more cautiously, non-marine) conditions, since they are never found in the same beds as the known marine faunas. The marine faunas occur only in thin bands, separated by great thicknesses from which marine fossils are absent, and it appears that the majority of the Coal Measure sediments were deposited in fresh water (or at least under non-marine conditions), the marine bands representing wide-

spread but quite temporary incursions of sea water into the lagoons.

In the detailed correlation of Coal Measures the importance of the marine bands was recognised at an early date, but it will be more convenient to deal first with the fresh-water fauna.

The Non-Marine Lamellibranchs

The most abundant non-marine shells are named *Carbonicola* (Fig. 3 *a, b*); they closely resemble the modern fresh-water mussel, *Unio*. They give rise in some places to conspicuous "mussel-

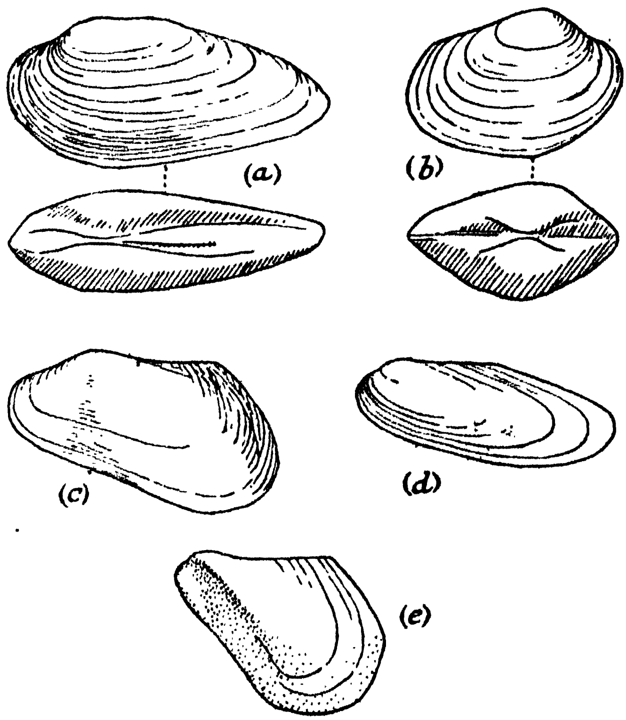

Fig. 3.—Non-marine shells from Coal Measures.

(*a*), (*b*) *Carbonicola* [(*a*), *C. atra*, Trueman; (*b*) *C. bella*, Davies and Trueman]
(*c*) *Anthracomya* [*A. robertsoni*, Brown];
(*d*) *Anthraconauta* [*A. tenuis*, Davies and Trueman];
(*e*) *Naiadites* [*N. triangularis*, J. de C. Sow]
All approximately natural size.

bands" several feet in thickness, but elsewhere they occur more sporadically in the shales or mudstones, being especially common within a short distance above many seams. Three other genera of non-marine shells have also been named, *viz. Anthracomya, Anthraconauta* and *Naiadites*, the latter having a considerable obliquity

of shell and a reduced anterior end, as in the modern *Mytilus*, to which it is regarded as related (Fig. 3 *e*). These shells are represented by a good number of species, many of which were named almost a century ago. That they have been comparatively little used in the study of Coal Measure correlation is at first surprising, but it has long been supposed that fresh-water shells from the nature of their habitat would be likely to show extreme variation and adaptation to the differing conditions in different parts of the Coal Measure lagoons: accordingly, the reliability of conclusions based on such organisms has been regarded as open to question.

Any casual examination of the shells from within an area of a few square yards of a single bed in the Coal Measures shows the extreme variability of these forms: careful collections statistically examined have shown that in many cases such a small area will yield examples of a homogeneous community in which there is an extraordinary range of variation between the extreme members, but in which intermediate forms link the whole together. Thus specimens which are so different as to appear to be members of different species are connected by innumerable transitional stages. At a higher or lower horizon some similar forms may be recognised, similarly linked with a wide range of variants, but these may not include all the forms seen at the previous horizon, while possibly including others not represented there.

According to the Linnean system of nomenclature, each such homogeneous community should doubtless be regarded as a "species"; but these shells raise difficult problems of nomenclature, for the palæontologist who refers the first group to a given species must either refer the second to the same species (on the ground that a proportion of the forms are indistinguishable), in which case his "species" will be valueless as criteria for separating these horizons, or he must find some method of giving different names to distinct forms. Probably the nomenclatorial difficulties have been responsible, more than anything else, for the delay in using these shells; the problem even now is far from solution, and only a working basis of nomenclature has been adopted.

Quite apart from any names given to the various forms of fresh-water shells, however, it is apparent after a very little experience that in some areas the roofs of many seams contain faunas which are sufficiently distinct to be characteristic of those particular horizons. Many years ago the late Dr. Wheelton Hind and Mr. J. T. Stobbs showed that many of the coals in the North Staffordshire coalfield could be identified by means of the faunas occurring in the beds immediately above them. Little was done to extend

this work for some years, but in 1923 similar studies were made in the South Wales coalfield. Throughout the anthracite area, extending for many miles along the northern part of the coalfield from the Vale of Neath into Pembrokeshire, it was found that almost every seam could at once be recognised by means of these faunas: in several cases the shells above a seam were so distinctive that one or two specimens were sufficient to enable the identity of a seam to be determined at places fifty miles or more apart, but in other cases more careful study was needed. Correlation proved easiest along the belt named, on the northern border of the coalfield, which represented a tract extending roughly parallel with the original land margin near which the strata were deposited; correlation with less distant areas lying to the south proved more difficult, for some of the faunas (which presumably lived under somewhat different conditions farther from the shore) included representatives of different species.

This use of the shells for the identification of individual seams has been extended to other coalfields with varying success, as might be expected from the facts outlined above. The method has proved of most value in South Wales, where, especially in the anthracite area, the strata are often very complicated in structure and where the seams are broken by great numbers of faults. The shells have thus yielded information of great economic value in the working of collieries even where the sequence of strata is already well known. Previous errors in the correlation of coals (which have led to mistakes in the naming of seams worked) have been rectified and in some cases this has made it possible to locate seams previously overlooked owing to the wrong identifications.

But while the shells have thus proved of importance in this detailed work in comparatively small areas, they have also provided a basis for much broader recognition of horizons over very wide areas. Wheelton Hind early in the present century suggested a broad scheme of zones for the Coal Measures of Staffordshire, but his work did not receive the recognition it deserved. Not until 1927 was a further step made in the use of these shells in zoning the British Coal Measures.[1] This further work was carried out in South Wales, a particularly fortunate region since almost the whole thickness of the Coal Measures is there represented by fossiliferous strata.

[1] J. H. Davies and A. E. Trueman, "A Revision of the Non-Marine Lamellibranchs of the Coal Measures, etc.," *Q.J.G.S.*, LXXXIII, p. 210; references to most other papers mentioned will be found in the following paper: Trueman, "A Suggested Correlation of the Coal Measures of England and Wales," *Proc. S. Wales Inst. Eng.*, XLIX, 1933, p. 63.

The Coal Measures were divided into six zones, as follows:

Zone of *Anthraconauta tenuis*;
,, ,, *Anthraconauta phillipsi*;
,, ,, *Anthracomya pulchra*;
,, ,, *Carbonicola similis*;
,, ,, *Anthracomya modiolaris*;
,, ,, *Carbonicola ovalis*.

These zones proved to be recognisable in all the coalfields of England and more recently, Scotland,[1] save that in some areas it was found impracticable to divide the strata represented by the zones of *Carbonicola similis* and *Anthracomya pulchra*, to which the name Similis-Pulchra Zone is now given; it was also found desirable to add a further zone (of *Anthracomya lenisulcata*) below the Ovalis Zone.

The broad vertical distribution of the fresh-water faunas which makes this zonal division possible depends mainly on a wholesale replacement of one fauna by another at certain stages in the Carboniferous. Thus both *Carbonicola* and *Naiadites* disappeared by the end of the time represented by the Pulchra Zone, after which *Anthraconauta* was dominant. These rather sudden changes of fauna were perhaps assisted by the changes in conditions during the deposition of the Coal Measures, for after the formation of each widespread coal seam (in conditions in which the fresh-water shells could not exist in the area) there was an opportunity for the entry of a new fauna; the formation of a marine band similarly involved the disappearance of the fresh-water fauna from a very wide area, and it is significant that the biggest change in the fresh-water faunas, at the top of the Similis-Pulchra Zone, follows an important series of marine bands.

Not only are the zones of non-marine shells recognisable in different parts of Britain, but substantially the same sequence can be traced in the Continental coalfields from France and Belgium to the Donetz basin.

The Marine Faunas

The fossils found in the marine bands of the Coal Measures are chiefly of types peculiar to muddy water, *Lingula*, *Pecten*-like lamellibranchs and goniatites, but in some areas corals and crinoids occur. Many of the species represented appear to recur in successive

[1] J. Weir and D. Leitch, "The Zonal Distribution of the Non-Marine Lamellibranchs in the Coal Measures of Scotland," *Trans. Roy. Soc. Edin.*, LVIII, p. 697.

marine horizons and apparently represent "long-ranged" forms, but a few are more restricted, and in some cases it is possible to identify a marine band as marking a particular horizon from the fossils found in it.

It was in the North Staffordshire area again that serious attention was first devoted to the marine bands in the Coal Measures, and Mr. J. T. Stobbs showed that they could be of great service in the correlation of sequences in various areas.

A marine band is chiefly of importance as affording a datum plane within the strata. For a marine band marks a single and widespread incursion of the sea, and coals formed before that incursion will always be found below the marine band; while those formed subsequently will always be above it. Since marine bands are comparatively few in number, it is often (though not always) possible to correlate the marine bands in two sections, and this carries the investigator some distance in correlating the coal seams; at least it prevents many incorrect correlations.

The use of marine bands has perhaps proved of greatest importance in the concealed part of the Yorkshire–Nottinghamshire coalfield, where boreholes and pits are sunk to the Coal Measures through a "cover" of Permian and Trias. The sections of Coal Measures revealed have been correlated most effectively when the position of a conspicuous marine band, the Mansfield Marine Band, has been determined, the seams sought occurring at distances below this which can be estimated fairly closely. This marine band occurs in the upper part of the Similis-Pulchra Zone (of the fresh-water sequence) and in cases of doubt its position has lately been checked by the non-marine shells occurring above and below it.

It is remarkable that a marine band with a similar rich fauna occurs in a corresponding position in the Coal Measures of South Wales, North Staffordshire, Lancashire, Scotland and many continental coalfields: apparently a marine incursion affected simultaneously wide areas of western Europe.

The Fossil Plants

Fossil plants are often found in great abundance above coal seams, and where shells are rare, plants afford useful information regarding the horizon. Occasionally they supply evidence which is of value in identifying individual horizons within a small area. In some cases the proportions of the different groups of plants found in the roof of a coal seam remain fairly constant over a considerable area, while differing markedly from those found over the seams above and below. The late David Davies investigated in very great

detail the relative abundance of the various plant groups at single horizons in South Wales, recording for the purpose hundreds of thousands of specimens collected above a particular seam at different places. While in certain cases the general proportions of the various plants is itself sufficient to identify the horizon, in the case of some of the higher seams in Somerset this method has so far yielded no useful results.

Plants have more recently been used for the broader division of the Coal Measures into zones comparable with the zones based on the non-marine shells. For many years their value for this purpose has been maintained and the late Dr. R. Kidston proposed four major divisions of the Coal Measures on the basis of the fossil plants. Since these divisions were several thousands of feet thick, however, and since their boundaries were very indefinite, they were never of much use for economic work. More recently, following the lines of palæobotanical work on the Continent, Dr. E. Dix has divided the Coal Measures into nine floral zones. The various species of fossil plants used in the recognition of these zones include many ferns and other plants with fern-like leaves; some of these are not always common, and in general it may be said that many of the most abundant plants (the giant horse-tails and club mosses, for example) are of little value in the zoning of these strata, for they usually have long vertical ranges. Both among fossil plants and fossil animals, the commonest forms are rarely the most useful in zonal correlation.

The Spore Content of the Coals

Two other lines of attack, chiefly on the problem of identification of coal seams, have been developed in the last few years. Both are essentially palæobotanical, but instead of dealing with the fossil plants occurring in the rocks associated with the seams, they depend on the distribution of different types of spores (produced by some of the different species of plants utilised in the method just noticed) within the coals themselves.

In the first place, an examination has been made of the megaspores found in certain coals. These may be studied in thin sections of the coals, when different types can be recognised by peculiarities in size, thickness of wall and ornamentation. The method has been used in Pennsylvania and in Upper Silesia, and in England (particularly in Yorkshire).[1] In the case of certain seams in Yorkshire

[1] D. A. Wray, L. Slater and G. E. Eddy, "The Correlation of the Arley Mine of Lancashire, etc.," *Summ. Progress Geol. Survey for 1930* (1931), Part II, p. 1; other references are given in this paper.

and Lancashire where there was some doubt as to the correlation, a series of thin slices was made from each seam, to give a complete section from top to bottom of the coal. The types and relative abundance of the megaspores in each portion throughout this thickness of coal were ascertained, and it was found that there

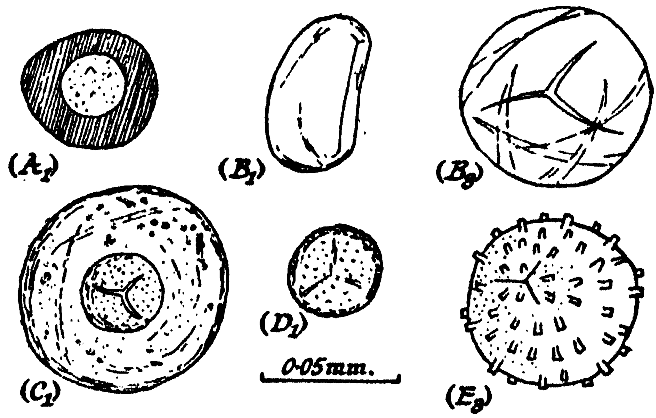

FIG. 4.—Microspores from Northumberland coals.
(*After* Dr. A. Raistrick)

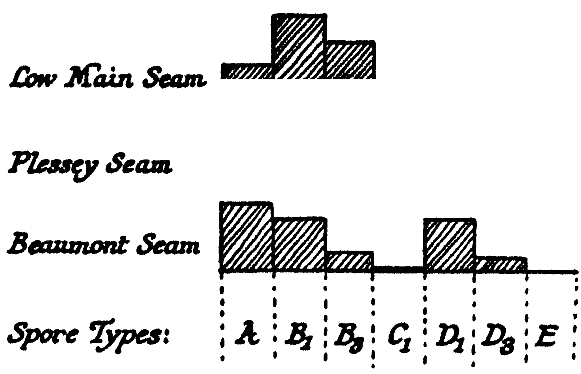

FIG. 5.—Graphs showing the proportions of different types of microspores in three Northumberland coal seams.
(*After* Dr. A. Raistrick)

was a much closer similarity in the spore content and distribution between the Arley Mine of Lancashire and the Better Bed of Yorkshire than between the former and the Silkstone of Yorkshire, which had been formerly supposed to be equivalent.

This is a long and laborious process, but it is likely to yield very important results within areas which are not too extensive.

The second method depends on the study of the microspores

of coals, and it represents a development initiated by Dr. A. Raistrick.[1] The various types of microspore are not conveniently studied in thin section, and Dr. Raistrick's method consists of isolating them by treating a crushed sample of the coal with Schultz solution (strong nitric acid with potassium chlorate). The spores are then mounted and examined under a high power of the microscope. At one stage in his investigations Raistrick treated separately each one inch of the coal throughout the thickness of the seam, but later he found that in many cases it is sufficient to grind together a representative sample of all parts of a seam, from top to bottom.

Few of the various types of microspore have yet been referred to the species of plant from which they were derived, but using a provisional system of identification the proportion of the different types in each seam is recorded graphically, as shown in Fig. 5; in Northumberland the graphs for corresponding seams show remarkable constancy, while differing markedly from other seams occurring in the same area.

This method of studying the microspore content is receiving considerable attention, and although it involves a difficult technique it represents a great development in the accurate identification of coal seams.

Conclusion

The great progress which has been made in the study of coals and of the factors which may lead to their correlation and identification during the past decade is an indication of the enthusiasm with which new methods have been developed and applied by many groups of workers, and especially by the officers of the Geological Survey and the Fuel Research Board. In this work, also, the colliery owners and officials have taken a keen interest. While the problems of coal correlation are by no means solved, the various methods now available make it possible to meet many difficulties which arise in coalfield exploration, although no single method is universally applicable.

[1] A. Raistrick and J. Simpson, "The Microspores of some Northumberland Coals, etc.," *Trans. Inst. Min. Eng.*, LXXXV, 1933, p. 225; also *ibid.*, LXXXVIII, p. 142.

THE APPROACH TO THE THEORY OF OPTICAL INSTRUMENTS

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THE subject of Geometrical Optics is naturally not amongst those in which indefinitely great progress is to be expected ; but even in this field there are many problems yet unsolved. The theory of optics can be approached and developed in a variety of ways, and it is no waste of time to give some consideration to the manner in which a subject, confusing and unsatisfactory unless dealt with in a proper manner, should be introduced to the beginner, whether in school or University. With this aim in mind the Physical Society produced in 1934 a " Report on the Teaching of Geometrical Optics." The main aim of this document is to unify, at least in part, the many conflicting sign conventions now in use. A review [1] of text-books on " Light " appearing since that time shows that the recommendations of the Report have been considered and acted upon in many cases.

The wider question of the philosophical presentation of the subject was a matter treated very briefly in the recommendations of the report, and then only in relation to the very early stages. But papers on various aspects of optical theory continue to be published by workers such as T. Smith, M. Herzberger and others ; the net effect being to enlarge knowledge of the fundamental relations implicit in the more familiar optical equations, and to give, as it were, a better perspective of the paths along which the approach may be made.

We will therefore first survey broadly the various systems which have been used for developing the theory of instruments, and then attempt to draw some conclusions as to the best presentation of the subject under certain conditions.

The most familiar of the general conceptions is that of the " collinear " relationship between the object and image spaces of an ideal " instrument." In such a relationship, the points, lines,

[1] *Nature*, **141**, 136, 1938.

and planes of the object-space have uniquely corresponding points, lines, and planes of the image-space, and the properties of the "instrument" can be described in terms of the relative positions of such conjugate planes as show magnification ratios of zero, unity, infinity, and so on. Although the whole idea of collinearity is purely geometrical, and quite independent of optical experiments, it can be connected with real optics in two ways. Firstly, if attention is restricted to the images and rays within a thin imaginary tube stretched along the axis of *any* axially symmetrical system of lenses (the paraxial region), we find relationships which approximate more closely to collinearity the smaller the bore of the tube; secondly, we find that any real instrument, to be at all useful, *must* make a further approach to collinearity in some respects at least, and thus its aberrations can be measured geometrically in terms of the departures from this ideal.

Even if the law of refraction had never been discovered it would have been simple, after establishing the collinear relationship by experiment, to justify the familiar conjugate distance relations, such as

$$\frac{f'}{l'} + \frac{f}{l} = 1$$

where l and l' are the conjugate distances of object and image planes measured from the principal or unit planes, and f and f' are the focal lengths of object- and image-spaces; also the "Newtonian relation"

$$xx' = ff',$$

where the conjugate distances x, x' are now measured from the principal focal planes; and the Abbe magnification relations

$$\frac{h'}{h} = -\frac{x'}{f'}, \frac{h}{h'} = -\frac{x}{f}$$

which give the ratio between the perpendicular distances, h and h' of object- and image-points respectively, from the axis.

We get thus a valuable working conception of the action of an optical instrument, misleading in some ways but helpful in others. The scheme has admittedly all the defects of a model; after the first conception has been obtained we can get little further with it. However, the systematisation thus rendered possible by the work of Gauss, Möbius, Maxwell, Abbe, and others is clearly of the greatest importance in the development of the subject, and cannot be lightly set aside. Many teachers prefer to introduce the ideas of collinearity by analytical equations such as used by Drude.

The equations mentioned above can, of course, be deduced by

using the law of refraction and applying it to an instrument composed of coaxial spherical refracting surfaces; but the discussion usually involves an early approximation by restriction to paraxial conditions, thus really introducing the collinear case and masking any generality which the expressions may possess. Gauss thus derived, however, the general form of equation in which the related conjugate distances are measured from the surfaces of the instrument.

From this point, if progress in optics is to be obtained the most usual course is to abandon the model, go back to the detailed study of the actual system of spherical refracting surfaces, and study its aberrations either in piecemeal manner, or in accordance with some more or less comprehensive treatment such as that of von Seidel. For example, the consideration of spherical aberration of the image of an axial object point shows that the axial crossing point of rays from a circular zone, of radius y, of the last surface is separated from the common crossing-point of the paraxial rays by an interval A where

$$A = ay^2 + by^4 + cy^6 + \text{etc.}$$

Other aberrations can be also expressed by series of similar character. It is possible to derive algebraical expressions for the coefficients a, b, c, etc., in such series, involving the radii and separations of the surfaces, the refractive indices of the media and so on, but whereas the expression for a may be fairly simple, that for b may be vastly more complex, and c almost beyond patience. In many practical cases, however, the expressions prove to be so rapidly convergent that even the first term may give a reliable indication of the defect. We thus obtain the so-called "primary" aberrations.

Results of this kind are often of little value for dealing with optical systems of large aperture which also transmit rays at large angles with the axis. Hence the design of these lenses receives but little aid from such analytical studies, and is still largely empirical.

The above rough sketch may serve to represent the way in which the major part of optical knowledge has developed, but it has often been felt that the theory of collinearity has inhibited a more natural and practical approach to the subject. The ophthalmologist in particular has to pay especial attention to the refraction of narrow pencils of rays, such as may enter the eye through a spectacle lens, and he early learns of the differing relations governing the refraction of narrow fans of rays lying in a plane containing the normal to a refracting surface at the point of incidence of the

middle ray (tangential rays) as against rays (in the same pencil) of which the initial tracks lie in a plane perpendicular to the one above-mentioned (sagittal rays).

The law of refraction allows us to obtain, without difficulty, relations governing the distances of conjugate points measured along the rays; these relations are still subject to the restriction of applying only to very narrow "fans" of rays, but they hold for refraction at any angle. They are (for a single surface):

$$\frac{\mu' \cos i'}{t'} - \frac{\mu \cos i}{t} = \frac{\mu \cos i' - \mu \cos i}{r} \quad \text{(tangential rays)}$$

$$\frac{\mu'}{s'} - \frac{\mu}{s} = \frac{\mu' \cos i' - \mu \cos i}{r} \quad \text{(sagittal rays)}$$

Note that the accent distinguishes quantities of the image-space from those of the object-space; t and s are conjugate distances in the tangential and sagittal cases respectively; μ is the refractive index of the medium; i is the angle of incidence of the principal ray of the bundle; r is the radius of curvature.

Gullstrand, a Swedish ophthalmologist, showed how to discuss [1] the image formation around such a bundle of rays after passage through any number of spherical refracting surfaces. The principal ray of the bundle must lie in a plane containing the axis of symmetry. His "object-size" is the perpendicular distance of the "object-point" from a closely neighbouring principal ray; the "image-size" is the distance of the point of re-union of the rays from the refracted path of the neighbouring ray. It can be shown in a straightforward way that the image formation can again be conveniently described in terms of conjugate distances measured from special points for which the magnification is zero, unity, and infinity (compare the principal foci and the principal points), although in general these points differ in the tangential and sagittal cases. Equations of the form quoted above as characteristic of the paraxial case are now found to apply in this wider sense, and the paraxial equations are seen to relate merely to the particular case in which the sagittal and tangential forms coincide ($i = i' = 0$). We thus avoid the notion that these characteristic equations are in any sense bound up with collinearity.

This approach provides a means whereby the excessive restriction of the earlier treatments is avoided, and the theory can be given a start along lines which are admirably suited to some investigations. Gullstrand continues his discussion by treating the ray

[1] See Appendix to the third edition of Helmholtz's *Physiological Optics*; English translation; published by the Optical Society of America.

distribution around the principal ray of the bundle when the strict limitation of angular divergence of the rays is relaxed. He casts on one side, a little contemptuously, the notion of collinearity, which, as he truly remarks, only emerges from an accumulation of mathematical singularities in the construction of an instrument. But, however elegant a discussion of this kind, it is difficult to see how a beginner could grasp, by means of it, a clear conception of the general action of an optical system.

There is, moreover, no specific need to write even the more general conjugate distance equations in a form connecting distances measured from unit points or principal foci. Just as Gauss obtained a relation between *axial* conjugate distances measured from the surfaces of a system, T. Smith showed in the appendix to the Report referred to above, that relations of the Gaussian type

$$a\frac{s}{\mu}\frac{s'}{\mu'} - b\frac{s'}{\mu'} - c\frac{s}{\mu} + d = 0$$

connect conjugate distances, s and s', measured from the surfaces along obliquely refracted rays, where the constants a, b, c, d, are analagous to those of the Gaussian memoir. Smith claims further that "the custom of referring properties to the unit surfaces involves in particular examples a certain amount of needless and useless numerical calculation."

No survey of the possible methods of approach to the theory of optical instruments can afford to omit some mention of yet another means by which an idea of certain essentials in the mode of action of an optical instrument can be obtained without a complete discussion of the details of the construction. A slight sketch of this important subject may interest some readers, as it is not nearly so well known as the "collinear" system. The principle of stationary optical path (Fermat's theorem) usually stated in the form

$$\delta\int_A^B \mu ds = 0,$$

tells us that if we calculate the time taken [1] for a disturbance to travel along a ray track from a point A to another point B and compare with it the time which would be taken along a neighbouring continuous path between A and B everywhere close to the first both in direction and position, the time along the true ray-track will be found to be a maximum, minimum or stationary.

It is not difficult to see that if a *continuous* series of true ray-

[1] Strictly speaking, we are concerned with the phase velocity, and not the time taken by a group of waves.

paths joins two points B and B′ the optical path along each ray-track will be the same; this equality can be secured by a suitable optical instrument; then the point B′ is the physical image of B. Such a constancy of optical path will, however, be exceptional, and more usually only one ray passing through a point x, y, z, of the object-space will pass through a single point x', y', z', of the image-space of the instrument. A bundle of rays can be defined by its origin from one point x_0, y_0, z_0, and loci at the same optical distance from this one point will define the wave-surfaces to which the rays are normals. Thus in the object-space we may have a wave-surface given by

$$F_1 = f_1(x, y, z) = \int_{x_0, y_0, z_0}^{x, y, z} \mu ds = \text{const.},$$

where μ is the refractive index at any point, and ds is the element of path length; and also in the image-space a wave-surface given by

$$F_2 = f_2(x', y', z') = \int_{x_0, y_0, z_0}^{x', y', z'} \mu ds = \text{const.}$$

Then the difference

$$F = F_2 - F_1$$

is clearly the optical-path length between wave-surfaces passing through x, y, z, and x', y', z'. This optical-path difference is a form of the "Characteristic function" discussed from 1828 onwards by Sir W. R. Hamilton. The publication (1930) of Hamilton's collected papers on optics has introduced his work to a wider circle of readers. If the equation to a surface is known it is an elementary theorem in analytical solid geometry to find the direction of the rays in terms of their "direction cosines," l', m', n'; thus in our case

$$\frac{\partial F_2}{\partial x'} = \frac{\partial F}{\partial x'} = \mu' l' \text{ etc.},$$

and similarly for the object-space

$$\frac{\partial F_1}{\partial x} = -\frac{\partial F}{\partial x} = \mu l \text{ etc.}$$

Similar equations hold if x, y, z, is the point of origin from which the rays diverge; thus for every point in the image-space we can, by differentiation of the appropriate function (if it be known), find the directions of the rays.

Of course, it is easy to grasp the general meaning of the characteristic function, but difficulties appear when we essay to obtain a treatment of the behaviour or aberrations of an optical system, either in terms of the departures of the rays from a certain image point in a certain plane, or in terms of the optical-path differences

of disturbances which can be considered (thinking in terms of Huygens's principle) to arrive in such a point. In order to obtain a better weapon to deal with ray-aberrations, Bruns,[1] working independently and in ignorance of Hamilton's work, developed the theory of the "Eikonal." Although Hamilton had already dealt with this function, the name given by Bruns is usually retained. It is again an optical-path length obtained as follows. Consider fixed origins O and O′ in object- and image-space respectively; from these origins we drop perpendiculars to the sections of one ray in the object- and image-spaces, the feet of the perpendiculars on the ray being P and P′.

The "eikonal" is then the optical-path length between the points P and P′. (See Fig. 1, which illustrates the case in which the object- and image-spaces have a common axis.)

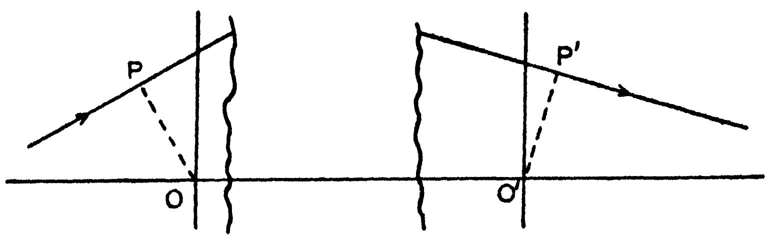


FIG. 1.

It is fairly clear that this optical-path length (W) is a function of the directions of the rays in object- and image-spaces and we may thus write

$$W = f(l,\ m,\ l',\ m'),$$

since $n^2 = 1 - (l^2 + m^2)$, and a similar relation holds for n', so that we need not put n and n' into the above expression. The direction cosines relate of course to arbitrary co-ordinate axes with origins at O and O′ in object- and image-spaces, and it can easily be shown that if we know the function W, we can get the points $(x,\ y)$, and $(x',\ y')$ in which any ray of the system intersects the respective planes $z = 0$ and $z' = 0$ from the relations

$$\frac{\partial W}{\partial l} = \mu x, \quad -\frac{\partial W}{\partial l'} = \mu' x'$$

and so on. It will be appreciated that a function of this kind will lend itself more readily to the discussion of geometrical aberrations.

We shall, in cases of practical interest, usually be dealing with axially symmetrical systems, and the axis of symmetry can be

[1] *Leipz. Sitz.-Ber.*, **21**, 323–436, 1895.

taken as the "z axis." The x and y axes will be parallel in object- and image-spaces.

The restriction to an axially symmetrical system makes it only necessary to fix three variables in order to determine W, for any skew ray in the object-space can be defined by the polar co-ordinates r_a, φ_a and r_b, φ_b of the respective points in which it intersects two planes $z = a$ and $z = b$; but since the system is rotationally symmetrical the value of W will be unchanged for all rays for which $\varphi_a - \varphi_b$ is constant. Hence three variables suffice.

For example, if we know the angle between the object-ray and the axis, the angle between object-ray and image-ray, and the angle between the image-ray and the axis: these three variables will suffice to determine the W value. The first of these will be fixed by $n^2 = 1 - (l^2 + m^2)$, the second by $ll' + mm' + nn'$, and the third by $n'^2 = 1 - (l'^2 + m'^2)$; thus the three variables are written a, b, c where

$$a = l^2 + m^2,\ b = ll' + mm',\ \text{and}\ c = l'^2 + m'^2.$$

To begin with then, all that we need assume in regard to optical instruments in general, is that the eikonal always exists. A number of alternative ways of procedure are now open. One method is to assume a pair of reference planes $z = 0$, and $z' = 0$ perpendicular to the axis of an axially symmetrical system, and to enquire what conditions will be necessary in order that (as far as the system of rays goes) the respective points of intersection x, y and x' y' coincide with the points of an object and image, the latter having a magnification M. The coincidence is attained if

$$x' = \text{M}x \text{ and } y' = \text{M}y$$

for all values of the variables.

It is not possible within the limits of the present sketch to give anything like a complete discussion, but the following picture emerges. Taking fixed reference planes O and O′, let W represent the value of the eikonal for a ray direction inclined to the axis, so that, although $a = b = c = 0$ for the axis itself, all these variables are now finite; then by Maclaurin's theorem if W_0 is the eikonal for the axial path,

$$\text{W} = \text{W}_0 + \left\{a\left(\frac{\partial \text{W}}{\partial a}\right)_0 + b\left(\frac{\partial \text{W}}{\partial b}\right)_0 + c\left(\frac{\partial \text{W}}{\partial c}\right)_0\right\} - \frac{1}{2!}\left\{a^2\left(\frac{\partial^2 \text{W}}{\partial a^2}\right)_0 + {}^2\left(\frac{\partial^2 \text{W}}{\partial b^2}\right) + c^2\left(\frac{\partial^2 \text{W}}{\partial c^2}\right)_0 + 2ab\left(\frac{\partial^2 \text{W}}{\partial a \partial b}\right)_0 + 2ac\left(\frac{\partial^2 \text{W}}{\partial a \partial c}\right)_0 + 2bc\left(\frac{\partial^2 \text{W}}{\partial b \partial c}\right)_0\right\} + \text{etc.},$$

where $\left(\frac{\partial W}{\partial a}\right)_0$ means that we first differentiate the expression for W (supposing it to be known explicitly) with respect to a and then put $a = b = c = 0$ in the result; and so on.

Now if the rays are restricted to the paraxial region, so that a, b, and c are all very small, then if we decrease these variables far enough we can always make the terms involving the second and higher powers small in comparison with the first two terms of the series. If the reference planes happen to be conjugate planes of the system W is then reduced to a certain linear function of a, b and c in which their coefficients depend only on the magnification M. The formation of $\frac{\partial W}{\partial l}$, $\frac{\partial W}{\partial l'}$, etc., then shows that $x' = Mx$ and $y' = My$, and the whole of the Gaussian System follows. For rays at greater angles the next term must be included; this involves terms in a^2, ab, etc., and the coefficients depend on $\frac{\partial^2 W}{\partial a^2}$, $\frac{\partial^2 W}{\partial a \partial b}$, etc.; these terms represent the so-called primary aberrations. The next main term involving a^3, b^3, etc., gives the secondary aberrations. In such a general manner, the whole theory of the symmetrical optical instrument can be built up from a very general point of view.

However satisfactory the general result, the complexity of the whole argument when set out in detail is considerable, and the implications of the various steps are not always very clearly brought out by the writers on these topics. For example, one point causing much difficulty to students is the necessary limitation of the rays to a system defined by definite parameters; for example in discussing a characteristic function the rays may be derived from a single radiant point situated in the object plane or elsewhere, but they must belong to some specific system. A clear conception of this feature is essential, yet references to it are sometimes very vague. Further, the original writers were writing for mathematicians; their followers who have written for junior students might well, when referring to the above variables (a, b and c) explain much more fully why "owing to the symmetry of the system, these variables are sufficient." It is not to be wondered at that many keen students whose main interests are optical have despaired of being able to master the theory since many vital points are referred to only in the briefest manner. The whole subject is apt to appear as a mere welter of equations, and the really interesting essentials in the development of the argument are lost sight of.

The ideal of the whole theory is, then, to provide a basis on

which the action of optical instruments can be explained, again without detailed reference to the construction of the system ; and one which, unlike the theory of collinearity, does not break down in any attempt to make useful advances in optical knowledge.

In contrast to the classical discussions it appears worth while to draw attention to some recent studies of Herzberger.[1] He first considers a rotationally symmetrical optical system and a skew-ray passing through it. Imagining now that a plane containing the axis is made to rotate around the axis, it will in general cut the ray-path in two points, one in the object-space, the other in the image-space. These points are called *point* and *diapoint* respectively ; the ratio of their distances from the axis is called the diamagnification. There will be points for which the diamagnification (β') is unity, and these correspond to unit or principal points. Further, there will be a position of the plane in which it is parallel to the "object-ray" and it will then intersect the image-ray in the *diafocal point* ; similarly for the diafocal point of the object-space.

Herzberger shows that the distances s and s' of point and diapoint from the principal points are connected by a relation

$$\frac{\mu'}{s'} - \frac{\mu}{s} = \frac{\mu'}{f'},$$

where μ and μ' are the refractive indices of the object- and image-space respectively, and f' is the distance from the principal point to the diafocal point of the image-space. We also obtain relations of the type

$$\beta' = -\frac{x'}{f'}, \text{ and } \beta' = -\frac{f}{x}$$

where x and x' are distances of point and diapoint from the corresponding diafocal points. Thus again we see in an even more general way that the above equations are not rooted in collinear geometry, but express more fundamental relations characterising axially symmetrical systems ; they hold, for example, if the surfaces are non-spherical, provided they are surfaces of rotation. By introducing the eikonal into the expression of these skew-ray properties, it can be shown comparatively simply how the Gaussian optics of the paraxial region arises as a reduction case from the general condition, making use of the second term in the expansion for W. This is, in effect, going back to first principles instead of deducing the necessary properties of an instrument from a consideration of the characteristic function alone, but there is a good deal to be said for this course when a practical end is in view.

[1] *Jour. Opt. Soc. America*, **26**, 197, 1936.

Various writers have also given considerable attention to what Herzberger terms "optical problems in the large." A study of the optical requirements in certain cases determines the necessary corresponding form of the eikonal; in some cases this can be shown to be incompatible with certain other conditions; thus, for example, it is demonstrated that if we could have a photographic objective giving *perfectly sharp* definition for an object plane at infinity situated on either side of it, the lens would necessarily suffer from distortion. Of course, it will be remembered that though a reasonably good symmetrical objective will produce a *fair* image of points at infinity on either side, the definition is not of optimum sharpness, and this experiment does not apply to the theoretical case. Many other cases have been studied, but while the results are generally of great interest, the greatest care is necessary in applying them to practical problems, and much work will be necessary before the true relations of theory and practice are really clarified.

Space does not permit here of the inclusion of any description of the way in which the use of Hamilton's characteristic function has been developed by Maxwell and others in a manner lending itself to the discussion of the optical-path differences of disturbances arriving in an image point. A similar but simplified method was used by Conrady in lectures to his pupils, but is at present still unpublished in its completer form.

It may now appear possible to make some remarks on the question of the best approach to the subject. There can be little doubt that any approach which over-emphasises the importance of the purely geometrical ideas of collinearity, and exhibits them as the main pathway to the formulation of optical knowledge, is seriously at fault. On the other hand, the general picture of such relations is so simple that it lends itself admirably to the purpose of acquiring some knowledge at least of the working of optical systems in a very short time. For the University teacher with a crowded curriculum this consideration is of paramount importance.

To occupy the main time of a *short* cause of optical instruction with the building, as it were, of a steam-hammer used only in the end to crack a nut would scarcely commend itself as wisdom. On the other hand, when a wider and more detailed knowledge is required, and where a foundation has been laid by suitable experimental studies and some study of fundamental optical ideas, it is attractive for the teacher to consider the adoption of a further systematic development of the theory along lines which first of all study the fundamental properties of real instruments, and end at a point where the general possibilities of optical systems can be

treated, and effectively judged in relation to actual attainments. It would be a great improvement over the usual piece-meal development. For this purpose we badly need a good text-book in English written not with a view to exhibiting the writer's ability to complicate the issue, but with a genuine desire to bring this field of knowledge into the purview of the student. Such a treatment need not involve any difficult analysis and should first aim at making the fundamental optical ideas stand out rather than the mathematical details. For this purpose some of the possibilities of approach mentioned above might be considered.

Such a book need not aim at giving instruction in the methods of designing optical systems (this is being adequately taken care of) but should give a clear picture of the origin of the aberrations and their discussion both in terms of ray-wandering and optical-path differences, bringing the whole as clearly as possible into relation with experimental observations.

RECENT ADVANCES IN SCIENCE

ASTRONOMY. By A. Hunter, Ph.D., F.R.A.S., Royal Observatory, Greenwich.

Perhaps the greatest practical importance of the astronomer to the community lies in the fact that at present he alone can provide it with an accurate time service. Competition from the physicist for the post of time-purveyor, however, is by no means so unlikely as it would have seemed only a score of years ago. The way in which improvements have been introduced into time measurement in recent years makes an interesting story. A later report will review the present state of the related subject of time recording.

In measuring time the astronomer begins by begging the question. Ignoring any philosophical difficulties about the nature of time, he assumes the realist's instinctive knowledge of the " passage of time " and the physicist's laudable desire to measure it. Any recurrent phenomenon which is regular in its recurrence will serve the purpose. The measure of its usefulness as a clock is by comparison with other periodic phenomena—that is in effect how we assess its regularity. Since the earliest times it has been recognised that the fundamental clock is the earth. Measurements of its rate of rotation relative to the various heavenly bodies still order man's daily life, as they did thousands of years ago. The particular body or set of bodies chosen as a reference system decides the sort of time which is measured. The *sidereal day* may be defined as the interval between two successive transits across the observer's meridian of a hypothetical mean star—hypothetical because transits of the real stars are affected by their proper motions. Their mean, of course, is not, and the day defined by using this mean can be regarded as the fundamental period of axial rotation of the earth. Whether or not this rotation is constant is a question which has not worried the astronomer until relatively recent times. He is still the only person likely to worry, because only astronomical observations, and not any comparison with other timekeepers, can at present detect any variation.

The sidereal day, then, is the astronomical standard. Our daily

lives, however, are not ordered by the stars (if the astrologers will permit the heresy), but by the sun, which appears to retrogress completely round the celestial sphere once a year. Successive transits of the sun fix the *apparent solar day.* Owing to the fact that the earth's orbit is neither circular nor coplanar with its equator, however, this interval, as measured in sidereal seconds, is not constant throughout the year. The practical unit is the *mean solar day,* which is constant and on the average the same length as the apparent solar day. In adopting this as the basis of civil time, we shall never depart enough from true solar time to cause such anomalies as would arise, for instance, from using sidereal time as the legal standard.

The astronomer's job is thus to observe sidereal time, but to provide the public with mean solar time. If he were a perfect observer, using perfect instruments, and with a perfect clock as accessory, all would be well. In point of fact, however, as might be expected, trouble arises from a combination of imperfections in the observer and in his tools. Great strides have, however, been made recently in reducing these imperfections. And as in all branches of science, improvements in one direction have brought in their train corresponding improvements in another. The development of precision clocks, for example, has brought into prominence quite recently a subject of two centuries' standing but of hitherto only academic interest. The sidereal day defined above is in practice measured in terms of transits of the vernal equinox—the so-called " first point of Aries "—whose precession along the ecliptic causes the adopted sidereal day to be nearly a hundredth of a second shorter than the true rotation period of the earth. (This precession has already progressed so far even in historical times that the first point of Aries is now in Pisces.) Unfortunately, however, the precessional motion is not uniform, solar and lunar perturbations producing a motion of *nutation* which varies the length of the apparent sidereal day. *Mean sidereal time,* bearing the same sort of relation to the apparent sidereal time measured by astronomical observations as mean solar time does to the sundial's apparent solar time —though the difference is of course very much smaller—has therefore to be used in work of the highest precision.

Dealing with the problems in turn, let us first consider the instrumental side. The fundamental time measurer is the transit instrument, in essence a refractor moving in the meridian, with which are observed the transits of certain stars called *clock stars* whose positions in right ascension are particularly well determined. Time is measured by using the instrument to find the error either

of a sidereal clock rated to run approximately correctly, or of the mean of several such clocks. From the sidereal time so determined, solar time can of course be obtained by suitable adjustment,[1] and distributed by rated chronometer, or by radio, telephonic, or telegraphic signals.

The ideal transit instrument would have its axis of rotation truly horizontal and lying east and west, and its optical axis truly in the meridian. Errors of level, azimuth, and collimation are, however, always present, to a variable extent depending on meteorological and geophysical conditions, and a somewhat laborious observational programme has to be employed at frequent intervals to eliminate their effects. In addition, there will be what may be termed " constructional errors," distinct from these errors of adjustment, such as those due to flexure in the axis and in the tube of the telescope, and to the pivots being imperfect cylinders or having their axes not quite collinear. For accurate work, the possibility of occurrence of such errors necessitates the utmost precision both in workmanship and in observing technique. However confident the astronomer may be in the exhaustiveness of his methods, the very multiplicity of the corrections needed must invite suspicion if the results given by the transit instrument are not all that can be desired. Woolley (*The Observatory*, **57**, 124, 1934) has shown from a discussion of the Greenwich observations that a small reversible transit circle can yield from a good night's programme (say of two hours' duration) a time determination whose final probable error is 0·01 sec., as determined from the internal concordance of the measures. But it is not unknown for such a determination to disagree from the value predicted from the rates of the observatory clocks by five times as much as this ; and for subsequent observations on the same instrument to confirm this uncertainty. In these circumstances, methods which promise accurate results with fewer errors needing elimination or correction are being tried.

Prominent amongst these methods is one which employs the photographic reflex zenith tube, used normally for latitude observations. Littell and Willis (*Astronomical Journal*, No. 931, p. 7, 1929) have described how the instrument of the U.S. Naval Observatory has been adapted to time determination. Light from stars

[1] The problem of making two dials show mean and sidereal time respectively from the same seconds impulses (either mean or sidereal) will interest the arithmetician. It involves finding a train of gears, each with a manageable number of teeth, to give the sidereal-solar ratio 366·242201/365·242201 to the required accuracy. Several such trains have recently been suggested (see Baumbach, *Zeits. für Instrumentenkunde*, **56**, 469, 1936, or Esclangon, *Comptes Rendus*, **206**, 289, 1938).

near the zenith passes through a horizontal objective, rotatable in its own plane, on to a free mercury surface, below the lens by half its focal length, and is thence focused on to a small photographic plate held rigidly just below the objective. The image of a star culminating close to the zenith is allowed to trail across the plate, a time-scale being provided by jogging the plate sideways (*i.e.* north and south) at each tick of the sidereal clock, exactly as a chronograph pen is operated. The objective and plate are quickly reversed through 180° five seconds before transit time, and the star trails off the plate again in the opposite direction. Suitable measurement of the plate will then evidently give the clock time of meridian passage. For stars which are not bright enough to give a measurable trail, a modification of the method has been devised, in which the plate is moved by clockwork to counteract the motion of the stars across the field. A simple arrangement registers on a calibrated chronograph record the position of the plate at certain points in its travel, both forward and reverse. Thence the procedure is in principle identical with that of the fixed-plate method.

The advantages obtained are many. First of all, the method is an independent one, essentially different from that employed in the transit instrument. Then the bugbear of the transit circle observer, the level error, is completely eliminated, as also are collimation and azimuth errors, pivot irregularities and flexure errors, and the personal equation of the observer, discussed below. The stability of the zenith tube is also superior to that of a transit instrument, whilst the use of photography, with its permanent record, is a great step forward.

The main disadvantage of the method is luckily only temporary. It lies in the limitation of programme. The stars which have to be used are usually fainter than magnitude 7·5, and their positions are naturally not very well known. This may indeed be the cause of certain systematic differences which exist between transit instrument results and zenith tube results (*Astronomical Journal*, No. 994, p. 9, 1933). As time goes on, however, the positions will be improved by transit circle determinations as well as internally, from the zenith tube plates themselves, and the method should take its place beside the older one as a fundamental way of measuring time.

Let us turn now to the imperfections of the observer. The earliest method of observation on the transit instrument was the eye-and-ear method, in which the observer, watching the clock star move across the reticle in his eyepiece, estimated its time of transit to a tenth of a second by interpolating between the beats of the sidereal clock. This method, even when every known source of

instrumental error had been removed, always gave systematic differences between the results of one observer and those of another. It was early realised that

> The fault, dear Brutus, is not in our stars
> But in ourselves.

Nearly 150 years ago, Maskelyne, the fifth Astronomer Royal, dismissed his unfortunate assistant for committing such "gross errors" as 0·5 (later 0·8) sec. in making time observations by this means. It was recognised only forty years later by Bessel that such personal differences between observers are inevitable, and are not caused by the culpable negligence to which Maskelyne attributed them. The rise of electromagnetism in the mid-nineteenth century saw the perfection of the chronograph method, in which the observer closed a contact operating a chronograph pen as the star moved across the crosswires. The clock registered seconds on the same record, so that the determination now consisted in a mere spatial interpolation. By using ten vertical wires for the transit of each star, this method can reduce the so-called *personal equation* to the order of 0·2 sec. This residual error is easy to understand and to interpret as a difference of temperament between observers, especially as it remains almost constant for each observer, changes due to fatigue, conditions of seeing, and rate of motion of the star image being of minor importance.

The effect of the brightness of the star observed is, however, of more significance. For any one observer a systematic difference between time observations deduced from bright and faint stars respectively has been found to exist. In chronographic registration the key may be pressed perhaps a tenth of a second later for the faintest stars used than for the brightest stars in the sky. This *magnitude equation* can be evaluated for a given observer, and the run of the error with magnitude may be used to evaluate the correction to be applied to each observation.

As the demand for accuracy increased, however, efforts have been naturally directed to eliminating these personality errors entirely, rather than applying corrections to neutralise their effects. The practice is now growing to fit transit instruments with some form of *impersonal micrometer*. In using this device, the observer drives the fiducial system in his eyepiece, by means of a screw motion, so as to follow the star image during transit. In a modification of the same device, the reticle frame is driven automatically at the rate appropriate to the star's declination, and the observer has merely to impart a retardation or acceleration, as required.

In both cases the motion of the frame in the eyepiece is electromagnetically recorded on a chronograph together with the clock signals, and the time of transit is deduced by interpolation. The introduction of the impersonal micrometer has reduced the effect of personal equation enormously, though systematic differences of the order of a hundredth of a second still exist. This is, of course, to be expected, as the observation is essentially one of keeping a star image bisected, and, as is well known, bisection estimates are systematically different in different observers. The auxiliary drive shows over the hand drive a small but definite improvement, which is probably connected with the reduction in the amount of concentration demanded from the observer.

A new and essentially different way of avoiding personality error has been very recently described by J. de G. Hunter (*Proc. Roy. Soc.*, A, **166**, 197, 1938). Whilst this device is in a compact form intended for use with field theodolites for longitude determinations, there seems no reason why it should not be adapted for observations of time, which are of course identical in principle. A small central portion of the telescope field is normally obscured by a shutter close to the focal plane of the objective. For longitude determinations, this shutter exposes the star for observation at a time controlled by a break-circuit chronometer. In field work the telescope is that of a theodolite and the star exposures are given for 0·070 sec. every third second by means of a relay system operating the shutter electromagnetically. During such a short interval the star appears stationary in the field of view, and its position is read at each of some 20 appearances in terms of a scale consisting of 40 horizontal lines ruled on glass at intervals of 0·001 inch. The time of shutter operation is subsequently compared with the rhythmic time signals radiated from Rugby or Bordeaux. A contact on the shutter arm is introduced into the output circuit of the radio receiver employed, and breaks this circuit for the duration of shutter operation. The incoming signals are thus extinguished during operation, and by suitable adjustment of the contact complete extinction of one signal can be obtained and a very precise coincidence measure secured. For transit circle observations, however, where there is no objection to the employment of a chronograph, the shutter impulses can evidently be treated just as the taps of the old "galvanic" method were, and imprinted on the recording tape together with seconds signals from the standard clock whose rate is being determined.

Until the principle has actually been applied to transit observations there can be little more than an estimate of its reliability, but there seems no reason to doubt that personality is eliminated

as well by this method as by the form of impersonal micrometer now in use. It is in any case evident that personality error is looming larger and larger on the astronomer's horizon as the accuracy of his observations increases. At present its magnitude is of the order of 0·01 sec. The fact that this quantity enters directly into the determination of time, a determination whose internal probable error is of the same order, makes it essential that it should be evaluated and allowed for. This is, in fact, done at present, more or less satisfactorily, by means of the personal equation machine. Nevertheless, no efforts are being spared in devising methods which will eliminate entirely the necessity of applying such corrections.

The process of perfecting these methods will be followed with interest not only by the professional astronomer but also by the horologist, whose most accurate workmanship is already being taxed by these developments on the astronomical side of time measurement.

PHYSICS. By F. A. Vick, Ph.D., University College, London.

The Acoustics of Buildings.—The foundations of Architectural Acoustics were well and truly laid by W. C. Sabine (*Collected Papers on Acoustics*, 1922). His careful experiments led to the well-known relation

$$T = \frac{0{\cdot}16V}{\Sigma\, a_1 S_1} \text{ seconds} \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where T is the "reverberation period" of the room, V the volume in cubic metres, and S_1 square metres the area of surface with coefficient of sound absorption a_1. If in a room a source of sound (for example, an organ pipe) is suddenly cut off after steady conditions have been reached, the time required for the sound intensity to fall by 60 decibels, that is to one millionth of the initial intensity, is this reverberation period T. A decibel is ten times the common logarithm of the intensity ratio. A change of 60 decibels was chosen because it represents approximately a fall in intensity from loud speech to the threshold of audibility. In an ordinary room a sound is reflected many times by the walls and objects in the room before it finally dies away, for it will be remembered that the velocity of sound in air is about 33 kilometres a second, and a normal hard wall is a better reflector of sound than most mirrors are of light. Reverberation is thus a confused or inarticulate prolongation of sound. An echo, on the other hand, is a definite or articulate repetition of a sound after an appreciable interval, due to strong reflections in a particular direction. The sound audible to a person in a hall therefore consists of (*a*) direct sound, reaching him without

reflection, (*b*) reverberation, (*c*) echoes. A small amount of reverberation is useful, because it increases the intensity of sound received, but if it persists too long, the intelligibility of speech, for example, is impaired. In this case, it is obviously of no use just to introduce loud-speakers, the only remedy is to reduce the reverberation time and *then* if necessary increase the sound intensity in more distant parts of the room.

General experience shows that a reverberation period of two seconds is about right for large halls, and one second for smaller halls in which the sound does not need so much reinforcement. Halls intended for music may with advantage have rather longer reverberation periods than those for speech. With a short period they sound "dead."

Sabine's reverberation formula implies (1) the duration of audibility of the residual sound is the same in all parts of the room; (2) the duration of audibility is independent of the position of the source; (3) the efficiency of an absorbent in reducing reverberation is independent of its position. To a first approximation these are found to be true for simple cases, and the formula enables us to obtain with surprising ease the reverberation period of a room, and to calculate how much absorbent is needed to reduce T for a given room to the desired value. Architects have found it of great value. But, good working rule though it is, the formula has limitations and has to be modified under certain conditions.

Equation (1) obviously breaks down when all the walls are completely absorbing, for it gives

$$T = \frac{0{\cdot}16\, V}{S}$$

and not zero. To derive the equation theoretically it is assumed that the space is uniformly filled with sound, which dies away continuously, a being small and T long. (See, for example, E. Buckingham, *Sc. Papers Bur. Stand.*, **20**, 193, 1925). A more satisfactory formula is obtained by taking account of the fact that the sound decays discontinuously, at time intervals corresponding to distances $l_1, l_2, \ldots l_n$ between reflections. To simplify calculations the "mean free path"

$$l = \frac{4V}{S}$$

is used (see V. O. Knudsen, *Rev. Mod. Phys.*, **6**, 4, 1934). The total number of reflections for the intensity to fall by a factor of 10^6 is N, given by

$$(1 - a)^N = 10^{-6}$$

where a is the absorption coefficient. The reverberation time is then N divided by n, the number of reflections per second, calculated from the velocity of sound v and the mean free path.

$$T = \frac{N}{n} = \frac{-6 \times 2{\cdot}30}{\log_e (1 - a)} \cdot \frac{4V}{v.S}$$

$$\therefore T = \frac{0{\cdot}16V}{-S \log_e (1 - a)} \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Equation (2) approaches the original equation (1) when a is small, since

$$-\log_e (1 - a) = \frac{a}{1} + \frac{a^2}{2} + \frac{a^3}{3} + \ldots$$

When a is larger, equation (2) shows that less absorbing material is needed for a given T than would be inferred from equation (1). Difficulties may arise if we have to deal with a room containing many different kinds of surface. If we follow Millington (*J. Acoust. Soc. Amer.*, **4**, 69, 1932) and use a summation analogous to equation (1), giving

$$T = \frac{0{\cdot}16V}{-\Sigma S_1 \log_e (1 - a_1)} \quad . \quad . \quad . \quad . \quad . \quad . \quad (2a)$$

and there is a surface, however small, with $a = 1$ (*e.g.* an open window), then T becomes zero, since then $-\Sigma S_1 \log (1 - a_1) \longrightarrow \infty$. See also Eyring (*J. Acoust. Soc. Amer.*, **1**, 217, 1930).

We have neglected so far the absorption of sound by the air in the room. V. O. Knudsen has shown that the coefficient of absorption of moist air is much larger than was once thought, especially at frequencies above 5000 (*Rev. Mod. Phys.*, **6**, 1, 1934). Our reverberation formula must then be modified further to

$$T = \frac{0{\cdot}16\,V}{4\,m\,V - \Sigma S_1 \log_e (1 \times a_1)} \quad . \quad . \quad . \quad . \quad . \quad (3)$$

where m is the absorption coefficient of the air. At frequencies just above the audible, the second term may be negligible compared with the first.

While considering Sabine's formula we have noted the assumption that the effect of an absorber is independent of its position. But suppose we have a room of rectangular section in which the walls are good reflectors and the floor or ceiling (or both) are good absorbers. Then the sound reflected in the vertical plane will die out much more quickly than that in the horizontal, and we shall have in effect a longer reverberation time than that calculated from the arithmetic mean of the absorptions [or from $\Sigma S_1 \log_e (1 - a_1)$] In other words, the reverberation time is lengthened by concen-

trating all the absorbents in one place. However, in general practice large halls are not simply rectangular in section, and there is not such a tendency for reverberation to persist in two dimensions. Structural features and ornaments tend to diffuse the sound. The effect is of greater importance in the absorption measurements mentioned below.

Further complications may arise if room resonance occurs, but fortunately the resonant frequencies of large rooms are too low to cause much disturbance. We may say that, in general, if the sound is reasonably diffused and pronounced echoes are absent, equation (3) gives results accurate to within 10 per cent. Care must be taken when applying it, however, where rooms are linked together, and with rooms of very irregular shape. Greater accuracy than this can be attained by taking suitable precautions, as in an acoustics laboratory.

To measure reverberation times, Sabine's subjective method is still used to some extent, but with his organ pipe replaced by a gramophone, amplifier and loud-speaker, the output of which can be controlled by a potential divider calibrated in decibels. A switch is arranged to start an electric clock when the loud-speaker is cut off, and stop it when the loud-speaker is switched on. A record of a "warble tone," whose frequency varies over a short range, is put on, and the output set by the potential divider to 60 decibels above the threshold intensity. With the sound intensity in the room at a steady value, the loud-speaker is cut off and the clock started. When the threshold of audibility is reached, the switch is reversed, stopping the clock and re-starting the loud-speaker. This procedure is repeated a known number of times, the clock adds up the reverberation periods measured, and so an average can be obtained. Entirely automatic "reverberation meters" (*e.g.* H. J. Sabine, *Electronics*, **10**, 30, 1937) are more often used now, sometimes the whole course of the dying away of the sound being recorded on a logarithmic scale. Some interesting curves obtained with such an instrument are given by van Urk (*Philips Tech. Rev.*, **3**, 65, March 1938). Sometimes the decay curve is "integrated" by using a microphone, amplifier, rectifier and galvanometer. If the rectifier is a linear one, the total quantity of electricity measured by the galvanometer is proportional to the reverberation time and to the steady current through the galvanometer just before the sound is cut off. An ordinary ballistic galvanometer can be used only when the reverberation time is very short, but the range is considerably extended by using a fluxmeter instead (Fleurent and Beauvilain, *Comptes Rend.*, **206**, 895, March 1938).

The reverberation formula being well established, measurements of absorption coefficients can be based upon it. The total absorption of a special room is determined by measuring the reverberation time by one of the above methods, first when the room contains a certain area of the acoustical material (not on the walls) and again when the specimen is removed from the room. Under these simple conditions, the original Sabine formula, equation (1), is generally used, and the absorption by the specimen is taken to be the difference between the two measured absorptions ($\Sigma a_1 S_1$). This is justified only when the sound is kept thoroughly diffuse during the whole process of decay. Warble tones and large rotating vanes help considerably. The specimen should be large compared with the wavelength of the sound used ($\lambda = 60$ cm. at 512 cycles, approximately), and yet should not be too large compared with the total surface of the room. The vibrations in the room must not react on the source. A recent paper by Morris, Nixon and Parkinson (*J. Acoust. Soc. Amer.*, **9**, 234, Jan. 1938) discusses measurements made by this method, especially with regard to the area of the specimen in relation to the size of the room. Absorption by moist air may vitiate the results at high frequencies, and V. O. Knudsen has suggested filling the chamber with dry nitrogen. This is more practicable with the small-scale method making use of stationary waves. The flat test specimen closes one end of a cylindrical tube (drain-pipes have been used), the open end of which faces a source of sound of constant frequency. From the pressure amplitudes of the standing waves the absorption coefficient can be deduced, since the reflected waves have smaller amplitudes than the incident ones. The theory of the method is discussed by Eckhardt and Chrisler (*Bur. Stand. Sc. Pap.*, **21**, 37, 1925).

In a third method the specimen (about 4 ft. $\times$ 5 ft.) is mounted as a partition between two rooms, otherwise acoustically isolated. The rooms are well lagged with absorbing material. Measurements are made of the sound reflected and transmitted by the partition, from which the absorption coefficient can be deduced. A general description of the method as used at the N.P.L. is given by Davis and Littler (*Phil. Mag.*, [vii] **3**, 177, 1927) and an extended series of results by Constable and Aston (*Phil. Mag.*, [vii] **23**, 161, 1937).

Along with these measurements, a good deal of theoretical work is being done on the absorption of sound by porous materials, following the original treatment by Rayleigh (*Theory of Sound*, Vol. 2, p. 329). As a recent example we may mention the paper by Monna (*Physica*, **5**, 129, March 1938), who concludes that for layers

1–2 cm. thick, an effective pore radius of 10 — 20μ will be the most favourable for absorption.

Absorption coefficients being known, application of equation (3) enables us to estimate the reverberation period of a room before it is built or to modify an existing one, but there is still the question of echoes. It is possible to predict the general direction of prominent echoes by simple geometry, on a similar basis to ordinary optical reflection, but the process is very laborious and it is difficult to allow for the spreading which occurs when sound is reflected by objects comparable in size to the wavelength. It has therefore been usual to construct a sectional scale model of the proposed room (a theatre, for example) and to study the reflections in it of sound pulses, or of ripples on water in a tank. The first method takes advantage of the refraction of light by the compression waves of sound, an impulse being started by a " sound-spark " and photographed after a chosen short interval by the light of a second (distant) spark. Both this and the ripple-tank method use wavelengths of the right order. Descriptions and photographs are given in most books on applied acoustics (*e.g.* Davis and Kaye, *The Acoustics of Buildings*, Bell, 1932).

Such experiments are useful, not only for discovering undesirable echoes, but also for ensuring a sufficient sound intensity in remote parts of the room, *e.g.* under a gallery. For such a purpose, optical models, made of aluminium sheet and with an electric light bulb to simulate the source of sound, have been used recently (R. Vermeulen and J. de Boer, *Philips Tech. Rev.*, **1**, 46, 1936; R. Vermeulen, *Philips Tech. Rev.*, **3**, 139, May 1938). The justification is that intelligibility of speech depends mainly on the higher frequency components, with wavelengths of 30 cm. or less, which are very nearly regularly reflected. The audience in these models is represented by opal glass, through which the distribution of intensity in the different parts can be seen. Theatres made with the aid of models and the reverberation formula confirm the predictions with regard to loudness and intelligibility.

With the knowledge summarised above as a guide, there is little excuse for an auditorium to be built with bad acoustics. Unfortunately, it is to be doubted whether modern architecture, with its preference for simple lines and smooth walls, is always as good acoustically as one would wish (in cafés, for example).

The subject of insulation against noise (defined as " sound undesired by the recipient ") has been introduced already in this journal by G. W. C. Kaye (SCIENCE PROGRESS, **30**, 385, 1936). A brief mention of one or two recent papers will therefore suffice.

McMahan (*J. Acoust. Soc. Amer.*, **7**, 204, 1936) discusses the silencing of fans, and Morreau (*ibid.*, **10**, 45, July 1938) the calculation of the insulation of partitions from the architect's point of view.

To prevent the intrusion of structural-borne sounds, the room should be completely isolated from its neighbours. In practice, this means "floating" the room on rubber pads inside another room with separate doors, etc. In connection with this, Davis and Knowler have pointed out (*Phil. Mag.*, [vii] **23**, 145, 1937) that if the air space between floating and fixed floors (for example) is closed at the edges and only moderately thick, the sounds transmitted by the air space may exceed those conducted by the rubber pads.

Two short papers by J. E. R. Constable are of general interest (*Proc. Phys. Soc.*, **50**, 360 and 368, May 1938). In one he describes how noises can be prevented from travelling along water pipes by the insertion of a length of rubber hose, and in the other some experiments on the transmission of sound by indirect paths in a block of flats.

METEOROLOGY. By E. V. Newnham, B.Sc., Meteorological Office, London.

Hailstones.—My notes in Science Progress for April 1938 on some recent attempts to deal mathematically with the physics of large hailstones have drawn the attention of certain American electrical engineers to the subject, and have led indirectly to the recording of a very interesting observation. Mr. R. P. Johnson of the Research Laboratory, General Electric Company, New York, in a letter dated May 28, 1938, wrote as follows:

I am prompted by reading your interesting review on "The Formation of Hailstones," in Science Progress recently, to hope that you may care to hear of some hailstones that fell in Washington, D.C., shortly after noon on last April 29, in a typical brief thunder-shower.

The stones had various sizes up to about 15 mm. mean diameter. Their shapes and their stratification showed plainly that they were fragments of spherical stones about 30 mm. in diameter, laminated radially with alternate clear and cloudy layers, each layer being roughly 2 mm. thick. In other parts of the city, spherical stones of this size (as judged from newspaper photographs) did fall, damaging greenhouses and automobile tops. The question is, how were the stones broken? They must have been broken rather high, for the pieces appeared to have terminal velocities suited to their sizes: they did not break further on hitting a wet concrete pave-

ment. Fracture must have occurred after the stones had fallen out of the region where they were formed, else additional layers would have been built onto each fragment. For this reason, it is not plausible that they were broken in collisions among themselves. Stresses caused by changes in temperature along the paths would hardly be enough to cause the stones to explode. The fact that these stones were without exception broken, while stones that fell two miles away were uniformly whole, suggests some fairly local catastrophe, though size-sorting by horizontal winds is possible. One hypothesis is that the stones were shattered in the pressure wave from a bolt of lightning that happened to pass near them as they fell. I do not know how reasonable this may be.

Perhaps other occurrences of this kind have come to your notice, and perhaps there is an accepted explanation for them. I should be happy to have your comments.

Mr. S. T. Martin and Mr. H. Herring examined some of the stone-fragments with me. The suggestion that the stones were struck by lightning is due to Mr. J. Schremp.

In subsequent correspondence Mr. Johnson has given the views of others whom he has consulted with a view to explaining what had been observed, but in the present writer's opinion theoretical explanations are of less importance than the original observation, which was evidently carefully recorded and does not rest on the testimony of a single witness only. In regard to the theory of shattering by a pressure wave set up by lightning, however, the following relevant observation was brought to the attention of the Director of the Meteorological Office, Air Ministry, a year or two ago: A violent thunderstorm was in progress in Johannesburg and a flash of lightning was observed very near to a car that was standing outside a garage. Although the car was not struck by the lightning, the windscreen was shattered in spite of its being made of very thick glass of a pattern designed to be virtually unbreakable.

The present writer may be excused for airing, at this point, a view that owed its origin to a difficulty experienced as a boy first studying physics—the difficulty of disentangling observational data from the theories built up around them. It was a surprise to him, and a pleasant one, to find so great a physicist as the late Professor J. H. Poynting, under whom he studied between 1911 and 1914, constantly exhorting his students not to attach too great importance to the elaborate mathematical theories that were being so often built up on the slender basis of a few physical observations made with apparatus bound soon to become obsolete with the advance

of engineering. The application to the hailstone problem is perhaps almost too obvious to need stating. We want more observations of the phenomena of the thunderstorm made by observers who are not only used to careful observation, and able to describe accurately what they see, but are unbiassed by too hastily formed theories as to the cause of what they have seen. When enough observations of that kind are available the mathematicians can step in and complete the work and there will be no further need for them to make simplifying assumptions, the validity of which can only be guessed, as so often happens when mathematical analysis is applied to the incomplete and inexact data of meteorology.

BIOCHEMISTRY. By W. O. Kermack, D.Sc., LL.D., F.R.S.E., Research Laboratory, Royal College of Physicians, Edinburgh.

The Chemotherapy of Bacterial Diseases.—The first great success of modern chemotherapy was the discovery by Ehrlich of the remarkable action of various arsenical derivatives against certain diseases due to trypanosomes and spirochætes. The second was the production of synthetic drugs with an antimalarial action of the same order as that of quinine, of which the most important are the compounds sold commercially under the name of plasmoquin and atebrin. But until a few years ago no real advance had been made towards the effective treatment by chemotherapy of diseases due not to protozoa but to bacteria. A whole new field has been opened up by the discovery of Domagk (of the German Chemical Combine, the I.G.) (*Deutsche Med. Woch.*, **61**, 250, 1935), that certain azo-dyestuffs (commercially called "Prontosil") containing a sulphonamide group, exert a remarkable curative action on mice and other animals infected by hæmolytic streptococci. Within the relatively brief space of time which has elapsed since the original discovery was made, these sulphonamide compounds have already saved thousands of lives and have established for themselves a most important place in the armoury of the physician.

An important advance was made in 1935 by Tréfouel, Nitti and Bovet (*Compt. rend. Soc. Biol.*, **120**, 756, 1935) when they discovered that the curative action was possessed not only by azo-dyestuffs but by the simple parent compound *p*-aminobenzene-sulphonamide (I).

(I)

This compound and certain simple derivatives of it have proved

of great efficiency in the treatment of infections due to hæmolytic streptococci especially erysipelas and septicæmias (*cf.* Colebrook, Buttle *et al.*, *Proc. R. Soc. Med.*, **31**, 149, 1937). A very important field in which it has been found useful is in the treatment of puerperal fever, the cause of so many tragic deaths of mothers after child-birth. The great importance of the new treatment is illustrated by the findings of Colebrook and Purdie (*Lancet*, **233**, 1237–42, 1291–3, 1937) who report that in the Queen Charlotte Hospital, London, the case mortality from this cause was reduced from over 20 per cent. to about 5 per cent. as the result of sulphonamide treatment.

It was soon discovered that the curative action of the sulphonamide derivatives was not limited to diseases caused by hæmolytic streptococci. There is a general concensus of opinion that these compounds are valuable in meningococcal infections and a considerable body of evidence indicating their disinfecting action in gonorrhœa and in infections of the urinary tract. Curiously enough, non-hæmolytic strains of streptococci seem to be much less amenable to treatment by these drugs whilst no clinical success has so far been achieved against septicæmias and other infections due to staphylococci.

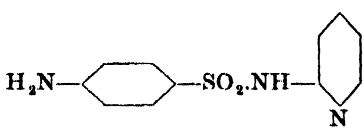
In view of these results, it is natural that a considerable amount of work should have been done with regard to the action of compounds of the sulphonamide series in pneumococcal infections. The problem of obtaining an effective chemotherapeutic agent against pneumonia has for long attracted attention. It is now over twenty years since J. Morgenroth and R. Levy (*Berl. Klin. Woch.*, **48**, Nr. 34, 1912) found that certain alkyl derivatives of dihydrocupreine (cupreine = quinine from which the ethyl group has been removed) had a curative action on mice infected with pneumococci. More recently certain other derivatives of quinine belonging to the apo-series have also given positive results in animals. Reports on the action of *p*-aminobenzenesulphonamide on mice infected with pneumococci show that there is usually a definite effect, that this effect varies according to the type of pneumococci employed, and that under the most favourable circumstances there is rarely if ever a permanent cure, but rather only a prolongation of life. The treated animals may survive for perhaps 3–5 days, instead of dying within 24–48 hours after infection like the controls. Some compounds have been found to be more active than the simple *p*-aminobenzenesulphonamide. Thus Whitby (*Lancet*, **232**, 1517, 1937) stated that both 4 : 4′-diaminobenzenesulphonanilide tartrate and 4 : 3′-diaminobenzenesulphonanilide have a definite protective

action on mice infected with pneumococcus type I. Similar results have also been obtained with certain derivatives of diphenylsulphide (II).

(II)

These compounds possess a chemotherapeutic action on hæmolytic streptococci comparable with that exacted by the active sulphonamide derivatives and in some instances they also have a distinct anti-pneumococcal action. Buttle and Gray (*Proc. R. Soc. Med.*, **31**, 154, 1937), for example, found that the benzylidene base of 4 : 4′-diaminodiphenylsulphone was active in type I pneumococcal infections, whilst Fourneau *et al.* (Fourneau, Tréfouel, Nitti and Bovet, *Compt. rend.*, **205**, 299, 1937) claimed that di-(*p*-acetamidophenyl)sulphone was specially curative in mice infected with pneumococci.

The results recently published by Whitby are of special importance (*Lancet*, **234**, 1211, 1938). In the course of testing out a large number of compounds prepared in the laboratories of May and Baker, he found one which possessed outstanding antipneumococcal activity. This is 2-*p*-aminobenzenesulphonamidopyridine (III).



(III)

It is relatively non-toxic and especially effective when administered to mice previously inoculated with pneumococci of type I, VII or VIII. Untreated mice usually die within 24 or 48 hours, but most of the treated mice live during the whole time of observation, that is to say 7 days after infection, which may be taken as equivalent to complete cure. Against pneumococci of types II, III, V, it is also potent though here the effects are perhaps less striking. It is interesting to learn that the new compound is at least as effective against hæmolytic streptococci as *p*-aminobenzenesulphonamide, and has the advantage of being much less toxic.

Closely following upon the report by Whitby of the action of 2-*p*-aminobenzenesulphonamidopyridine on pneumococcal and streptococcal infections in mice, comes an account by Evans and Gaisford of their experience with this drug in the treatment of human pneumonia (*Lancet*, **235**, 14, 1938). These workers treated one hundred

patients at the Dudley Road Hospital, Birmingham, and used as a control series another 100 cases treated by the ordinary methods. In the control series, the mortality was 27 per cent., a figure which is about average for this disease. In the treated series, however, the mortality was only 8 per cent. It is of special interest to notice that of the 8 deaths, 6 occurred during the first few weeks of the investigation when relatively small quantities of the compound were being administered in view of the possibility of the appearance of toxic symptoms. As experience was gained, it was found possible to use larger doses, and indeed 6–9 grams per day could be administered with little or no signs of even mild toxic effects. The extreme harmlessness of the new compound would seem to be one of its great advantages.

It is of course too early as yet to reach any conclusion as to the real value of *p*-aminobenzenesulphonamidopyridine in human disease. Even the most carefully controlled clinical trials of a new treatment sometimes give an unduly optimistic impression of its efficiency. No doubt the possibilities of a compound of such fine promise will be rapidly explored; and not only of this compound alone but of many closely related ones as well, for some of these might prove to be of equal or even greater efficiency.

The mechanism of the action of the sulphonamide derivatives has naturally aroused great interest, but in spite of much work there is as yet no simple and generally accepted explanation. The original prontosil was an azo-dyestuff, and it was shown by Nitti and Bovet that this is reduced in the animal body with the formation of *p*-aminobenzenesulphonamide, which is excreted in the urine, and that the latter compound is itself at least as curative as the original dyestuff. They therefore concluded that the prontosil acts through producing the simpler amino derivative. This conclusion is supported by the fact that the dyestuff has little or no antibacterial action in vitro, whilst *p*-aminobenzenesulphonamide exerts a bacteriostatic and in higher concentrations a bactericidal action on cultures of hæmolytic streptococci and certain other organisms.

No one, however, contends that this antibacterial action of *p*-aminobenzenesulphonamide and related compounds by itself explains their remarkable chemotherapeutic action. There are many compounds much more bactericidal in vitro—sulphonamide derivatives are rarely active in concentrations of less than 1 in 10,000, whilst acriflavine is bactericidal at from one-tenth to one-fiftieth of that concentration—which are useless for the treatment of animal or human septicæmias. Further, the action of various compounds of the group on different bacteria does not run parallel

with the curative action in vivo. Some evidence suggests that the immunological resources of the organism are stimulated by the compounds. Another possibility is that *p*-aminobenzenesulphonamide is converted in the body into some still more active agent. The fact that *p*-nitrobenzenesulphonamide is highly active suggests that both this and the *p*-aminocompound may act by being converted into some intermediate form. Mayer (Mayer and Oechslin, *Compt. rend.*, **205**, 181, 1937; R. L. Mayer, *Presse Medicale*, 1009, 1937) has for instance found that the hydroxylamino derivative is highly active in vitro, and thinks that this may be the real curative agent, but direct evidence in support of this type of theory does not so far seem to have been obtained.

Various investigators such as Vaisman, Whitby, and Fleming, have laid emphasis on the part played by the defensive weapons of the organism which are presumably more effective against the damaged bacteria than the drug. Much interest attaches to the observation by Whitby that pneumococci taken from the peritoneal cavity of mice a few hours after treatment with *p*-aminobenzenesulphonamidopyridine, and before complete sterility has been effected, undergo characteristic changes in their appearance. Under the action of the drug the capsule swells and is ultimately lost. Some observers have noticed similar though less striking changes in cultures of hæmolytic streptococci in the presence of *p*-aminobenzenesulphonamide. It may be these compounds act by promoting the disintegration of the " armour-plate " defences of the micro-organisms and thus rendering them specially vulnerable to attack and destruction.

Some experiments recently reported by Fleming (*Lancet*, **235**, 74, 1938) are in agreement with this view. Blood plasma free of leucocytes has little or no effect on pneumococci. When the leucocytes are present, some but not all the bacteria are killed. The addition of *p*-aminobenzenesulphonamidopyridine in concentrations of 1 in 32,000 or 64,000 to the blood plasma lacking leucocytes gives it a strong bacteriostatic but little or no bactericidal action. If, however, the leucocytes are also present the organisms are not only temporarily prevented from multiplying, but they are permanently inhibited. Similar results are obtained with hæmolytic streptococci. These observations which confirm and extend those of previous workers with *p*-aminobenzenesulphonamide, suggest that the leucocytes are able to deal with and destroy the bacteria more efficiently after the latter have been damaged by the drug.

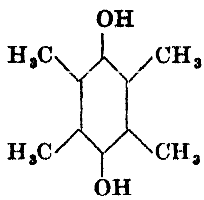
Vitamin E.—It is now sixteen years since Evans and Bishop

(*Science*, **56**, 650, 1922) first detected the existence of a food factor not apparently required for growth or the maintenance of health but necessary for the reproductive function in both sexes. Male rats reared on a diet deficient in vitamin E were sterile as the result of failure to produce spermatozoa, whilst female rats similarly nourished could become pregnant, but the embryos died and underwent a process of resorption in utero. The reproductive vitamin which like vitamins A and D belongs to the fat soluble group was found to be present in various foodstuffs, and is relatively abundant in the oil from wheat germ, whilst more recently, rice germ oil and cotton seed oil have also been used as sources.

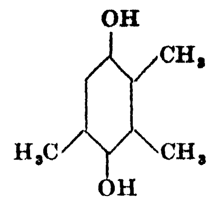
Various attempts were made to isolate the active compound, and it was soon found that it could be obtained in a relatively concentrated form in the non-hydrolysable sterol-free fraction of the oil. Compared to other vitamins it was surprisingly stable and could be distilled in vacuo at a high temperature without a serious loss of activity (Evans and Burr, *Mem. Univ. Calif.*, **8**, 1927; Olcott, *J. Biol. Chem.*, **107**, 471, 1934). Drummond, Singer and MacWalter (*Bioch. J.*, **29**, 456, 1935) separated a highly active oil the analysis of which corresponded with the formula $C_{28}H_{50}O_2$, but in the absence of crystalline derivatives there was no guarantee that the compound was really pure.

The spectacular advances achieved during the past two years were made possible by the work of Evans, Emerson and Emerson, who isolated two active compounds from wheat germ oil in the form of their allophanates. (Allophanic acid is an unstable acid of the formula $H_2N.CO.NH.COOH$, and the allophanates are formed by treating the crude oil with cyanic acid.) The two active compounds were named α- and β-tocopherol, and the allophanates melted at 164° and 146° respectively. α-Tocopherol, which has the formula $C_{29}H_{50}O_2$ is able, in doses of 3 milligrams, to restore to avitaminous rats the power to bear young, whilst β-tocopherol $C_{28}H_{48}O_2$, is rather less active, the minimum dose being 8 mg. Both alcohols are oils which, once obtained pure, yield a number of well-defined crystalline derivatives.

An important clue to the structure of these compounds was obtained by Fernholz (*J. Am. Chem. Soc.*, **59**, 1154, 1937) who found that α-tocopherol, when heated to 350°, yielded duroquinol (IV). An analogous observation was made by John (*Z. physiol. Chem.*, **250**, 11, 1937) who found that β-tocopherol (he called his compound cumotocopherol, but its identity with β-tocopherol seems to have been well established) on thermal decomposition yielded pseudo-cumoquinol (V).

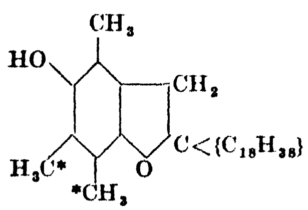


(IV)



(V)

A natural interpretation of these results was that α- and β-tocopherol were ethers of duroquinol and pseudo-cumoquinol respectively with an alcohol of the formula $C_{19}H_{38}O$. But various observations soon pointed to the incorrectness of this interpretation, and independently and almost simultaneously Fernholz (*J. Am. Chem. Soc.*, **60**, 700, 1938), Bergel, Todd and Work (*J.C.S.*, 1938, 235), and Karrer, Salomon and Fritsche (*Helv. Chim. Acta*, **21**, 309 and 520, 1938) suggested that the tocopherols were heterocyclic derivatives, containing either a coumarane or a chromane skeleton. Thus the formula for α-tocopherol would be either (VI) or (VII), whilst that for β-tocopherol would be similar, except that one of the two methyl groups marked * is here replaced by H.



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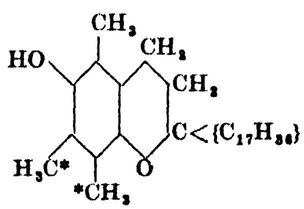


(VII)

Karrer surmised that the group $C_{18}H_{37}$ or $C_{17}H_{35}$ is derived from isoprene units like phytyl alcohol (VIII), a view supported by the fact that phytol is found to be present in wheat germ oil along with the tocopherols.

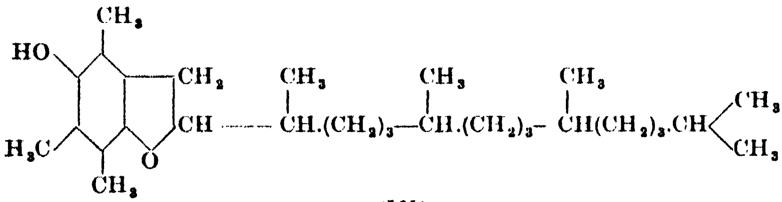
$$CH_3{-}\underset{\displaystyle CH_3}{\underset{|}{CH}}{-}(CH_2)_3{-}\underset{\displaystyle CH_3}{\underset{|}{CH}}{-}(CH_2)_3{-}\underset{\displaystyle CH_3}{\underset{|}{CH}}{-}(CH_2)_3{-}\underset{\displaystyle CH_3}{\underset{|}{C}}{=}CH.CH_2OH$$

(VIII)

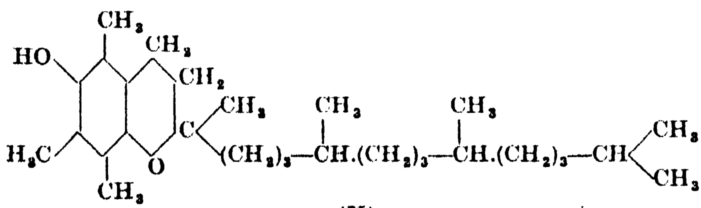
He proceeded to prove the correctness of the suggested formula by a direct synthesis of α-tocopherol (Karrer, Fritsche, Ringier, Salomon, *Helv. Chim. Acta*, **21**, 520, 1938). He heated phytyl bromide with pseudo-cumoquinol in an inactive solvent with zinc chloride as condensing agent. The product closely resembled but was not identical with α-tocopherol. It was, in fact, the racemic compound, the natural α-tocopherol being a stereoisomer (though

possessing little or no optical activity). This was proved by the fact that the synthetic compound was biologically active in a dose of 6 mg., and that it was resolved by fractional crystallisation as the bromocamphorsulphonate (Karrer, Fritsche, Ringier, Salomon, *Nature*, **141**, 1057, 1938). In this way a compound was obtained identical with the bromocamphorsulphonate of the naturally occurring α-tocopherol.

More recently an independent synthesis has been reported by Bergel, Jacob, Todd and Work (*Nature*, **142**, 36, 1938). They find that phytol itself readily condenses with pseudo-cumoquinol in presence of zinc chloride to yield racemic α-tocopherol. Both of these syntheses are compatible with the formulation of the compound as either (IX) or (X). But the weight of evidence appears to favour the latter (*cf.* John, *Naturwiss.*, **26**, 449, 1938). The final settlement of this problem is scarcely likely to be long delayed.



(IX)



(X)

GEOLOGY. By G. W. Tyrrell, A.R.C.Sc., D.Sc., F.R.S.E., The University, Glasgow.

Metamorphism and Metamorphic Rocks.—In two papers K. H. Scheumann ("Zur Nomenklatur migmatitischer und verwandter Gesteine," *Min. u. Petr. Mitt.*, **48**, 1937, 297–302; "Metatexis und Metablastesis," *ibid.*, 402–12) discusses and clarifies the nomenclature of injected rocks. Metatexis appears to be invasion of the host-rock by definite migmatitic vein-systems; metablastesis is invasion by swarms of blastocrysts forming vaguely defined bodies.

The thermodynamics of Riecke's principle, which is employed to explain the recrystallisation of mineral grains under regional metamorphism, is the subject of an important paper by H. Seng

(*N.J. f. Min.*, *B.-B.*, **73**, Abt. A, 1937, 239–308). Apparently the operation of the principle leads to "pseudofluidal" behaviour of the affected rocks, and produces what is essentially a plastic mechanism of deformation and recrystallisation.

In a further paper H. Seng deals with "Die Migmatitfrage und der Mechanismus parakrystalliner Prägung" (*Geol. Rundsch.*, XXVII, 1936, 471–92) with special reference to Riecke's principle.

R. Brauns ("Sanidinite und Sanidingesteine," *Min. u. Petr. Mitt.*, **48**, 1936, 248–52) opposes G. Kalb's views of the origin of these rocks from the contact-metasomatism of crystalline schists (see SCIENCE PROGRESS, XXXI, 1937, 514). G. Kalb ("Sanidinite oder Sanidingesteine," *ibid.*, 253–4) reaffirms his former conclusions.

From work on the flinty-crush rocks of Glen Coe, R. W. van Bemmelen ("Over de zoogenaamde Smeltmylonieten [= Pseudotachylieten"], *Geol. en Mijnbouw*, **15**, 9, 1936, 74–9) concludes that an extra source of heat, *e.g.* the contemporaneous granitic fault intrusion in the case of Glen Coe, is necessary for the formation of pseudotachylyte and related rocks. It is not believed that the rock powder arrives at complete fusion, and that the often-noted appearance of intrusion is due to the mobile powder being drawn into the fissures produced by crushing in a brittle rock.

G. E. Goodspeed has discussed the "Development of Quartz Porphyroblasts in a Siliceous Hornfels" (*Amer. Min.*, **22**, 1937, 133–8). Quartz is usually placed near the end of the crystalloblastic series, and rarely forms euhedral crystals under conditions of recrystallisation. The earlier quartz crystals in the hornfels dealt with are irregular in shape and full of inclusions of the groundmass; but in later stages of recrystallisation the quartz clears itself of inclusions, and often assumes a euhedral shape, especially when associated with pyrites. It is believed that diffusion of volatiles or solutions have caused this unusual phenomenon.

A somewhat similar mechanism is invoked by the same author (*ibid.*, 1133–8) for the origin of plagioclase porphyroblasts in biotite- and hornblende-bearing hornfels formed near granite contacts in N.E. Oregon. The final stage of the process produces rocks of distinctly igneous aspect.

Dealing with the "Paragenesis of Kyanite-eclogites," C. E. Tilley (*Min. Mag.*, XXIV, 1936, 422–32) suggests that the appearance of kyanite as a mineral phase in eclogites results from the presence of alumina in excess of the amount that can be accommodated in the usual bimineralic pyroxene-garnet assemblage. The chemical composition of kyanite-eclogites agrees with that of the Porphyritic Central Magma type of the Hebridean region.

Arising out of the foregoing investigation C. E. Tilley has studied the "Paragenesis of Kyanite-amphibolites" (*Min. Mag.*, XXIV, 1937, 555–68). In view of the known ability of kyanite substance to migrate during stress metamorphism, it is suggested that the origin of kyanite in these amphibolites is traceable to migration and segregation during the conversion of eclogites into the amphibolite stage.

In describing "Anthophyllite-cordierite-granulites of the Lizard," C. E. Tilley (*Geol. Mag.*, LXXIV, 1937, 300–9) remarks that the anthophyllite-cordierite assemblage provides a good example of mineral convergence in metamorphism. The Fennoscandian sulphide-ore fields exhibit it as derived from the metasomatism of siliceous leptites. The Lizard area affords examples of its derivation, (*a*) by metasomatism of basic igneous rocks, and (*b*) by regional metamorphism of argillaceous sediments.

"Cummingtonite-bearing Rocks from the Lewisian" are described by C. E. Tilley (*Geol. Mag.*, LXXV, 1938, 76–81). Three types are distinguished : (1) Cummingtonite-garnet-schist $\pm$ quartz ; (2) Cummingtonite-garnet-sideroplesite-ankerite-schist ; and (3) Banded quartz-knebelite-magnetite-schist with accessory garnet and cummingtonite. Both cummingtonite and knebelite in these rocks are believed to have been derived from reactions between ferruginous carbonates and quartz, the original rocks being carbonate-rich ferruginous and magnesian sediments.

F. C. Phillips's "A Fabric Study of some Moine Schists and Associated Rocks" (*Quart. Journ. Geol. Soc.*, XCIII, 1937, 581–620) is valuable, not only in regard to the metamorphism of the Moine Schists, but also because of the introductory section, which gives a concise general account of *Gefügekunde* (petrofabrics or petrotectonics), and English equivalents of the many recondite technical terms that have been invented by German authors. On the basis of this method of study Dr. Phillips suggests that the Moines arrived at their present state of regional metamorphism by a deformation acting along approximately S.W. to N.E. lines, subsequently to the intrusion of the Carn Chuinneag and Inchbae augen-gneisses, but prior to the dislocation-phase of the Caledonian movements.

In his memoir on "Metamorphic Correlation in the Polymetamorphic Rocks of the Valla Field Block, Unst, Shetland Islands," H. H. Read (*Trans. Roy. Soc. Edin.*, LIX, Pt. I, 1937, 195–221) shows that three metamorphisms under very different physical conditions have affected the rocks of the western part of Unst. The mineral assemblages produced during each metamorphism in rocks of different composition—pelitic, calcareous, siliceous, mig-

matitic, granitic and hornblendic—have been correlated both isophysically and isochemically. It is concluded that in polymetamorphism the stability of an assemblage depends on the bulk-composition of the rock, and that the minerals produced cannot be considered apart from the rocks in which they occur.

C. E. Tilley describes " Eulysites and Related Rock Types from Loch Duich, Ross-shire " (*Min. Mag.*, XXIV, 1936, 331–42), and concludes that these iron-rich fayalite-bearing rocks have been derived by high-grade metamorphism from some type of ferruginous sediment.

" Eclogites in the Neighbourhood of Glenelg, Inverness-shire," described by A. R. Alderman (*Quart. Journ. Geol. Soc.*, XCII, 1936, 488–530), occur as bands and lenticles restricted to inliers of Lewisian Gneiss, and have the same N.N.E.–S.S.W. strike as the limestones and igneous gneisses of the Lewisian complex. The true eclogites form cores to bands composed of amphibolised eclogite, garnet-amphibolite and hornblende-schist, which have been injected by the acid members of the Lewisian gneiss. It is believed that the eclogite bodies have formerly been of considerably greater extent, and that much of the surrounding banded gneiss is of eclogitic parentage.

In a paper on " Der Eklogite von Gilsberg im Sächsischen Granulitgebirge und seine metamorphen Umwandlungsstufen," H. Hentschel (*Min. u. Petr. Mitt.*, **49**, 1937, 42–88) suggests the derivation of eclogite from peridotite by, first, the formation of a pyroxenite facies through the reaction of peridotite with granite-pegmatite solutions, and then the de-mixing of the early pyroxenes into garnet and pyroxene with recrystallisation.

An " intracrystalline " conglomerate horizon with its accompanying coarse and fine clastic sediments, which occurs within the East Sudetic Block, is described by K. H. Scheumann in his paper on " Konglomerattektonite und ihre Begleitgesteine in der epizonalen Schieferscholle südlich von Strehlen in Schlesien " (*Min. u. Petr. Mitt.*, **48**, 1937, 325–72). The sedimentary series carries material derived from an older granite, now the Strehlen Gneiss. This occurrence is of regional significance for understanding the precrystalline composition of the East Sudetic Block, and of general significance for the study of mixed gneisses.

A. Schüller directs attention to striking petrographic and tectonic phenomena in the gneisses of the Fichtelgebirge (*Geol. Rundsch.*, XXVII, 1936, 260–75). They have all suffered an epizonal dislocation-metamorphism which has transformed them largely into mylonite-gneisses. In the contact-zones of the Variscian granites,

the gneisses have been reconstituted under katazonal conditions. Finally, a large-scale tectonic of Alpine type has given rise to extensive crumpling of the rocks.

According to H. Ebert ("Hornfelsbildung und Anatexis im Lausitzer Massiv," *Zeitschr. d. Deutsch. Geol. Ges.*, **87**, 1935, 129–47), the Lausitz granite mass is intruded into sediments of granite-like composition. On its northern border the Kulm greywackes and slates have suffered normal hornfelsing; but large masses of the same sediments in the interior of the intrusion have been completely granitised, their composition, of course, greatly facilitating the transformation. The "two-mica" granite of Lausitz is regarded as the final product of the granitisation process.

The following are three recent studies on metamorphism from the region of the Alps: F. Angel and R. Staber, "Migmatite des Hochalm-Ankogel-Gruppe (Hohe Tauern)" (*Min. u. Petr. Mitt.*, **49**, 1937, 117–67); F. Kümel, "Über basische Tiefengesteine der Zentralalpen und ihre Metamorphose" (*ibid.*, 415–41); H. Wiesenader, "Ergänzungen zu den Studien über die Metamorphose in Alt-Krystallin des Alpen-Ostrandes" (*ibid.*, **48**, 1937, 317–24).

"Geological Observations in the Opdal-Sunndal-Trollheimen District," by O. Holtedahl (*Norsk. Geol. Tidsskr.*, **18**, 1938, 29–53), lead to the conclusion that the rocks of this district in southern Norway, formerly thought to be Archæan, are mainly Eo-Cambrian feldspathic sandstones (Sparagmite) in a highly metamorphic facies. It is shown that non-metamorphic Sparagmite grades into a schistose variety, and that into granulitic types. T. F. W. Barth (*ibid.*, 54–65) describes this progressive metamorphism, which was accompanied by regional metasomatism involving an increase in the proportions of soda and alumina. Solutions from which albite was precipitated permeated the Sparagmite sediments along the Caledonian folding zone. The basal gneisses of Opdal are interpreted as migmatitised Sparagmites.

An important memoir by L. Royer on "Les terrains crystallophylliens des massifs d'Alger et de la Grande Kabylie" (*Bull. Sérv. Carte Géol. de l'Algerie*, 5^e Sér. Pétrographie, No. 2, 1937, 76 pp.) deals with a formation of sericite-chlorite-schists, mica-schists, gneisses, cipolins and amphibolites, which underlies the Burdigalian, and is probably to be correlated with pre-Devonian metamorphic formations which occur in other parts of Algeria. The original sediments first suffered a regional metamorphism, and later, powerful movements which produced an extensive series of mylonites.

In his paper "L'Émeri de Samos," J. de Lapparent (*Min. u. Petr. Mitt.*, **49**, 1937, 1–30) shows that the rocks of Samos which

have been called "emery" are pisolitic types with diaspore, but devoid of true corundum. They have probably been derived from diaspore-bearing bauxites, first by shearing stress with the recrystallisation of diaspore and the formation of magnetite and chloritoid, and secondly by the epitaxial formation of a mixture of aluminous spinel and taosite (titanoferrous corundum). The name *samosite* is proposed for this series of rocks resulting from the metamorphism of ancient bauxites.

Discussing the "Origin of the Emery Deposits near Peekskill, New York," J. W. Butler, Jr. (*Amer. Min.*, **21**, 1936, 537–74) shows that they are of contact-metamorphic (pyrometasomatic) origin, and were formed during the early liquid-magmatic stage of the basic Cortlandt intrusions by the passage of emanations through the country-rocks consisting of Manhattan Schists.

In the second part of his "Structural and Petrologic Studies in Dutchess County, New York," T. F. W. Barth deals with the "Petrology and Metamorphism of the Palæozoic Rocks" (*Bull. Geol. Soc. Amer.*, **47**, 1936, 775–850). Three different mineral facies occur in the Hudson River Slate as it is traced from N.W. to S.E.; the muscovite-slate facies, the kyanite-schist facies, and the sillimanite-gneiss facies. There are also local developments of chlorite-schist, glaucophane-chlorite-schist and chloritoid-schist. The argillaceous sediments grade into gneissic rocks through the intermediate stages of slate, phyllite and schist. The conclusion is reached that, during the orogenic period, the sediments were heated and stewed in liquids of magmatic and anatectic origin, which reacted with the sediments and metasomatically transformed them into well-defined types of schists and gneisses. This memoir, in which eighteen new chemical analyses of metamorphic rocks are given, is full of interesting and important detail on metamorphism, which defies short summary.

A memoir comparable in size and importance to the foregoing is M. P. Billings's "Regional Metamorphism of the Littleton-Moosilauke Area, New Hampshire" (*Bull. Geol. Soc. Amer.*, **48**, 1937, 463–566). The region consists of a great variety of sedimentary and igneous rocks in various stages of metamorphism. Plutonic rocks of several ages are abundant, and the New Hampshire Magma Series was contemporaneous with the main folding of Palæozoic (post-Devonian) age. A progressive increase in the intensity of metamorphism towards the south-east is ascribed to increasing temperature, probably due to the great abundance of igneous intrusions in that direction. Migrating solutions and stress have also played considerable parts in the metamorphism.

In a paper on "Introduction of Potash during Regional Metamorphism in Western New Hampshire" the same author (*ibid.*, **49**, 1938, 289–302) shows that argillaceous sediments first recrystallised, even in the sillimanite zone, without significant change in chemical composition except loss of water; but in a later metasomatic phase, potash was introduced with a consequent increase in the potash-alumina ratio. The earlier stage was regional in its incidence, but the later stage was local, leaving areas of sillimanite-schist untouched.

M. P. Billings and R. P. Sharp have carried out a petrofabric study of a high-grade mica-schist from Mt. Clough, New Hampshire, which contains a practically undistorted fossil *Spirifer* (*Amer. Journ. Sci.*, XXXIV, 1937, 277–92). The rock is shown to be a good tectonite. It is concluded that the differential motion of the rock involved slipping of sheets of minerals past one another parallel to the original bedding. If the individual grains had rolled to any extent, the fossil would certainly have been destroyed.

F. Fitz Osborne describes the structure and composition of Grenville rocks and their relation to the neighbouring red granite-gneisses in his paper on "Petrology of the Shawinigan Falls District (Ontario)" (*Bull. Geol. Soc. Amer.*, **47**, 1936, 197–228). The lower part of the Grenville consists of amphibolites derived from basic volcanic rocks; the upper part is the result of high-grade metamorphism of sedimentary rocks. In both series the metamorphism belongs to the sillimanite grade.

According to R. W. Chapman, the "Contact-Metamorphic Deposit of Round Valley, California" (*Journ. Geol.*, XLV, 1937, 859–71) consists of a roof-pendant of limestones, argillaceous sandstones and pure sandstones, entirely surrounded by granite. The sandstones have been changed to hornfels, and the limestones to complex lime-silicate rocks and marbles. The rocks contain tungsten in the form of scheelite.

In his paper on "The Contact Zone of Sheep Creek, Montana," J. H. Taylor (*Geol. Mag.*, LXXV, 1938, 219–26) describes the effects of lamprophyric intrusions on a series of Cambrian shales and limestones. Calc-silicates have been formed in the limestones, but the shales have been little affected. Silica, alumina, iron, and especially magnesia, have been introduced by hydrothermal solutions into the altered rocks.

The study of metamorphism in New Zealand progresses apace in the hands of F. J. Turner and his colleagues. In a paper on the "Interpretation of Schistosity in the Rocks of Otago, N.Z.," F. J. Turner (*Trans. Roy. Soc. N.Z.*, **66**, 1936, 201–24) traces the

history of the terms schistosity and foliation, and applies the petrofabrics method to Otago schists with sub-horizontal or uniformly tilted schistosity, to which Daly's hypothesis of load-metamorphism might be applied. In such rocks the fabric should be symmetrical about a line perpendicular to the schistosity; but most of the rocks investigated show a pronounced linear element suggestive of symmetry about a line lying within, or inclined at a low angle to, the schistosity. The rocks are tectonites whose complex fabrics have been determined by shearing along low-hade thrusts, and the hypothesis of load-metamorphism is thereby excluded.

The same author has described "The Metamorphic and Plutonic Rocks of Lake Manapouri, Fiordland, N.Z." (*ibid.*, **67**, 1937, 83–100; 227–49). The oldest rocks are steeply dipping, regularly banded paragneisses containing interstratified igneous rocks and tuffs of both basic and acid characters. This basement series has been injected by a great, compound, synchronous batholith of trondhjemite and oligoclase-granite with their accompanying pegmatites. This mass is believed to have been squeezed up while folding movements were in progress, and immediately after the metamorphism of the invaded gneisses. The Pomona Island Granite, however, is regarded as a subsequent batholith intruded under totally different physical conditions after the compressional movements had almost ceased.

A general account of "Progressive Regional Metamorphism in Southern New Zealand" is given by F. J. Turner (*Geol. Mag.*, LXXV, 1938, 160–74) on the basis of work carried out during the last ten years. A brief summary of the mineral assemblages in the various metamorphic zones is given, and attention is drawn to certain mineralogical and structural peculiarities which differentiate the New Zealand schists from those of classical European areas. The early crystallisation of biotite without intervening development of chlorite is discussed, and it is suggested that this phenomenon is due to increased temperatures under the influence of rising granitic magmas.

INVERTEBRATE PALÆONTOLOGY. By H. DIGHTON THOMAS, M.A., Ph.D., F.G.S., British Museum (Natural History).

AN important revision is that carried out by J. Bridge and G. H. Girty, who have given "A Redescription of Ferdinand Roemer's Paleozoic Types from Texas," in *U.S. Geol. Surv. Prof. Paper*, CLXXXVI M, 1937, 239–71. The descriptions are accompanied by new figures of the types, and by figures of topotypes, as well as of other species considered synonymous with Roemer's species.

The stratigraphical horizons of the specimens have been accurately determined, and the geographical distribution of several of the forms has been much extended.

"Ordovician and Silurian Faunas from Arctic Canada" are the subject of a long memoir by C. Teichert, *Rept. Fifth Thule Exped. 1921–24*, I, 5, 1937, 1–169 (reprinted in *Mus. Min. Géol. Univ. Copenhague*, *Comm. Paléont.*, LIX, 1937). The evidence points to the greater part of the Ordovician faunas being of Trenton age, while three distinct Ordovician transgressions can probably be distinguished in the Arctic regions. The Silurian fossils which Dr. Teichert describes are all of Niagaran age.

The same author describes a new basal Trenton fauna from Washington Land, North Greenland, in *Meded. om Grønland*, CXIX, (1), 1937, 1–65, while the three Lower Ordovician faunas of East Greenland which C. Poulsen analyses (*ibid.*, CXIX, (3), 1937, 1–72) can be correlated with the British Tremadocian, the American Upper Canadian and the American Chazyan respectively.

W. H. Twenhofel deals with the "Geology and Paleontology of the Mingan Islands, Quebec," in *Geol. Soc. Amer. Spec. Papers*, XI, 1938, 1–132. The fossils come from the Romaine and Mingan formations, and show that the former is to be correlated with the upper part of the Beekmantown formation, and the latter with the Chazy formation. Several of the species are new, and there is one new genus of cephalopods.

R. R. Shrock and G. O. Raasch (*Amer. Midl. Nat.*, XVIII, 1937, 532–607) have examined the fauna of the highly disturbed Ordovician rocks near Kentland, Indiana, and show that the fossils, which occur at three faunal horizons, are of Black River age. They include corals, stromatoporoids, bryozoa, brachiopods, many pelecypods and gastropods, some cephalopods, trilobites and ostracods.

The "Fauna der Sandsteine von Golonóg," which is described by S. Weigner in *Bull. Serv. Géol. Pologne*, IX, (2), 1938, 3–79, is considered to belong to the upper part of the Lower Carboniferous. It is essentially molluscan in character (including some cephalopods), but contains other groups as well.

T. S. Jones recognises two easily distinguishable formations as a result of his study of the "Geology of Sierra de la Peña and Palæontology of the Indidura Formation, Coahuila, Mexico," *Bull. Geol. Soc. Amer.*, XLIX, 1938, 69–150. The Middle Albian Aurora formation yields brachiopods and molluscs, with rudistids common towards the top, while in the Indidura formation (Middle Albian-Turonian) mollusca are numerous and include abundant ammonites. Thus, though the sea withdrew from the United States at the end

of the Albian, it persisted in parts of Mexico throughout much of later Cretaceous times.

In addition to discussing the stratigraphy of the coastal region of Syria, L. Dubertret and others write on its palæontology (*Haut-Comm. Répub. Franç. Syrie Liban Serv. Trav. Publ. Sect. Études Géol. Notes Mém.*, II, 1937, 1–230). Jurassic and Cretaceous hydrozoa are represented, as well as Aptian and Albian echinoids, and ammonites from Albian to Senonian horizons. These faunas point to comparisons with the western Mediterranean. In addition, Eocene rocks are also present, and yield a Lutetian suite of larger foraminifera.

"The Lower Cambrian *Olenellus* Zone of the Appalachians" yields a fauna considered by C. E. Resser and B. F. Howell (*Bull. Geol. Soc. Amer.*, XLIX, 1938, 195–248) to be either closely related to or identical with the *Olenellus* faunas of the lower Mount Whyte, Hota, and Eager formations of British Columbia. Its generic assemblage suggests that it is ancestral to the Burgess Shale fauna, which contains almost all the genera represented in the Appalachian *Olenellus* fauna except the Olenellids. Noteworthy elements, in addition to the trilobites, are the gastropods and also the fragile fossils preserved in the Kinzers formation.

C. E. Resser analyses the "Middle Cambrian Fossils from Pend Oreille Lake, Idaho," in *Smithson. Misc. Coll.*, XCVII, (3), 1938, 1–12. The area is isolated from other Cambrian outcrops. Its strata include three Middle Cambrian horizons, the uppermost two yielding fossils which indicate their relation to the Mount Stephen formation. Middle Cambrian trilobites are also described from West Sayan, Siberia, by O. K. Poletayeva in *Rec. Geol. West Siberian Region*, XXXV, 1936, 25–54, who shows that they must have lived in an extremely isolated basin of deposition, but were related to the North American fauna.

P. T. Cox discusses "The Genus *Loftusia* in South Western Iran" (*Eclog. geol. Helvet.*, XXX, 1937, 431–50), and recognises five valid species, of which three are new. He points out that their initial microspheric spires closely resemble the spire of *Choffatella decipiens* Schlumb. in equatorial section, and *Cyclammina* in axial section. He considers it most probable that *Loftusia* was derived from a Lituolid ancestry.

H. Bartenstein and E. Brand have contributed an important article on "Mikro-paläontologische Untersuchungen zur Stratigraphie des nordwest-deutschen Lias und Doggers," in *Abhandl. Senckenberg. Nat. Gesellsch. Frankfurt*, 439, 1937, 1–224. They show that there is a definite stratigraphical distribution of the

foraminiferal assemblages in the rocks with which they deal. Similar results have been obtained by W. Mohler for some foraminifera from horizons ranging from Bajocian to Kimmeridgian in Northern Switzerland. There the foraminiferal faunas reflect the facies conditions (*Abhandl. Schweiz. Palaeont. Gesellsch.*, LX, 1938, 1–53).

Palæozoic stromatoporoids from Victoria are the subject of three papers by E. A. Ripper (*Proc. Roy. Soc. Vict.*, XLIX, (2), new series, 1937, 178–205 ; *ibid.*, XL, (1), new series, 1937, 1–8, and 11–38). In the first of these, Miss Ripper shows that the stromatoporoid fauna of the Lilydale Limestone has little in common with that of the British Wenlock, but that its advanced characters point rather to a correlation with European and North American Devonian faunas. She shows, in the second paper, that the stromatoporoids of the Yeringian of Loyola appear to be older than the Lilydale fauna. On the other hand, the assemblages from the limestones of the Buchan district (with which the third paper deals) have, in the main, definite Middle Devonian affinities, though some typical Wenlock species persist. The limestones at Rocky Camp, however, may be on a lower horizon because of the high proportion of Upper Silurian and Lower Devonian forms.

G. A. Kellaway and S. Smith describe the first Jurassic stromatoporoids from England (*Quart. Journ. Geol. Soc. London*, XCIV, 1938, 321–30), and figure two French forms at the same time. The English specimens are from the Aalenian and Vesulian and appear to be the oldest Jurassic stromatoporoids known. Upper Jurassic hydrozoa (including stromatoporoids) and some corals are described from Italian East Africa by R. Zuffardi-Comerci in *Palaeont. ital.*, XXXII, suppl. 3, 1938, 1–3.

In " Some Further Considerations on Trends in Corals," W. D. Lang (*Proc. Geol. Assoc.*, XLIX, 1938, 148–59) shows that trends may appear within a family, a genus, or even a single species. One consequence of their operation is that the orthodox genera cut across the lineages. Moreover, certain genera, hitherto considered distinct, owe their differences to the temporary expression of a trend. Thus, certain generic names should lapse as systematic units, but Dr. Lang advocates their retention as morphological terms to indicate such genomorphs, as he has termed them. He applies the principles he advocates to the Devonian corals of the group of *Disphyllum*, and shows that several trends and genomorphs can be recognised.

G. A. Stewart gives a critical systematic study of the coral-fauna of a long-famous coralliferous group in her paper " Middle Devonian Corals of Ohio " (*Geol. Soc. Amer. Spec. Papers*, VIII,

1938, 1–120). One hundred species are discussed, and are divided among 31 genera, of which one is new. The distribution of the species both stratigraphically and geographically within the state is also dealt with, and the authoress shows that 78 are represented in the Colombus Limestone.

Other important papers on corals are the following: L. B. Rukhin, "The Description of some *Favosites* from the Upper Silurian-Lower Devonian Deposits of the Transbaikal District," *Scient. Mem. Leningrad State Boubnoff Univ.*, X, 1936, 96–110; W. Weissermel, "Eine altpaläozoische Korallenfauna von Chios," *Zeit. Deut. Geol. Gesellsch.*, XC, 1938, 65–74; O. H. Schindewolf, "Zur Kenntnis der Gattung *Zaphrentis* (Anthoz., Tetracorall.) und der sogenannten Zaphrentiden des Karbons," *Jahrb. Preuss. Geol. Landesanst.*, LVIII, 1938, 439–54.

"Some Aspects of Evolution in Echinodermata," by W. K. Spencer (in "*Evolution: Essays on Aspects of Evolutionary Biology presented to Professor E. S. Goodrich on his Seventieth Birthday.*" Edited by G. R. de Beer, 1938, Clarendon Press, Oxford), is concerned essentially with the feeding mechanism of the group. On this basis, Dr. Spencer proposes a new classification dividing the Echinodermata into Dactylozoa and Podozoa. The former had no tube-feet but collected their food by ciliary currents created on long thin projections ("fingers" or "brachioles"): these comprise the Cystoidea, the Blastoidea, the Edrioasteroidea, and *Dendrocystis* and its allies. The Podozoa, on the other hand, had tentacles, and include the Crinoidea, the Asterozoa, the Echinozoa, the Holothuroidea and the extinct Carpoidea.

E. W. MacBride and W. K. Spencer give detailed accounts of the structure of "Two new Echinoidea, *Aulechinus* and *Ectinechinus*, and an Adult plated Holothurian, *Eothuria*, from the Upper Ordovician of Girvan, Scotland," in *Phil. Trans. Roy. Soc. Lond.*, B, CCXXIX, 1938, 91–136. The two echinoids are like most Palæozoic forms in having interambulacral areas composed of many rows of plates and a flexible test, but show characters more primitive than those known in early echinoids. Some of these features can be directly associated with primitive features which can be deduced from the embryology of Recent forms. *Aulechinus* has the general shape of a regular urchin, but *Ectinechinus* is elongated like an irregular echinoid, differing, however, in that the ambulacra terminate apically in a ring round the anus. The former probably rested under sand, whereas the latter almost certainly burrowed. The belief that *Eothuria* is the first-known plated holothurian is supported by the absence of ocular plates, and by the

ambulacral pores being unlike those of any known eleutherozoan. This form almost certainly burrowed.

U. S. Grant IV and L. G. Hertlein have monographed " The West American Cenozoic Echinoidea " in *Publ. Univ. California Los Angeles Math. Phys. Sci.*, II, 1938, 1–225. Necessarily, a few new genera and species are created by the authors.

" Types of Carboniferous Bryozoa of the European Part of the U.S.S.R." are described by A. Nikiforova in *Paleont. U.S.S.R. Acad. Sci. U.S.S.R.*, IV, (5), fasc. 1, 1938, 1–290. The classic collections of Keyserling, Eichwald, and Stuckenberg were examined, as well as new ones made at their type-localities and at those of Fischer. This revision of a neglected group of common Carboniferous fossils (and a few Lower Permian forms are also considered) shows that the species are well-characterised. Several new genera and species are erected.

Rhætic polyzoa are of infrequent occurrence, so that their discovery by F. Prantl in beds of that age in the Carpathians is important. He describes a new species of *Berenicea* and a species of *Stomatopora* in " Erster Fund von Bryozoen in dem Karpathischen Rhät " (*Zentralb. für Min., etc.*, 1938, B, 262–4).

" Brachiopoda of the Cambrian and Silurian Systems of U.S.S.R., Fasc. 1, Upper Silurian Brachiopoda of the Central Asiatic Part of U.S.S.R." form the subject of an important work by A. Nikiforova in *Paleont. U.S.S.R. Monog.*, XXXV, 1937, 1–93. As far as the material allowed, attention was paid to the internal structures as well as to the exteriors of the valves. New genera, sub-genera, and species are described by the authoress, who finds that the Upper Silurian brachiopods with which she deals are related to those of the Urals and Bohemia.

The Devonian Cedar Valley Limestone of Iowa yields abundant species of *Atrypa* and *Stropheodonta*, which have been examined by M. A. Stainbrook (*Journ. Paleont.*, XII, 1938, 229–56). Most of the species are well characterised and short-ranged, so that they are good horizon-markers. None of the species occurs in the Devonian beds of Missouri or of eastern North America.

The Chonetidæ of the Pennsylvanian and Permian of North-Central Texas are also useful zone-markers, because of their limited range and wide distribution. The associated Productidæ, however, are less useful, being more restricted geographically. Both families are considered by R. H. King (*ibid.*, 257–79).

A. P. Ivanov and E. A. Ivanova have published the results of their studies on the " Brachiopoda of the Middle and Upper Carboniferous of the Moscow Basin (*Neospirifer*, *Choristites*)," in *Trav. Inst.*

Paléozool. Acad. Sci. U.R.S.S., VI, (2), 1937, 1–215. The degree of development of the apical callosity and the size of the apical plates are good criteria for the identity of the species of *Neospirifer*. The internal structures of *Choristites* were also analysed, and the authors were able to study variation in the plates. The distribution of the species in the Moscow Basin is also shown to be of stratigraphical value.

In "Notes on the Morphology of *Schizophoria*," T. N. George and D. R. A. Ponsford, *Trans. Leeds Geol. Assoc.*, V, (4), 1936–7, (1938), 227–45, show that in some species the punctae do not certainly pass completely through the test. The cardinal process is markedly variable in the small group of British species, while the brachiophore plates appear to be strongly developed. The muscle field of the ventral valve is divided by a prominent septum, which is of variable development within a species.

The "Gastropoda. Teil 1: Allgemeiner Teil und Prosobranchia" are considered by W. Wenz (*Handb. Paläozool.*, VI, 1938, 1–240). Sections dealing with the anatomy, morphology, ecology, and views on the phylogeny of the gastropods, are followed by diagnoses and figures of the many genera known.

The gastropods of the important Permian occurrence at Sosio, Sicily, are revised by B. Greco in "La Fauna Permiana del Sosio conservata nei Musei di Pisa, di Firenze e di Padova," *Paleont. ital.*, XXXVII, 1937, 57–114. They support the conclusions based on the ammonite evidence of the correlation of the Sosio limestone with the *Waagenoceras* zone of Guadeloupe and the Upper Artinskian of the Urals.

J. Gardner describes "The Molluscan Fauna of the Alum Bluff Group of Florida. Part VI. Pteropoda, Opisthobranchia, and Ctenobranchia (in part)" in *U.S. Geol. Surv. Prof. Paper*, CXLII F, 1938, 251–435. She deals with 241 species and sub-species, but less than 10 per cent. are common forms. The *Terebras* and turritids, and certain other groups, are littoral forms and sharply reflect the ecology and the relationship of the fauna to that of shores near by.

A revision of "The Claibornian Scaphopoda, Gastropoda and Dibranchiate Cephalopoda of the Southern United States" has been carried out by K. V. W. Palmer (*Bull. Amer. Paleont.*, VII, 32, Part I (Text), Part 2 (Plates), 1937, 1–730). This large monograph is concerned with the systematics of those mollusca, and includes descriptions of several new forms.

L. F. Spath reaches some unexpected conclusions as a result of his elaborate studies which are published in "A Catalogue of the Ammonites of the Liassic Family Liparoceratidae in the British

Museum (Natural History)," *Cat. Brit. Mus. Nat. Hist.*, 1938. Contrary to generally accepted opinion, Dr. Spath, on incontestable stratigraphical evidence, concludes that the more or less involute, often sphærocone Liparoceratids were ancestral to the more evolute (dimorph) forms, and that these in turn gave rise to the capricorns, which are the most evolute Liparoceratids. Further, he shows that new characters have arisen cænogenetically, so that, in this group, recapitulation in ontogeny is no criterion of the evolution.

All workers on ammonites will welcome the large volume by F. Roman on "Les Ammonites jurassiques et crétacées. Essai de Genera" (4to, Paris). The author states and figures the genotypes of the various genera, and discusses the validity of the latter.

In a paper on "Ammonites of the Taraises Formation of Northern Mexico" (*Bull. Geol. Soc. Amer.*, XLIX, 1938, 539–602), R. W. Imlay is able to show that the stratigraphical divisions of that Neocomian horizon can be recognised over a wide area in Mexico, and that it contains a large and distinctive fauna. Several of the species and two of the genera described are new.

A. K. Miller and W. M. Furnish describe the representatives of the nautiloid genus *Aturia* in the Eocene and Oligocene of Mexico, and, as a result of a discussion of the morphology and phylogeny of the genus, conclude that the recognition of subgenera is impracticable (*Journ. Paleont.*, XII, 1938, 149–55).

"Triopsiden (Crust. Phyll.) aus dem Keuper Frankens" is a paper by F. Trusheim on the structure of a new species of phyllopod. Such a wealth of material is available that the ventral as well as the dorsal surfaces can be reconstructed (*Palæont. Zeit.*, XIX, 1937, 180–98).

E. R. Eller gives "A Review of the Xiphosuran Genus *Belinurus* with the Description of a new Species, *B. alleganyensis*," *Ann. Carnegie Mus.*, XXVII, 1938, 129–50. All the species known and available to the writer are figured by him. Like other Devonian forms, the new species was found in marine strata, whereas the Carboniferous and Permian species are limited to fresh or brackish water deposits, though Jurassic and Recent Limuloids are again found in marine sediments. If the assumption be true that the place of burial indicates the habitat, then it is suggested that the changes in habitat stimulated and then arrested evolution, thus making forms of the Limulida good examples of persistent types.

G. Zalessky writes "Sur un représentant d'un nouvel ordre d'insectes permiens" in *Ann. Soc. géol. Nord*, LXI, 1936, 50–71. A new order Hemipsocoptera is erected for a specimen from near Perm, which is represented by the impression of the complete

insect. Dr. Zalessky considers that the new order is close to, and a direct descendant of, the common ancestors of the Psocoptera and the Homoptera.

BOTANY. By Professor E. J. Salisbury, D.Sc., F.R.S., University College, London.

The Flora of Oland is the subject of a monograph by Rikard Sterner, which is illustrated with nearly 300 distribution maps (*Acta Phytogeographica Suecica*, IX, 170 pages and 64 pages of maps, 1938). This island, which is about 130 kilometres long, is mainly calcareous and boasts a flora of some 1050 species, of which about 820 are regarded as indigenous. Of the 200 species regarded as associated with human activities about 120 are considered as old-established species, whilst amongst those classed as "neosynanthropen" are *Aquilegia vulgaris*, *Bellis perennis*, *Berberis vulgaris*, *Daucus carota*, *Pastinaca sativa* and *Sambucus nigra*. About 10·8 per cent. of the total area is occupied by woodlands which have not been planted, of which half is deciduous woodland, about 23 per cent. *Pinus sylvestris* woodland and 1 per cent. *Picea excelsa*. The information afforded by the numerous maps is too varied to summarise here, but attention may be drawn to the wide distribution on this island of species which are rare members of the continental element in the British Flora. Thus *Orchis ustulata* occurs in a number of localities throughout the island, and another marked calcicole, namely *Seseli libanotis*, though more sparsely distributed, is shown as occurring in a number of stations both in the north and the south of the island. The study of these distribution maps brings home to one the importance of the edaphic factor, especially for species approaching the northern limit of their geographical range.

A paper by H. N. Ridley (*Jour. Bot.*, LXXVI., 144) calls attention to the distinctions between *Arum neglectum* (Townsend) Ridl. and *Arum italicum*. The former is confined to the south of England, the Channel Islands and the north coast of France and is held to be quite distinct from the continental *A. italicum*, which is mainly south European and north African in its distribution. In both, the leaves are formed at the end of the year, that is they are winter green species in contrast with the vernal *A. maculatum*. Whereas the leaves of *A. italicum* are typically dark green with white veins, those of *A. neglectum* are yellowish-green with paler veins and the basal lobes which overlap and leaf as a whole are broader with a sinuate edge. The fruits contain one or two seeds in contrast to the two-four in *A. italicum*.

The results of lysimeter experiments recorded by A. Demelin

and E. Bastisse (*Soil Science*, **46**, 1–7, 1938) are of considerable ecological interest, as they were carried out under natural rainfall over a series of years during which the precipitation ranged from 398·9 mm. to 732·4 mm. From a sandy loam the evaporation ranged from 53·3 per cent. to 72·6 per cent., with an average of about 60 per cent., but when cropped the loss by transpiration and evaporation together amounted to about 17 per cent. more than the evaporation alone from the uncropped soil. The effects of cropping with barley, wheat, oats, maize and carrots respectively had similar effects upon the rate of water-loss, so that it would seem as though the differences in transpiration rates of these plants was approximately compensated by differences in the shelter effect of the crop species on the soil. The average annual loss of CaO was 553·6 kilograms per hectare from bare soil, whereas when cropped the loss due to leaching and to uptake by the plant averaged about 172 kilograms less. From crushed granite containing 0·93 per cent. CaO the loss in five years by leaching was about 0·01 per cent. The base-exchange capacity of the B horizon of a granite soil was 81 me., as compared with 67 me. for the finer clay fraction of the weathered granite.

In the same journal (pp. 9–19) W. H. MacIntire, W. M. Shaw, and B. Robinson report the results of examinations of the leaching action of rainwater on dolomite and limestone of varying degrees of fineness and in varying amounts incorporated with quartz. These show that the rate of leaching of calcium, though increasing with the amount added, is not proportional thereto. With calcic limestone added in amounts of 1 is to 6·6 is to 26·5 the ratio of the rates of leaching expressed as $CaCO_3$ was 1 is to 2·1 is to 2·6. With dolomitic limestone added in the same relative amounts the rates of leaching were respectively 1 is to 3·1 is to 4·9. At the lowest calcium content the degree of division did not make any significant difference in the rate of leaching; with higher rates of addition the finely divided calcic limestone (60–100 mesh) underwent a leaching of calcium that was from 1·1 to 1·2 times that of the coarser material (20–40 mesh) added in the same amount.

At all rates of application the dolomitic limestone exhibited a slower rate of leaching, being at the lowest rate only about one-third of that shown by the calcic limestone, but with the higher rates of application the effect of the increase of surface due to finer division was more pronounced in the dolomitic limestone, the rate of leaching of the 60–100 mesh limestone being about 1·4 times that of the 20–40 mesh.

The effect of wind action upon plants is one concerning which

all too little data is available. V. P. Rao from experiments with *Setaria italica* grown under a wind velocity of 11 miles per hour shows that not only is there an appreciable reduction in growth of the over-ground parts, as Finnel found with marigolds and Clements with sunflowers, but that the root growth was markedly reduced also. The weight of the roots of plants exposed to wind was 59 per cent. that of the control plants, whilst the root volume was 62 per cent. The amount of water transpired was 1·29 times that of the controls (*Bull. Torrey. Bot. Club*, **65**, 229, 1938).

The *Annals of the Missouri Botanical Garden* for April 1938 (XXV, 467–724) is mainly devoted to the results of the Second Byrd Antarctic Expedition. This includes an account of the ecology and geographical distribution, by P. A. Siple, of the Lichens by C. W. Dodge and G. E. Baker, and of the Mosses by E. A. Bartrom. Eighty-five species of lichens and five mosses are recorded, all of which are endemic to the Antarctic.

The result of cytological examination of 2201 twin plants is reported by A. Müntzing (*Hereditas*, XXIV, 487, 1938). Of this total 95, or 4·3 per cent., were found to have chromosome numbers which deviated from the normal. The material included 14 species of grasses, *Trifolium pratense* and *Solanum tuberosum*. Triploids were most frequent (77), haploids numbered 11 and tetraploids 2.

Experimental production of haploids in *Nicotiana rustica* has been attempted by M. A. Ivanov employing various methods. Irritation of ovarian tissue by puncturing, pollination by pollen of other Solanaceous genera, the effect of pollen tube growth for varying periods within the style before the style was removed were all alike unsuccessful, but from pollination with pollen treated with X-rays four haploid plants resulted, which differed from the maternal parents in reduced vigour, smaller size of leaves and flowers and greater sterility (*Genetica*, XX, 295, 1938).

P. G. Fothergill has studied the cytology of wild populations of hybrids between *Viola tricolor* ($n = 13$) and *Viola lutea* ($n = 24$). The hybrid plants were either intermediate in character or resembled more closely *V. lutea* with $2n$ usually from 48 to 52. The gametic numbers varied from 13 to 27, with 23 to 26 as the most common condition. The variation in chromosome number is, it is suggested, an advantage, as providing a greater range of material for selection (*Genetica*, XX, 159, 1938).

Silene otites is a diœcious species in which the female plants bear flowers containing an ovary only, whilst the male plants bear flowers, which in addition to the functional stamens contain a normally abortive ovary, but as a rare condition the ovary may

be here functional. In *Silene otites* the male plants preponderate in the ratio of 1·5 males to 1 female. Species known to be heterogametic for sex in the males show a preponderance of females in the wild populations. In *Mercurialis annua*, where the female is heterogametic for sex, the males preponderate. From breeding experiments Sansome concludes (*Jour. Genetics*, **35**, 387, 1938) that in *S. otites* the female plants are heterogametic for sex, but that in the related *S. pseudotites* the male plants are heterogametic.

PHYSICAL ANTHROPOLOGY. By L. H. DUDLEY BUXTON, M.A., D.Sc., Exeter College, Oxford.

PHYSICAL anthropologists have in the last few years concentrated their attention especially on studies on the evolution of man. The technique of modern archæologists has enabled us to date with some degree of certainty the odd, and often extremely exiguous, fragments of human skulls which turn up from time to time in ancient gravels. A careful comparison of these finds shows that modern man is much more ancient in his physical form than would have been supposed a few years ago. This question is very carefully discussed by Matthew Young who describes the "London skull" in *Biometrika*, XXIX, 1938. The fragments include the greater part of the occipital and left parietal bones and part of the right parietal. The skull was probably that of a female between 40 and 50, and the date is at a conservative estimate Aurignacian, that is Upper Palæolithic, though some archæologists consider that it is much older. The most remarkable character of the fragments is the presence of a preinterparietal bone, that is a supernumerary bone lying anterior to the normal interparietal bone which forms part of the squama of the occipital bone. This abnormality is not uncommon in modern skulls, but recently it has been found in a Neandertal skull excavated near Rome—the so-called Saccopastore skull. This unusual condition of the occipital bone, although not morphologically of any significance, is of importance as it has considerable effect on the measurements of the parietal and occipital arcs and may cause apparent divergence or similarity where none actually exists. Young has very clearly demonstrated this in his paper by taking comparisons between modern skulls with and without the abnormality. Until recently it has been customary to describe ancient skulls by themselves with no consideration of normal variations; such a procedure of course made each individual skull a type. Young has, however, shown that, taking into consideration both the means and standard deviations, the London skull falls well within the range of a series of Scottish skulls in the

anatomical collection in Edinburgh, the differences, where they occur, being due not to morphological variation but to the abnormality already mentioned. In fact, in all its morphological and anatomical characters there is no clear evidence that the skull is primitive, but rather that it falls well within the limits of the modern Scotch type.

Turning to Upper Palæolithic skulls, the London fragment closely resembles the mean values of those of Aurignacian and Solutrean Age, especially the latter, and indeed from such measurements as can be made it seems practically indistinguishable from the so-called Solutré V. Among Neandertal skulls of the Middle Palæolithic period it most closely resembles that from Saccopastore, to which reference has already been made, especially in the unusual flatness of the parietal segment of the sagittal arc. But this flattening does occur among modern skulls and, as has been shown, is due to an anatomical abnormality.

When compared with Piltdown skull, Young declares that in the extent and curvature of its parietal arc, as well as in the proportion of the biasterionic breadth to the maximum parietal breadth, and of the biasterionic breadth to the length of the occipital chord, the London skull is nearer the Scottish than the Piltdown type, while in the fullness of curvature of the lower occipital region it is in quite as close agreement with one type as the other.

There remain two other interesting comparisons, first with the fragments from Bury St. Edmunds which are stated to be of Late Acheulean date, that is the end of the lower Palæolithic. As far as can be judged the two calvaria are similar both in size and shape.

Finally, Young has compared the London skull with the Swanscombe skull, which also is the subject of a special memoir by the finder, A. T. Marston (*Royal Anthropological Journal*, LXXVII, 1937, issued 1938). Young concludes that, as far as the two can be compared, there is no definite evidence to show that the Swanscombe skull diverges any more from the modern type than does the London skull.

Marston's memoir goes into greater detail and will be followed by a special memoir by a committee who are investigating the find. He discusses the actual date with considerable detail, but as his findings are not endorsed entirely by the committee they need not be discussed here. It seems, however, beyond doubt that we accept the skull as being the first definitely authenticated human remains of Acheulean date. The fragments consist of an occipital and a parietal bone and belonged to a person who probably died in her early twenties. Young estimates the length as being about 180 mm.,

agreeing with his estimate of the length of the London skull. There is, however, a point which must be mentioned. There is a considerable extension of the sphenoidal air sinuses into the basal part of the occipital, and it is possible that there were considerable air sinuses in the frontal bone, a condition which would give the skull in all probability a beetling brow. It has been suggested that the great thickness of the bone may also have been associated with heavy brow ridges; an examination of the Oxford collection suggests that, though this is usually the case, some very thick female skulls have only moderately well-developed brow ridges.

The great breadth of the occipital bone is a remarkable character, as it exceeds 120 mm. Although male skulls do occur with a width of nearly 130 mm., the average female measurement is about 108. The relation of this breadth to the parietal chord from bregma to lambda is about 114, compared with an average of about 96 for English females, but even this figure is not absolutely exceptional. There is in the Oxford collection a female skull from Corfu, with an index of over 115; the skull otherwise shows no remarkable features. The exceptional feature, however, of the Swanscombe skull is its great thickness. I have compared it with a Romano-British female skull in Oxford which is normal and has no pathological conditions, but is slightly smaller. It is impossible to make a comparative estimate of length, as the Oxford skull has so prominent an external inion that the greatest length is Glabella-Inion, but the comparable measurement to Young's estimate would give a length of 175 mm. Now the most remarkable features of the Swanscombe skull are its great thickness; the Oxford skull, in spite of its smaller size, is only slightly thinner. On the parietal bone (I give both measurements under their initial) maximum S. 12 mm., O. 11·2, Lambda S. 11·2–10·7, O. 10·2–9·8, the general thickness of the Oxford skull being just under a millimetre less than the Swanscombe. At the asterion the Swanscombe skull is no less than 3·6 mm. thicker than the Oxford skull, but as this is abraded here the measurement is uncertain; on the other hand other measurements on the occipital bone show that it is definitely considerably thicker than the Oxford specimen. The position of the internal inion in relation to the external differs in the Swanscombe from that of most modern skulls, but it is exactly paralleled in the Oxford specimen. We may say then that, apart from the great thickness of the occipital bone, on the whole the Swanscombe skull does not differ from modern skulls, but that the thickness of the parietal is not remarkable. This is clearly a matter of great interest and importance. We are gradually accumulating evidence which puts

modern man further and further back in history, and we can say that we have now specimens of Lower Palæolithic which are not only modern in form, but which in their general dimensions resemble closely both inhabitants of this country in modern times and, going a little further back, our Romano-British forbears. The beautiful scheme of actual invasions, establishing a new type in association with the establishment of a new culture, will have to be revised and we shall have to consider how far cultural invasions can affect the aboriginal population of a country.[1]

Turning to modern man, interest among anthropologists seems to be very much concentrated on the question of "race," though few give a very exact definition of their use of the term. Dr. Branimir Males in *L'Anthroplogie*, **48,** 1938, 277, discusses the Dinaric Race. Ever since the masterly work of Ripley on the races of Europe the majority of those interested in the ethnology of that Continent have, possibly under different names, believed in three great races. Actually, however, as early as 1595 the difference between some of the dwellers on the Illyrian coast from the group which to-day we call Alpine had attracted attention. Deniker, after a tour in that region in 1880, brought these people into his scheme of European races, and recently, although Deniker's six races are not usually recognised, the Dinaric has been given a place beside Ripley's Nordic, Mediterranean, and Alpine. They differ from the latter, especially in their great stature. Among recent writers Montandon suggests linking them with the Alpine and Czanowski with the Nordic and Armenoid. Czanowski has, in fact, an elaborate scheme whereby he takes certain theoretical basic races, a pure abstraction, and sees in each actual European people a certain proportion of his abstract races. Dr. Males denies that there is any evidence of admixture in the Dinaric race and affirms that they should be given a definite position among the races of Europe.

The persistence of the European racial types when removed from their own home has for long been a matter of interest. It acquired very considerable popularity after the publication of Boas' *Descendants of Immigrants*, to which reference has been made previously in these columns. Briefly Boas believed that the descendants of immigrants into the United States changed their head form and as it were formed a new race. Dr. George Montandon in "L'Ethnie Française au Canada" (*Revue Scientifique*, **76,** 7, 1938) affirms that "race is not merely a matter of environment but in

[1] I have to acknowledge with thanks the generosity of Mr. Marston in lending us at Oxford the original specimens of his most important find, so as to enable us to examine them at leisure.

actual fact, in so far as the matter needs demonstrating, above all a question of heredity." In discussing European types in Canada he is concerned with national types and has worked on photographs of prominent persons which have appeared in the daily press. He finds that French names are associated with the Alpine types, and the Nordic type is especially frequent among those with English names. He maintains that the distinction is clearer than, say, in a European crowd of French and English. The French-Canadians have remained a compact group, keeping their own language and bound together by two cultural features, the Catholic religion and country life. They have large families and tend to increase. Dr. Montandon's articles raise points of great interest, and it would be of considerable importance if someone could obtain the co-operation of Canadians and take the necessary measurements. This should put on one side for ever the somewhat barren controversies about the reliability of Boas' work.

Turning to craniological material, little has been done at present in Canada, but, thanks especially to the energy and enthusiasm of Dr. Hrdlička in Washington and Dr. Hooton at Harvard, there exists a good deal of material for a survey of the racial types of the Indians of the United States, not perhaps as much as could be desired. This material has been surveyed by G. von Bonin and G. M. Morant in "Indian Races in the United States—a Survey of previously published cranial measurements" (*Biometrika*, XXX, 1938). The authors set before themselves the fundamental problem whether the American aborigines are a homogeneous population. They used the coefficient of racial likeness and statistical methods generally. Their monograph may be conveniently divided into three parts, a consideration of the variability of the measurements, the comparison between various Indian groups and finally the relationship of Indian groups with the peoples on the other side of the Pacific. Their material consisted of 1167 male skulls divided into 16 series, the majority of which were statistically adequate, although the number of measurements on each skull was fewer than could be desired. Apart from the people of Pecos Pueblo which definitely represents a more heterogeneous population, the various Indian series all of them show approximately the same variability and are very close to a long series of late dynastic Egyptians examined by Pearson. The variability is rather less than modern series from Western Europe. In view of the fact which we shall see later that the various series differ very much from one another this is a point of considerable interest. It would appear that normal human groups living under primitive conditions tend

to a fairly constant degree of variability. This point may be considered important from two points of view. First of all technically, as the writers point out, it shows that in series whose variability is not known the Egyptian series can safely be used. Secondly, and this is more important, it would seem justifiable to suggest that when we are dealing with a single ancient skull we may reasonably presume that it belongs to a series whose variability probably closely coincided with known variabilities.

Turning to the second problem, the racial classification of the Indians in the United States, there is in a general way, as might be expected, a resemblance between neighbouring peoples, but the Algonkin, of whom there are four series, show remarkable dissimilarity in spite of their close linguistic and cultural relationship. The present evidence does not allow any precise statement of the number of "races"—the authors do not define their use of the term, but presumably apply it in the sense in which it is applied to the "races of Europe"—but on the whole the statistical evidence supports von Eicksted's classification. His *Margide Gruppe* would include the Californian and Florida series, which it is interesting to find are allied. His *Sylvide Gruppe* must be divided into at least two, first the Plains Indians and secondly the Indians of the north-eastern woodlands. The present writer's impression is that it will be found that, as far as Canada is concerned, for which von Bonin and Morant had no data, some of the Algonkin tribes of the east are very closely allied to the Indians of the Plains, whereas others would agree with their findings based on Hrdlička's measurements of Iroquois and their allies south of the Canadian border. The Kentucky group are isolated and have no close allies in the published series. The *Zentralide Gruppe* may be represented by the old Zuni—Pueblo people from the south-west at Santa Fé—and the Basket-makers. There is, on the whole, a marked diversity of Indian types as compared with those normally found among comparable groups in other parts of the world.

Turning to the third problem, the authors conclude that the available material makes it possible to distinguish a few groups of closely allied people among those of Eastern Asia and North America, but the connection between groups and their various affinities must remain undecided. The results of cranial comparisons appear to be in favour of the most generally accepted hypothesis that the present aboriginal inhabitants of America entered that continent by the Behring Strait. It is suggested that the Chuchcki of Eastern Siberia, who show little affinity with any of the present inhabitants of Eastern Asia, represent people who

fell by the way in the course of the migration. The Arikara, who are related to the Sioux, show a relationship with the western Eskimo, and this may be an indication of former contact between the two peoples. Statistical evidence suggests, without affirming, a link between the Indians of California and some of the peoples of Eastern Asia, but it is suggested that this is rather an indication of the line of migration than of any trans-Pacific migration. Finally, there are no characters for which there are satisfactory series of measurements which will distinguish all the North American Indians from their relations in Asia. They tend on the whole to have broader and higher faces. It is this great breadth of face that gives the living Indian his characteristic facial form, but even here there are some people on the Asiatic continent whose faces have this enormous breadth, though in the living not that angularity which characterises many, but not all, American Indians.

A similar problem in regard to racial homogeneity is discussed by Howells and Warner in "The Anthropometry of the Natives of Arnhem Land and the Australian Race Problem," *Peabody Museum Papers*, XVI, 1937. A comparison of the figures obtained by Warner with published material suggests that the Australian aborigines are really a homogeneous group, the difference between widely separated groups being on the whole not greater than between those in close geographical proximity. In the north the stature is slightly higher. The breadth of the head is lower in the south, where extreme dolichocephaly is met with ; the head height is also lower in the south. The differences which exist the authors consider to be due to geographical isolation.

NOTES

Magnetic Viscosity (L. F. B.)

Ewing [1] and Rayleigh showed that when long wires of soft iron are placed inside a solenoid in which a current is either gradually or suddenly changed there occurs a distinct, slow change of induction, as manifested by a magnetometer deflection, for some time after the current has reached a steady value, especially when weak magnetising fields are used. Hence, it may happen that ballistic measurements of magnetic induction give results slightly lower than static measurements ; the difference, which does not normally exceed 1 or 2 per cent., is attributed to *magnetic viscosity*. Hence, when a ferromagnetic is exposed to an alternating magnetic field, losses due to hysteresis, eddy currents and magnetic viscosity must arise, and, on account of their importance in telephony, the latter have recently been studied in detail.

It has been found that carbonyl iron supplied by Heräus, Hanau, mainly in the form of tape $0{\cdot}1 \times 15$ mm² in cross-section, shows extraordinary magnetic viscosity when recrystallised by heating for 2 hours in a stream of commercial hydrogen at 1000° C. Richter [2] showed by magnetometer and by ballistic measurements that magnetic changes at room temperature continue for some 100 seconds following the removal of a magnetic field, but are practically ended within a few minutes. In one set of experiments a field + H acting on a specimen was suddenly changed to − H and so maintained for an interval of time t', after which the field was switched off. As t' approached zero the viscosity effects became more and more like those obtained merely by cutting off the field + H, showing that magnetic viscosity and ordinary hysteresis effects are not closely related. A series of curves obtained by plotting the difference between the residual induction measured at a time t after switching off a magnetic field and the static value of the residual induction against $\log t$ for a range of fixed tempera-

[1] A. Ewing, *Magnetic Induction in Iron and Other Metals*, 3rd edition, p. 127.

[2] G. Richter, *Ann. der Phys.*, **29**, 605, 1937.

tures show displacements parallel to the time axis, the viscosity effects decreasing rapidly with increase in temperature.

These results were confirmed by Schulze [1] by a power-loss method. In this case the material is used as a spiral core or ring upon which an inductance coil of resistance R is wound. The latter is placed in the arm AB of a Maxwell inductance-capacity bridge ABCD, the arms BC and DA containing fixed resistances R_1 and R_2 and the arm CD an adjustable capacity K shunted by an adjustable resistance. When the bridge is supplied with steady current and a thermal galvanometer is connected across BD the condition for balance is $R = R_1.R_2/W_0$, where W_0 is the value of the shunt resistance. When alternating current is supplied the conditions for balance are $R + r = R_1 \cdot R_2/W$ and $L = R_1.R_2.K$, W being the new value of the shunt resistance, r an apparent resistance introduced into the arm AB by power losses in the core, and L the inductance of the coil under the experimental conditions. L is, of course, a function of the current and may be written $L = L_0(1 + \beta.H)$ since the current is small, where L_0 would be the inductance with the core replaced by air, H the field amplitude in the core and β a constant. Following Jordan [2] we write

$$r/f.L = r_n/L + r_h.H/L + r_e.f/L$$

The first term on the right side of the equation represents the effect of magnetic viscosity with an alternating field of frequency f, the second and third terms those of ordinary hysteresis and eddy currents respectively. If now we plot $r/f.L$ against f for two fields of amplitude $H = H_1$ and $H = 2H_1$ we get two graphs from which we can obtain the graph for $H = 0$, since the distance apart of the two curves is $r_h.H_1/L$. If this extrapolated curve is linear then its slope measures r_e/L, and $r_n/f.L$ is independent of the frequency. If it is not linear then $r_n/f.L$ is a function of the frequency which can be obtained by measuring the distance apart of the extrapolated curve and the line $r_e f/L$ against f which can be drawn on the basis of a relation given by Wolman.[3]

Schulze found that at temperatures below 20° C. the magnetic viscosity of the *virgin* material was constant over a frequency range of 60 to 5000 cycles per sec. It is believed that this frequency-independent or Jordan component of the magnetic viscosity persists at all temperatures. At higher temperatures, however, the curves

[1] H. Schulze, *Wissen. Veröff. aus den Siemens-Werken*, **17**, 151, 1938.
[2] H. Jordan, *Elektr. Nachr.-Techn.*, **1**, 7, 1924; *Ann. der Phys.*, **21**, 405, 1934.
[3] W. Wolman, *Zeit. für techn. Phys.*, **10**, 595, 1929.

of r_n/L against T for fixed values of f have the form of resonance curves, with a maximum value of r_n/L at a temperature $T_r{}^\circ$ K, given by

$$\log 2\pi f = \theta\left(\frac{1}{T_\theta} - \frac{1}{T_r}\right) = 34{\cdot}7 - \frac{10600}{T_r},$$

It is found that magnetic viscosity and associated high permeability which varies with f appears only when the carbonyl iron has been annealed or recrystallised at 1000° ; the phenomenon is suppressed when the material is deformed by tension. The effects can be explained on the assumption that a pure and well-annealed specimen consists of parallel threadlike Weiss domains magnetised in parallel and anti-parallel directions only. As the threads increase in length, the energy associated with the demagnetisation effects decreases and that associated with the surfaces of separation between the threads increases, so that at each temperature there must be a stable state with a definite degree of thread formation.

Annual Report of the South African Institute for Medical Research 1936 (P. J.)

Dr. Ordman has made a continuous study of pneumonia and allied respiratory diseases in local mine natives, and has prepared a pneumonia vaccine for prophylactic purposes comprising the prevailing bacterial flora on the Witwatersrand. The bacterial divergences in the various communities were not sufficiently marked to warrant the issue of special and separate vaccines.

The use of prophylactic pneumonia vaccine is increasing in the mines and a number of satisfactory results are recorded. Large-scale experiments in City Deep and Crown mines, and also in Geldenhuis Deep, are in progress, and inoculations will be continued for some years in order to obtain further evidence on the value of this prophylactic measure.

An outbreak on the East Rand Proprietary Mine, the native employees of which had not been inoculated against pneumonia, is being investigated, and details will be given in a forthcoming publication. Towards the end of April the incidence of pneumonia and the number of deaths began to increase ; the peak being reached in July, and the outbreak was at an end in October. As a result of bacteriological findings a suitable prophylactic vaccine was supplied to the mine for the inoculation of all future native recruits.

The majority of the pneumonics in this epidemic were typical lobar variety associated with pneumococcus of Groups " C " and " B " (Lister Types I and II) ; these types are represented in the

present prophylactic vaccine, and were prominent in preinoculation days of the Witwatersrand Mines, so that such an outbreak would not have occurred in a vaccine-protected community.

A number of Bushmen in Johannesburg who were suffering from minor respiratory maladies was investigated, and it was recommended as a safeguard that these Bushmen be inoculated with the Pneumonia (Compound) Vaccine in general use.

The question of streptococcus types occurring locally has been considered. A number of strains of hœmolytic streptococcus isolated from various human sources have been typed and all shown to belong to Lancefield's Group "A." Closer analysis of the intra-group classification of the organisms on the lines of Griffith's method is proceeding. There may be value in this work from the point of view of vaccine for prophylaxis of respiratory diseases; one or more specific strains of hœmolytic streptococcus may be of particular importance in local cases of acute respiratory diseases; if so, only these need be included in the vaccine.

In typing strains of meningococcus the simple agglutination test has been substituted for the more laborious agglutinat-absorption test formerly employed, which in recent years has not been found to give the more clear-cut results which were at one time obtained. The meningococcus is now classified into Groups 1 and 2 by means of 2 group sera; Group 1 serum representing antigens of Gordon's types I and III, whilst group 2 serum is polyvalent, representing a number of strains not conforming antigenically to Gordon's types I and III. The inclusion under Group 2 of many strains is arbitrary and unsatisfactory as the Group 2 serum is never sufficiently polyvalent to cover more than a small proportion of the numerous "aberrent" strains met with; in the present state of knowledge it is the most practicable expedient available.

The relation of meningococcus strains to therapeutic serum production is of importance to the Institute Serum Department, and much careful study has been undertaken in collaboration with that department in the selection of suitable organisms to provide the most adequate antigen for the preparation of an antigenically comprehensive polyvalent antiserum in horses. The efforts have been justified, the anti-meningococcus serum produced is satisfactory in its agglutinating power against meningococcus strain isolated from current cases of disease; the case mortality rate in cerebrospinal meningitis locally has shown a considerable drop.

Plague Investigation.—Dr. Pirie reports an apparently steady diminution in the virulence of the strains of *B. pestis* obtained from both human and animal sources. All positive results are

originally obtained either by guinea-pig inoculation or by culture from material received; these are checked by guinea-pig scarification; formerly these usually died in from 5 to 7 days, now they live 10 or 12 days before succumbing to the disease or not infrequently survive and recover after development of a bubo but without further spread of the disease.

Experimentally it is becoming more difficult to obtain a local strain which can be relied upon to kill rats regularly in three days by subcutaneous inoculation of a moderate-sized dose. There is no explanation of this drop in virulence. Two years ago it was possible to maintain a strain at its original virulence by continuous passage through rats, but not to increase its virulence by passage. Now such a plan does not even maintain virulence, and there is a slow but distinct loss in the course of a year's passage.

Experiments have been started using a living nonvirulent strain of *B. pestis* obtained from Madagascar. Colonel Girard has used this as a preventive vaccine with success. It was found experimentally to give better protection in rats than a killed vaccine. It has been used for the preparation of an antiserum, and so far as present experimental results go, the indications are that a more potent serum is thus obtainable than by the use of killed vaccine for immunisation.

Dr. Mavrogordato has continued the study of dust from the lungs of those exposed to underground air-borne dust of the Witwatersrand goldfields. Cases were of 3 types: early clinical silicosis, fibrosis of silicotic type found in lungs after autopsy, and, no evidence of silicosis either during life or after autopsy. Experience shows that over a considerable number of cases there is a relation between the mass of dust recovered and the amount of damage to the lungs.

For estimation of dust concentration, stress was laid upon the number, size, and size frequency of the dust particles. The results so far show that 90 per cent. of the mass of dust is composed of particles of 4 microns or less, and "a main mass composed of particles of 2 microns." This applies to samples of air-borne dust from underground and to dust recovered from lungs.

In 1924 the average dust returns were about 1 mg. per cubic metre, and have remained at about that level. In 1909 it was of the order of 20 mg. per cubic metre and has been falling steadily since. Each year a certain number of miners are found to be suffering from silicosis, and this is the expression of dust to which they have been exposed throughout their underground service. The average duration of exposure necessary to produce a clinical

silicosis has increased from 9½ years to about 16½ years as the average mass of dust has fallen. Since exposures have fallen to the order of 1 mg. per cubic metre, only two cases of clinical silicosis have been detected in miners who began work in 1924 or later, while among miners who have only worked on scheduled mines and who passed a physical examination before being accepted for underground work, the average duration of exposure necessary to produce a clinical silicosis is about 18 years. This initial examination was introduced in 1916 when the average concentration of dust was 3·9 mg. per cubic metre.

The "average time" needed to set up a clinical silicosis is about 2 to 2½ times as great as the time required for the most susceptible subject. With dust at 1 mg. the most susceptible subjects have been requiring 10 years' exposure, so the average exposure should be of the order of 20 to 25 years.

Present experience suggests that if there were only ultra-small particles in their present concentration in the underground air, there would be very little silicosis, it would require a long period to develop and would be almost confined to particularly susceptible subjects. With the increased duration of invasion necessary to produce a clinical silicosis, the excess tuberculosis rate is becoming less marked on the Witwatersrand.

The total incidence of tuberculosis, including tuberculo-silicosis, for 1929–32 was 1·97 per 1000, and for the 1929–35 was 1·47 per 1000 (Europeans). In the period 1917–20 the tuberculosis incidence (all forms) was 8·7 per 1000, and at this time the expectation of life after the notification of secondary silicosis was about 2 years while it is now about 6 years.

Annual Report of the Institute for Medical Research, Federated Malay States (Kuala Lumpur), 1936 (P. J.)

The activities of the many divisions of this institution have continued throughout the year, and a good general survey is supplied by Dr. Lewthwaite.

Before 1916 diphtheria was a rare disease in the Malay States. From 1916 to 1926 an average of only 26 cases per annum were diagnosed bacteriologically, but now from 100 to 150 cases are annually diagnosed. The local strains isolated from these cases showed that although 62 per cent. are virulent they belong either to the "Mitis" or "intermediate" variants; none of the "Gravis" variety, said to be responsible for the high death rates in Europe, was met with. Schick tests showed that the indigenous population possesses a degree of immunity to local strain. Of 762 dead bodies

(including children and infants) investigated during the past three years, 41 (= 5.4 per cent.), yielded throat swabs positive of *C. Diphtheriæ*. Although the annual cases have risen to 110–150 and the dead body rate is 5·4 per cent., it is not possible to say that diphtheria is increasing in prevalence in F.M.S.—but there is an approach to more accurate conception of the incidence of the disease.

Dysentery.—The main type of dysentery in hospital patients is *B. Dysenteriæ Flexner*; *B. Dysenteriæ Shiga* occurs in small isolated epidemics. One case of Sonne dysentery occurred in 1934 and in 1935, but during the present year 9 cases of Sonne dysentery of mild type were investigated; a fourth variety, *B. Dysenteriæ Alkalescens*, occurring locally, appeared for first time during 1936.

Enteric Fevers.—About twice as many cases of enteric fever are now being diagnosed by bacteriological or serological means as compared with ten years ago. This is in part due to improvement in serological technique and especially to the use of culture from Widal blood clot, which remains after the serum has been separated for the Widal reaction. This method often confirms a positive Widal reaction, and often enables a positive diagnosis to be made some days prior to agglutinins developing in the patients' serum, and permits a positive diagnosis among that proportion of enteric fever cases when for some unexplained reason the Widal reaction remains below diagnostic level, or is negative. Details are given of 62 cases of enteric fever diagnosed by culture of Widal blood clot. Had this additional diagnostic method not been employed no positive laboratory diagnosis could have been given, and it is probable that many of the 62 cases would have appeared in the annual returns as "fevers of unknown origin." Among 486 cases clot cultures were positive in 155 or 32 per cent.; and among these 155 cases the Widal reaction was also positive in 93 or 19 per cent. (of 486 cases). The clot culture method may be said to have not only a confirmatory value—as regards the Widal reaction—of 19 per cent.; but what is more important a diagnostic value entirely of its own of 13 per cent.; if culture methods had not been employed 13 out of every 100 cases of enteric fever would have escaped diagnosis.

On behalf of the National Commission of the League of Nations a number of experiments were designed to afford data regarding the efficacy and safety of atebrin and quinine administered as clinical prophylactics to malarious populations. Three plantations, T. (a tea estate), Y. and U.C. (rubber estates), were chosen; 1733 individuals were observed for periods up to 12 months and 1,174 malarial attacks were treated.

On estate T., there was a highly malarious population of about 500 Indian labourers and independents in one set of lines, within a short distance of which was free breeding of *A. umbrosus*; the area was isolated from outside sources of infection by a jungle barrier several miles deep, peopled only by a small wandering tribe of aborigines.

The entire labouring population was divided into three groups, believed to be comparable in all respects. The first group was given prophylactic atebrin 0·2 grams. on successive days twice weekly; the second group 0·4 grams. of quinine bihydrochloride in tablet form, daily; the third group (control) received a yellow tablet known to be inert. During the period (Sept. 2, 1935, to Aug. 26, 1936) the number of malarial attacks were 45 in group 1, 135 in group 2, and 475 in control untreated group.

There were 8 deaths during the 12 months—4 in group 1, 2 in group 2, and 2 among the controls. Administration of atebrin had no obvious ill effect on the pregnant women—most abortions occurred among controls and were associated with attacks of malaria; there was an increase of live births due to the suppression of malarial attacks.

Prophylactic administration of atebrin was suspended for 8 weeks, during which period there occurred 200 malarial attacks, many clinically severe, and associated with high parasitic counts in the two protected groups; an incidence nearly twice that of the unprotected control during the same period.

In estate Y., an old-established rubber estate in Selangor, a total of 555 individuals were observed up to 14 months, the population being divided into three groups as in the case of estate T.; but results were not satisfactory and the statistical findings are not detailed. As on the T. estate there was a rapid reappearance of malaria within a few weeks of the prophylactic drugs being withdrawn. The U.C. estate is a highly malarious rubber estate—the labour force is Tamil and the malarial vector *A. maculatus*.

In January 1936 the trophozoite rate was 50 per cent., and the monthly malarial attack rate about 25 per cent. It was hoped it might be possible on this estate to collect data as to the smallest prophylactic dosage of atebrin necessary to control malaria under conditions of fairly high endemicity.

The population was divided into two groups. The first group was given prophylactic atebrin 0·2 grams, once a week, the second group receiving an inert tabloid. The incidence of malaria among adults and children during the year was 214, and the treated group 48.

Atebrin is an expensive drug and optimum dose for prophylaxis is an important question. The cost of 1000 tablets is over $50. The content per tablet is not given; but 0·2 grams. twice a week makes the cost per adult $10 per annum.

Miscellanea

The honours list published on the occasion of H.M. the King's birthday included the following names:—*Order of Merit*: Sir Arthur Eddington. *Baron*: Sir Josiah Stamp. *K.B.E.*: Prof. T. H. Easterfield, formerly director of the Cawthron Institute of Scientific Research, New Zealand. *Knight Bachelor*: Mr. W. G. Ball, dean of the Medical College, St. Bartholomew's Hospital. *C.B.*: Dr. J. J. Fox, Government Chemist. *C.M.G.*: Prof. A. E. V. Richardson, deputy chief executive officer, Council for Scientific and Industrial Research, Commonwealth of Australia. *C.B.E.*: Mr. H. Bishop, assistant chief engineer, British Broadcasting Corporation; Prof. B. Melvill Jones, Francis Mond professor of aeronautical engineering, University of Cambridge; Mr. E. J. Wayland, director of geological survey, Uganda; Mr. E. B. Wedmore, director, British Electrical and Allied Industries Research Association. *O.B.E.*: Dr. S. E. Chandler, principal of the Plant and Animal Products Department, Imperial Institute; Mr. R. O'F. Oakley, principal, Department of Scientific and Industrial Research; Mr. D. Robinson, superintending examiner, Patent Office; Mr. B. S. Smith, superintending scientist, Anti-Submarine Establishment, Portland.

The Right Hon. Neville Chamberlain has been elected fellow of the Royal Society under the Statute which permits the election of persons whose election would be of signal benefit to the Society.

Prof. J. J. Abel, lately professor of pharmacology at the John Hopkins Medical School, Baltimore, and Prof. N. E. Norland, since 1923 director of the Danish Geodetic Survey, have been elected foreign members of the Royal Society.

Sir William Bragg, P.R.S., has been elected foreign associate of the Paris Academy of Sciences.

Prof. A. Fowler, emeritus professor of astrophysics in the University of London, and Prof. D. M. S. Watson, Jodrell professor of zoology and comparative anatomy, University College, London, have been elected foreign members of the U.S. National Academy of Sciences.

Sir Thomas Lewis, physician-in-charge of the Department of Clinical Research, University College Hospital, and Prof. G. I.

Taylor, research professor of the Royal Society, have been elected to be honorary fellows of the Royal Society of Edinburgh.

Sir Frank Smith, secretary of the Department of Scientific and Industrial Research, has been elected an honorary fellow of the Institute of Physics.

Prof. R. H. Fowler has found it necessary for reasons of health to renounce his recent appointment as director of the National Physical Laboratory and has been re-elected Plummer professor of mathematical physics in the University of Cambridge.

Sir Thomas Middleton has been appointed to succeed the Right Hon. Lord Richard Cavendish as chairman of the Agricultural Research Council.

Mr. H. J. Gough, superintendent of the Engineering Department of the National Physical Laboratory, has been appointed director of scientific research at the War Office.

Prof. Irvine Masson, head of the Department of Pure Science in the University of Durham, has been elected vice-chancellor of the University of Sheffield.

Prof. M. N. Saha has been appointed to be Palit professor of physics in the University of Calcutta in succession to Sir C. V. Raman (1918–32) and Prof. D. M. Bose.

Sir Gilbert Morgan is retiring from the directorship of the Chemical Laboratory of the Department of Scientific and Industrial Research at Teddington under the age limit.

Prof. J. E. Littlewood of Cambridge University has been awarded the de Morgan medal of the London Mathematical Society. The Melchett medal of the Institute of Fuel has been awarded to Prof. R. V. Wheeler, director of the Safety in Mines Research Board Experimental Stations at Sheffield and Buxton.

Dr. A. P. M. Fleming has been elected president of the Institution of Electrical Engineers for the session 1938–39.

We have noted with great regret the announcements of the death of the following well-known scientific men during the past quarter: Prof. J. J. Abel, For. Mem.R.S., pharmacologist; Prof. W. A. Bone, F.R.S., lately professor of chemical technology in the Imperial College of Science; Prof. E. W. Brown, F.R.S., of Yale University, mathematician; Mr. B. D. Burtt, botanist, Tanganyika Territory; Dr. W. W. Campbell, For. Mem.R.S., of the Lick Observatory; Dr. Eagle Clark, ornithologist; Mr. M. J. R. Dunstan, agriculturalist; Dr. A. Galt, keeper of the Technological Department, Royal Scottish Museum, Edinburgh; Dr. E. Goulding, lately of the Imperial Institute; Lt.-Col. C. H. H. Harold, water engineer; Dr. G. E. Horváth, of Budapest, zoologist; Dr. Ch. E. Guillaume,

formerly director of the Bureau International des Poids et Mesures; Sir Colin Mackenzie, anatomist; Dr. J. W. Mellor, F.R.S., chemist; Mr. W. M. Mordey, electrical engineer; Mr. E. M. Nelson, microscopist; Mr. H. Ramage, spectroscopist; Prof. W. Stroud, chairman of Barr & Stroud, Ltd.; Mr. C. F. M. Swynnerton, C.M.G., director of Tsetse Research, Tanganyika; Mr. H. N. Thompson, C.M.G., lately director of Forests, Nigeria; Dr. A. E. H. Tutton, F.R.S., crystallographer.

The eighteenth session of the International Geological Congress will be held in London from July 31 to August 8, 1940. It is the first meeting of the Congress to be held in Great Britain since 1888. The necessary financial support for the proper organisation of the Congress has been secured and permission has been obtained to establish the office and headquarters of the Congress in the building of the Geological Survey and Museum, South Kensington, London, S.W.7, where all communications should be addressed. The general secretaries are Dr. W. F. P. McLintock, deputy director of the Geological Survey of Great Britain, and Prof. W. B. R. King, University College, London.

Lord Nuffield has given the £60,000 required by the University of Birmingham for additional buildings and equipment in the Physics Department.

Mr. P. L. Harrison has lent his collection of old Japanese clocks for exhibition at the Science Museum, South Kensington. The clocks are of great interest as they were constructed to indicate time on the old Japanese system, in which the periods of daylight and darkness were each divided into six hour-intervals. On this system, which was in use throughout Japan until the second half of the nineteenth century, the length of a day "hour" was therefore different in general from that of a night "hour" and both varied according to the season of the year. In the oldest and largest of the clocks lent to the Museum there are two separate time-keeping elements, one being in use during the day and the other at night, the change-over from one to the other at dawn and again at dusk being automatically carried out by the clock itself. In some of the other clocks the time-keeping mechanism goes at the same rate both by day and by night but the actual hour numerals of the dial are adjustable by hand; in summer, for instance, with its long days and short nights, the night "hours" would be set to form a close group while the day "hours" are widely spaced.

Memoir No. 7 from the Imperial College of Tropical Agriculture deals with the storage and transport of tropical fruits and vegetables. C. W. Wardlaw, the author, gives a useful summary of what is already known regarding the subject, classified under the diverse products, and each is accompanied by bibliographical references. Some idea of the comprehensive character of the treatment can be gathered from the fact that more than 150 products are dealt with in alphabetical order. Success involves not merely the correct temperature, humidity and duration of the storage, but the requisite rapidity of transport from the grower, the precise time and mode of harvesting, as well as the choice of appropriate varieties and the proper conditions of growth. It is claimed that the absence of suitable shipping is the chief obstacle to the wider utilisation of tropical produce. Both the scientist and those concerned with the practical aspects of the subject will find this a valuable summary, which can be obtained from the Editor of *Tropical Agriculture,* Imperial College of Tropical Agriculture, Trinidad (pp. 224, 4*s*. post free).

The Official Directory of the British Chemical Plant Manufacturers Association for 1938 gives an alphabetical list of members and the products which they manufacture. It should prove invaluable to any firm or person who is considering installing chemical plant of any description. The Directory is issued gratis to those interested in the purchase of chemical plant, and may be obtained from the Secretary, British Chemical Plant Manufacturers Association, 166 Piccadilly, London, W.1.

The *Report of the Building Research Board* for the year 1937 (H.M. Stationery Office, 3*s*. 6*d*. net) contains a few points of general interest. An investigation of the efficiency of heating systems has shown that the best position for hot-water radiators is the conventional one beneath a window since in this position the heating is much more uniform and the floor draught is reduced to a minimum. Floor heating is on the whole the most efficient, while there is little to choose between ceiling heating and ordinary radiator heating. The method of cleansing the stone work on buildings by light brushing after the dirt has been softened by applying to it a fine misty spray of water for a suitable time which was described in a previous *Report* has now been used successfully on three buildings in London. The method has the advantages that no harmful chemicals are employed and hard scrubbing, which may damage delicate carving, is avoided.

The Report states that rendered finishes on the outside of build-

ings are usually less successful in England than on the Continent. This appears to be due to two causes. Firstly, on the Continent the rendering is " thrown on " and not " laid on " according to the plastering technique employed in this country. The result is better adhesion and uniformity of texture with less risk of crazing. Secondly, the mortar employed—a mixture of cement, lime and sand—is dry mixed in the factory, only the water being added on the building site. Over here the materials are mixed on the site and the control of the final product is obviously much less effective.

The *Journal of Research* of the U.S. National Bureau of Standards for June 1938 contains a preliminary account of an investigation by N. E. Dorsey on the supercooling and freezing of water. The results are very surprising for " it is evident that neither the length of time that a specimen (of water) is held at a low temperature nor the recently past thermal history of the water within the range from room temperature to the specimen's spontaneous freezing-point is of any prime importance in the freezing of these specimens. Also, in an attempt to supercool water it is unnecessary either to limit one's self to excessively small volumes of water or to protect the water carefully from mechanical disturbances, for without such protection and with volumes as large as 8 cm.^{3} supercooling to $-21°$ has been observed."

Preliminary heating of the water lowers the spontaneous-freezing-point, the lowering being greater the higher the temperature to which the water is heated and the longer it is maintained at that high temperature. Thus a sample which repeatedly froze at $-6{\cdot}2°$ C. froze at *c.* $-7°$ C. after being maintained at 54° C. for 8 hours and at *c.* $-14°$ C. after being kept at 97° C. for nearly 10 hours. The lowest spontaneous-freezing-point, namely $-21°$ C., was obtained with water described as " vacuum distilled from chromic solution." The highest, $-3{\cdot}3°$ C., was obtained with water from the bottom of an alga-covered pool.

The water was frozen in half-filled and sealed cylindrical glass bulbs, about 2 cm. in diameter and 4 cm. long, suspended in alcohol which was cooled as desired. The explanation of the various phenomena which were observed remained uncertain but it was considered that the size of motes in the water may be the significant variable. There was no indication that the nature of the glass used for the bulbs is of any consequence.

The *Report of the National Physical Laboratory* for the year 1937 gains interest from the fact that it is the only one which will bear

the signature of W. L. Bragg. It contains, as usual, an extensive record of routine and research work successfully accomplished, and it is possible here to refer only to a very few of the matters with which it deals. The Physics Department has been largely concerned with various thermal measurements, with radiology and with acoustical investigations. The conductivity of various kinds of mica have been determined by the divided bar method. The conductivities of light concretes have been found to vary from 1·1 to 2·8 B.Th.U. per sq. ft. per hour for 1 in. thickness as against 7 for gravel concretes. Heat leakage through multiple windows has been investigated and the accuracy with which temperatures on the International Scale can be determined with the standard platinum platinum-rhodium within the range 660° C. to 1063° C. has been increased considerably. The radiology department has tested 25 gm. of radium—an increase of 25 per cent. on the previous record figure—and in the Optics section a careful comparison has been made between the Laboratory scale of radiation intensity and the scales established at other laboratories.

The Electricity Department has investigated methods for preventing sparks due to static electricity in dry-cleaning processes and in hospital operating theatres where they may ignite inflammable vapours. Preparation has been made for testing the measuring apparatus used by electricity supply authorities in accordance with the Electricity Supply (Meters) Act, 1936, and new substandards of luminous intensity were prepared for use in the Laboratory when the new unit comes into use on January 1, 1940. This is to be known as the " new candle " and is one-sixtieth part of the luminous intensity of 1 square centimetre of a black body at the temperature of solidification of platinum (2046° K.).

The Metrology Department completed the determination of the acceleration due to gravity at Teddington, the provisional result being 981·180 cm./sec.^{2} to within about $\pm$ 2 parts in a million. The Radio Department has been largely concerned with direction finding. Measurements of the phase velocity of electromagnetic waves along the ground gave a mean value of $2{\cdot}95 \times 10^{10}$ cm./sec. for frequencies ranging from 2·5 to 15 Mc. per sec. with no systematic variation with frequency. The results did not support measurements made in America, which indicated that the phase velocity along the ground for this frequency band is considerably less than the velocity of light in free space.

The eleventh *Annual Report* of the Council for Scientific and Industrial Research of the Commonwealth of Australia summarises

the very great amount of useful work which was carried on under the direction of the council during the year ending June 30, 1937. Most of this work was designed to assist the agricultural industry; but early in 1937 a Committee appointed to consider what could best be done for other industries completed its report and the matter is now being considered by the Government. It is proposed to establish a Standards Laboratory, to extend the technical information service, and to carry out research which cannot well be handled by individual firms.

The investigations dealt with in the 1937 *Report* are arranged under the usual headings: Plants, Entomology, Weeds, Animal Health and Nutrition, Soil, Irrigation, Forest Products, Food Preservation, Fisheries and Miscellaneous, including the work of the Radio Research Board.

Work on the myxomatosis of rabbits has shown that all domesticated and wild animals (including birds and the wild hare) suffer no ill effects from the virus. Investigation of timber suitable for bending has shown that red tulip oak is excellent, that brown mallet is nearly as good but that karri can be used only for mild bends. The benefits accruing from the destruction of the prickly pear by the *Cactoblastis* are being realised. Large tracts of country are being improved for wool production, and towns and settlements in the pear country reflect the changed conditions by their renewed prosperity. The cost to the Commonwealth remained quite moderate: the total expenditure for the financial year under review was less than £200,000 and of this amount nearly £60,000 was subscribed by interested bodies for specific purposes.

ESSAY REVIEW

MECHANICAL THOUGHT. By W. R. THOMPSON, Ph.D., D.Sc., F.R.S., Assistant Director, The Imperial Institute of Entomology. Being a Review of **The Axiomatic Method in Biology**, by J. H. WOODGER, D.Sc. [Pp. x + 174.] (Cambridge: at the University Press, 1937. 12*s*. 6*d*. net.)

ONE of the most fundamental characteristics of modern science is the tendency to mathematical formulation. The ultimate aim of all science, wrote the great physiologist, Sir William Bayliss,[1] is to express in a mathematical form the discoveries that have been made. Many other leaders of science have said the same thing; and there are a good many clear indications that those branches of scientific work in which mathematical treatment is difficult, or has not become popular, tend now to recede into the background, like poor relations at a family gathering. One reason for this is the tremendous power of the mathematical method as an intrument of exact prediction. Man's marvellous conquest of material nature is in great measure due to it.

But the mathematical trend or mood corresponds also to a deep-seated and indeed *specific* element in the great modern philosophical movement that originated with Descartes. "Those long chains of simple and easy arguments that the geometricians habitually use to work out their most difficult proofs," writes Descartes, in the *Discours de la Méthode*, "gave me the idea that all things that can be known by man can be linked up in the same way"; "among all those who have hitherto sought for truth in the sciences, only the mathematicians have been able to give demonstrations, that is to say, certain and evident reasons." Even the religious apologetic of Pascal is affected by his mathematical bias and by his view[2] that though everyone seeks truth, and "the logicians profess to lead us to it, the geometricians alone attain it; and outside their science and what imitates it, there are no veritable demonstrations."

The idea that is here struggling for expression emerged full-

[1] Introduction to W. M. Feldman's "Biomathematics," 1923.

[2] Pensees, *De l'Art de Persuader*.

fledged in the philosophy of Leibniz. "My metaphysics," says Leibniz, "is entirely mathematical." Though "there are some who think that mathematical rigour is impossible outside of the so-called mathematical sciences," this is merely because "they do not realise that discussions of the mathematical type and what the logicians call formal reasoning are the same thing." From these conceptions to the belief that the symbolism used in the mathematical discourse has a general validity, and that it is possible to construct a *universal algebra*, to which "everything that an angelic reason can discover and demonstrate is accessible," is but a little step. Leibniz, like Van Holten, dreamed of a calculating machine which would be arranged so as to give all the fundamental relations between given terms and thus enunciate all possible theorems: like the thirteenth-century precursor Ramon Lull,—the Balearic "Doctor Illuminatus"—with his "Ars Generalis Ultima," or machine for theological demonstrations, built for the conversion of the Saracens.

The Axiomatic Method in Biology, by Dr. J. H. Woodger, is an attempt to apply the system of symbolic logic—now known as logistic—to the biological sciences. Modern logistics, as elaborated by Whitehead and Russell, is, according to Dr. Woodger, not so much an extension of pure mathematical methods to the field of logic, as a demonstration that mathematics can be deductively developed from logistic. But the essence of the matter consists in the treatment of biological material by deductive reasoning working on a system of symbols; and to describe it as an application of mathematical methods to biology seems accurate enough for practical purposes.

When we consider the "chronic controversies" of biology, says Dr. Woodger, we find that these are largely "traceable either to failure to eliminate metaphysical elements from biological topics, or to difficulties created by the shortcomings of current biological language." "Both of these difficulties," he feels, "can be avoided by paying attention to the requirements of an ideal scientific language. Because if we have a perfect language, we need not dispute, we need only calculate and experiment." *Words* have emotional and other connotations which deflect reasoning, often in a very subtle manner. Let us then substitute *symbols* for words. Once the laws of manipulations of the symbol-system are established it must inevitably lead to the correct conclusions, as does the symbol-system of mathematics. As Bertrand Russell put it, "a good notation has a subtlety and suggestiveness which at times make it seem almost like a live teacher . . . and a perfect notation would be a substitute for thought."

In any branch of natural science, says Dr. Woodger, we can distinguish the objective reference or subject matter and the language in which this is recorded and communicated. Work on the subject matter is *practical*: work on the language of science is *theoretical.* Practical work leads first to *observation-sentences*, next to *empirical generalisations*, and, finally, to the construction of sentences which have groups of the generalisations *as their logical consequences.* A Science is an *exact science* when its *language* has attained such a degree of perfection that its *syntax* is completely known, *i.e.* when we know and can formulate precisely the rules of the constructions and sequence of sentences in it. Such a language is a *calculus or axiom-system.*

The word axiom is not used here to designate a self-evident proposition. An axiom-system consists of *signs* and *expressions constructed from the signs*, in accordance with the syntactical rules of the system, which rules do not themselves belong to it. The axiom-system may be an *abstract* or *uninterpreted* system in which the subject-matter symbols are called empirical contrasts and have a definite and objective significance. In an *uninterpreted* system, a theorem is *true*, if it can be validly deduced from the axioms which, though they are constructed according to the rules of the system, have no objective reference. In an interpreted system the theorems are true *because* either *directly verifiable in reality*, or the *source* of *deductive arguments whose consequences are verifiable.* In addition to the subject-matter signs, the system also contains *logical constants*, which are symbols designing words connecting prepositions (such as " and," " or," " implies "), indicating values of variables in relating to some functions (as " all," " some," " none ") as well as such signs as the $=$, $\times$, and the bracket signs of mathematics.

The main objects of Dr. Woodger's book are to provide an introduction to logistic method and sketch the outlines of an axiom-system suitable for biology. It was not to be expected that anything startling would emerge from this preliminary essay. It does, however, give an opportunity to compare the axiomatic method with the ordinary procedure.

The exponents of logistics claim that for the treatment of scientific data in *general*, ordinary language can advantageously be replaced by a system of signs. Now language is itself a system of signs. The essential character of a sign is *reference to something other than itself.* Words, which are the component signs of language, refer to things, but not directly—merely indirectly, through the medium of *concepts.* Concepts, therefore, are also signs. But between the

two kinds of signs there is a profound difference. The concept is what the medievals called a *formal sign* whose *entire entity*, so to speak, *consists in its reference to the thing signified*; for the concept is simply *that in which we know the thing*. The word, however, is a *material sign* and it has, in addition to its *referential entity* (so to speak), an independent entity of its own, as a sound, which may be represented by a written word, which is a sign of the same character, but one remove farther distant from the thing. Thus while concepts of things cannot be detached from things, words can, and when we come to symbols, of the type used in logistics, the detachment attains a maximum.

Rational thought about things is an ordering of concepts (in which we know the things), according to certain fundamental and self-evident laws. No self-evident connections exist between the material signs in themselves. The connections we are accustomed to find, are derived wholly from the concepts signified. But language is, as it were, vitalised by thought, of which it is the instrument; and language thus vitalised is, in its turn, a help to thought.

Suppose, now, that we replace spoken words by symbols instead of by written words, then we may after practice learn to think with these symbols, using them as signs of concepts, without the intermediary of the word. These symbols will then be *ideographs*. In the last analysis, however, we shall still be thinking with concepts, and the self-evident laws of thought will still govern the process.

This, however, is not the method of logistics. In an interpreted axiom-system the symbols originally designate concepts; and even in the uninterpreted system the logical constants have a meaning, and represent a rational relation between concepts. But the essence of the method really consists in detaching the symbols from thought and materialising the relations between them as a system of manipulatory rules. "The symbols," says an authority on logistics, "must be such as to facilitate the deduction of conclusions according to a mechanical process in which thinking is reduced to a minimum."

The idea that this is possible appears to be derived from the beliefs that it is achieved in mathematics and that mathematics is nothing but a department of logic. But the object of logic is the order among *concepts*, including, of course, the concepts of quantity whereas the object of mathematics is the order among *quantities*. The proposition that the three angles of a plane triangle are equal to two right angles, is a *mathematical* proposition. *The reasoning* involved in establishing this proposition is tested by logic; but logic could not itself establish the proposition, because the properties of geometrical figures do not form part of its *specific* object. When

logic studies the relation between the concepts of quantity, it considers the relation as ontological, not as quantitative ; and deals with combinations and dissociations on the plane of being (in esse), not on the plane of quantity.

The fundamental characteristic of the category of quantity, in which it differs from everything else, is that quantities can be added and subtracted. The more complex mathematical operations can ultimately be reduced to addition and subtraction. Furthermore, while the formal object of logic, the concept, exists only in the mind, the formal object of mathematics may be *either* an imaginary quantity or a real quantity. Thus we can make an addition not only " in our heads," but by putting concrete objects together, as with the abacus. The simplest kind of written mathematical operation is of this kind. When we put down one dot beside another dot, we have not merely symbolised the addition of quantities, we have actually added one concrete thing to another concrete thing, and the process, as an *addition*, is *immediately intelligible*, just as a triangular figure is intelligible. The mathematical sign-language is simply a shorthand representation of this, in which rational law is openly visible. The reason why we manipulate the symbols in a certain manner is not merely that we have learned a set of manipulatory rules, but the fact that the rational laws of quantitative relations are directly visible in the symbol-system and compel us to do so. Even though we substitute a non-specific quantity-sign, such as a, for a specific number such as 2, our symbol remains *adequate* and is still visually subject to the laws of quantitative entities.[1]

It is quite otherwise with non-quantitative entities. Geometrical figures can, it is true, be used to represent, analogically, certain logical connections ; but even here, the *rational* law involved is not in itself representable in the diagram, since it connects entities that exist only in the mind and are visible only to the intelligence. Any fact or factual relation can, of course, be represented by a symbol,

[1] If mathematics can be deductively developed from logistics, it follows that what we have called visible intelligibility is not necessary to it. This is, in fact, the opinion of the partisans of axiomatic geometry, for whom the euclidean continuum is not inherently privileged in relation to the non-euclidean continuum, though the conclusions of a geometry based on non-euclidean " axioms " are not directly verifiable in the imaginative intuition. But Gonseth has shown that intuition cannot be totally eliminated from geometry ; and this is indicated by the efforts writers using non-euclidean systems make to induce their readers to *see* what they mean, starting from the basis of euclidean constructions. (Cf. Sir James Jeans on " spherical space " in *The New Background of Science*, 1933.)

but outside of the quantitative order, we cannot make a symbol-system in which rational connections are *visible*: merely one in which they can be learned. The exponents of classical logic assert that the logisticians have been led by their manipulations into erroneous conclusions about the syllogism, which suggests that even in relation to concepts of the greatest generality one cannot safely trust to the system.

But the fundamental difficulty involved in the application of logistics to biology is that biology is not a deductive, but an inductive science. Dr. Woodger seems to believe that by applying mathematical logic to biology, we can develop a "rigorously scientific theoretical biology." Now "theory" does not, as Dr. Woodger suggests, concern merely the language in which results are stated. It concerns the way in which things happen, or the causes that are at work. Mathematics (like philosophy and logic) is a deductive science, dealing with things that are essentially intelligible, so that when the formal object has been defined, this definition can be taken as a starting-point for a series of syllogistic arguments leading to new propositions, established by reason alone. But though this is possible with algebraical expressions and triangles, it is not possible with cows and cells. We cannot get any new information about the cell, by considering our definition of it; because this definition is merely *descriptive* and does not present the mind with the intrinsic intelligible connections necessary for a deductive argument. Biology is therefore one of those sciences in which the student must maintain intimate contact with external reality, which perfect themselves as sciences, not so much by refining their language as by applying themselves more accurately to the facts. Here, as in all fields of rational effort, the perfecting of language is no doubt highly desirable and advantageous. But in an inductive science, dealing formally with material objects and real events, the symbolic language of logistics, in which the reference through concept to object is deliberately abolished and thinking is replaced by the mechanical manipulation of material signs, seems essentially unsuitable as an instrument of thought. Much as one must admire the industry and enthusiasm of Dr. Woodger, it is difficult to regard his thesis as anything but a deliberate defiance to the nature of biological science.

REVIEWS

MATHEMATICS

A Treatise on the Analytical Dynamics of Particles and Rigid Bodies; with an Introduction to the Problem of Three Bodies. By E. T. WHITTAKER. Fourth edition. [Pp. xiv + 456.] (Cambridge: at the University Press, 1937. 25*s*. net.)

IT is now ten years since the third edition of this treatise was published, and it is very gratifying to find that there is still a steady and continuous demand for a book dealing with this classical branch of Applied Mathematics. It would be superfluous now to attempt any praise or introduction of the work. Since the first edition was published in 1904 it has been the classical treatise on the subject of analytical dynamics—a work at once eminently readable, rigorously exact and almost encyclopædically comprehensive. The book contains an almost exhaustive account of all the standard soluble problems in the kinetics of particles and rigid bodies. Moreover, it contains a general account of the theoretical developments of dynamics associated with the names of Lagrange, Hamilton, Gauss, Hertz, Appell, and Lie. There is an excellent account of the application of the Theory of Continuous Groups to dynamics, and the volume closes with a detailed study of the classical problem of three bodies.

In the new edition references to recent work have been inserted. During a rapid perusal of the book I noticed the absence of only one topic which I should like to have seen discussed, namely the elegant geometrical treatment given by Darboux of the problem of the determination of the trajectories of a dynamical system. His discussion, which does not seem to be very well known, is given in Tome II of *La Théorie des Surfaces*, and by associating the determination of dynamical trajectories with the determination of geodesics of a certain Riemann manifold it throws a flood of light upon both problems. This is, however, a trifling criticism of the book under review, which will always remain a monumental work for students of rational mechanics.

G. T.

Contributions to the Calculus of Variations (1933–37). [Pp. viii + 566.] (U.S.A.: University of Chicago Press; Great Britain and Ireland: Cambridge University Press, 1937. 13*s*. 6*d*. net.)

THIS book is the third of a series on the Calculus of Variations published by the Mathematics Department of Chicago University. It contains eleven Ph.D. theses on a wide range of topics, including the problem of Lagrange with various side conditions, the inverse problem of the Calculus of Variations, the conditions for the minimum of a functional, etc.

A thesis on "Fields for multiple integrals" deals with the attempts which have been made to extend to multiple integrals some of those notions (field, invariant integral of Hilbert, etc.) which proved so useful in the theory of the simple integral. The theorem of Jacobi, which states that from a complete integral of the Hamilton equation the totality of extremals can be determined by differentiation, does not extend to multiple integrals. Attempts to generalise it from the functional point of view have not proved of service in the problem of determining an external surface through a given boundary curve.

Another thesis "On the dependence of the focal point upon Curvature in the Calculus of Variations" is written in the same notation as the recent tract on Finsler space by Cartan, with which this paper has many points of contact. The Euler equations for the integral of the second variation, which are the Jacobi equations for the original problem, are extensions to spaces with a generalised metric of the equations of geodesic deviation given by Levi-Civita for Riemann space.

E. T. D.

Introduction to Mathematical Probability. By J. V. Uspensky. [Pp. x + 411, with 20 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1937. 30*s*. net.)

The book starts from the following definition of mathematical probability (p. 6):

If, consistent with conditions S, *there are* n *exhaustive, mutually exclusive, and equally likely cases, and* m *of them are favourable to an event* A, *then the mathematical probability of* A *is defined as the ratio* m/n.

The first few chapters deal with the direct calculation of probabilities, including contingent probabilities, and lead up to Bernoulli's theorem and its applications (Chaps. VI–VIII). Mathematical expectation, dispersion and standard deviation is described in Chap. IX, the law of large numbers and its applications in Chaps. X and XI, geometrical probability in Chap. XII and distribution, correlation and other important topics in Chaps. XIII–XVI. The exposition is lucid and the subject matter interesting. The book will be valuable to students in any subject involving statistics, contemporary physics for example.

The relation between the calculation of probabilities and experiment is illustrated in Chap. VI. Eight series of experiments are described, which consist of tossing a coin many times and counting the number of "heads" and similar observations. I find it difficult to understand what these experiments show, and the difficulty goes back to Uspensky's definition of mathematical probability, in which, as he confesses in an earlier passage, the term "equally likely" is undefined. If the meaning is taken for granted (*cf.* J. M. Keynes' statement of the "Principle of Indifference" in his book on probability), what, if anything, does an experiment with a coin indicate, beyond the trivial fact that the particular coin used has no mechanical eccentricities? However, the scope of the book is explicitly mathematical, and Uspensky was perhaps wise in avoiding even the mildest excursion into the domain of philosophy.

J. H. C. W.

The Handmaiden of the Sciences. By E. T. BELL. [Pp. viii + 216, with 34 figures.] (London: Baillière, Tindall & Cox, 1937. 9s. net.)

PROFESSOR BELL has set out to describe in simple language the applications of mathematics to physical science from the time of the Greeks to the present day. The achievements of the Greek geometers, the calculus as a weapon for the measurement of change, Cartesian geometry opening out into general relativity are all passed under review. Discussion of these matters is followed by chapters on invariance, least action, wave-motion and the theory of groups as the mathematics of the discrete. The style is forceful and witty and the expositions both simple and clear. The general impression left on the reader is that mathematics, though a necessary tool in physical science, is not as perfectly adapted to the work it has to do as we commonly suppose. Scattered throughout the book there will also be found remarks giving the author's views on such varied subjects as school-teachers, lawyers, militarists and the perversity of human nature in general.

Unfortunately the treatment can hardly be called an objective account of a difficult subject. The author is evidently strongly prejudiced in favour of the analytical as against the geometrical way of thinking in mathematics. Thus he gives the impression that the theory of relativity is a rather unsatisfactory stop-gap required by minds not yet capable of purely analytical thought without, however, stating that this is merely a personal view. Again, he regards the logical basis of continuity in mathematics as unsound and objects on this ground to the use of continuous mathematics in physical science. By implication he seems to regard as self-evident the proposition that the description of nature must necessarily form a logically consistent system. On firmer ground is his contention that at the present moment quantum phenomena suggest a need for a discrete or discontinuous type of mathematics in physical science. But here again the case for "discreteness" is by no means as strong as Professor Bell apparently believes it to be. The theory of wave-mechanics, in spite of its imperfections, clearly shows how far it is possible to travel along the road of continuity even in dealing with the atom.

G. C. McV.

Segmental Functions: Text and Tables. By C. K. SMOLEY. [Pp. xliv text, with 11 figures + 438 tables.] (Scranton, Pa., U.S.A.: C. K. Smoley & Sons, 1937. $5.00 net.)

THE purpose of this book of tables is to facilitate the calculations pertaining to segments of a circle. The text explains clearly, by numerous examples, the use of the tables, and, when the notation has been mastered, calculations can easily be done. Such problems as finding the length of the chord, given the angle of the segment and the radius, the finding of the area of a segment, are capable of quick solution. The scope of the tables is somewhat limited, for repeated calculations involving segments of a circle are few; possibly their best use would be in the calculations of the flow of water in circular pipes.

In addition there are some of the more common tables; logarithms; squares; cubes; square and cube roots; reciprocals; and the more unusual ones of logarithms of feet and inches from 0 ft. to 200 ft., and ordinates for the setting out of circular curves.

All tables are written as logarithms, and this form might advantageously be adopted in many books of tables.

A. M. B.

Trigonometry. By T. M. MacRobert, M.A., D.Sc., and William Arthur, M.A. (London : Methuen & Co., Ltd., 1938.) **Part III : Advanced Trigonometry.** [Pp. xi + 345–478, with 5 figures], 4*s*. 6*d*. **Part IV : Spherical Trigonometry.** [Pp. x + 479–541, with 20 figures], 3*s*.

Parts I and II of this book have already been reviewed in this Journal.

Part III is concerned with the more advanced aspects of the subject, attention being directed to the convergence of series, to infinite products, and to functions of a complex variable. The treatment is detailed and, as regards series and products, the book is as much a study of algebra as of trigonometry. The applications are, of course, chiefly trigonometric. (It is to be noted that the authors are determined to popularise the hypergeometric function.)

This book will be very useful to students for both general and honours degrees. As in Parts I and II, there is a valuable collection of worked examples, and the reader can test himself on plenty of others at the end of each chapter.

Part IV does for a spherical surface what Part I does for a plane. In forty pages of text the authors generalise the formulæ of plane trigonometry and finish with a chapter on the solution of spherical triangles. Few will be thrilled by spherical trigonometry and, I imagine, most will be thankful to the authors for their brevity.

It is proper here to survey Parts I–IV as a whole. They give the impression of a course well organised with a view to examinations. Students want the best theorems and these books have them. The student who wants to know why these are the best theorems may be glad to have, in addition, the help of a teacher who can point out the trend and discuss the significance of particular results.

J. W. A.

ASTRONOMY AND METEOROLOGY

The Observational Approach to Cosmology. By Edwin Hubble. [Pp. ix + 68, with 7 plates and 3 figures.] (Oxford : at the Clarendon Press ; London : Humphrey Milford, 1937. 6*s*. net.)

Dr. Hubble was appointed to deliver the Rhodes Memorial Lectures at Oxford in 1936, and their substance is now published in book form.

Following an historical sketch showing how cosmological ideas gradually progressed from the small closed universe of the Greeks, with its thin boundary shell of stars, Dr. Hubble describes the observational methods which, with the aid of the 100-inch telescope, have enabled him to present the modern picture of a sample of space, 1000 million light-years in diameter, throughout which are scattered with approximate homogeneity 100 million nebulæ averaging between 15,000 and 20,000 light-years in diameter. A further observational contribution is the Law of Red Shifts, according to which each line in the nebular spectra is displaced towards the red by an amount increasing with the apparent faintness of the nebula.

Different interpretations of these data produce widely different pictures of the Universe. On the natural assumption that the red shifts are due to radial motion of the nebulæ we find that these velocities of recession accelerate with the distances, and we are confronted with an expanding universe of finite volume, so small that possibly one-quarter can be explored with existing

telescopes, and so young that the "initial instant," marking the commencement of the expansion, must fall within the life-history of the earth. Alternatively, if the red shifts are ascribed to some as yet unknown principle of nature which reduces the energy of light in proportion to the distance it travels through space, the observations fit in excellently with a non-expanding infinite homogeneous universe, the observable region being an insignificant fraction of the whole. Dr. Hubble leaves his readers in no doubt as to the direction in which his sympathies lie. Possibly the 200-inch telescope will provide the key to the problem; at present it is obviously insoluble.

R. W. W.

Synoptic and Aeronautical Meteorology. By H. R. Byers, Sc.D. [Pp. x + 279, with 58 figures and 6 maps.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1937. 21*s*. net.)

This book is intended for airplane pilots and students of meteorology who wish to begin a study of the newer methods of synoptic meteorology. A slight previous knowledge of the subject is assumed, but it can be added that previous knowledge is by no means indispensable in order to understand this very interesting account. The author has attained his object to a very considerable extent, and the book is a valuable and interesting contribution to meteorological literature. Although the charts and other observational material are drawn from American sources, this account of American methods and practice can be read with interest and profit by English meteorologists.

After two chapters dealing with radiation phenomena in the atmosphere, and with stability, instability and vertical motion in the atmosphere, the author describes in detail what has now come to be known as air-mass analysis. He describes the distinguishing features of different air-masses, their classification, frequency over different parts of the U.S.A., and their relation to weather in general. From this we naturally pass to the procedure followed by the forecaster, with special consideration of precipitation, fog, thunderstorms, tornadoes, waterspouts and dust-storms. Charts illustrating these different phenomena are included, and great care was shown in the selection of the charts to be reproduced. We find in one of the later chapters a clear account of the problem of formation of ice on aircraft, and the author gives a statement of the localities and cloud types in which ice-formation is probable. This is the first time this subject has been discussed in any text-book, and it is a valuable feature of Dr. Byers' book.

The main thesis of the book, the description of the principles and practice of air-mass analysis, has been excellently handled. The brief descriptions of the physical principles were found less satisfying. Fig. 3 in the book is taken from a paper in which Simpson treated the radiation from the atmosphere as "grey" radiation. It would have been better if the author had mentioned the supercooling of water drops in the early treatment of convection of damp air, instead of there describing the freezing of all water at 0° C., in view of the fact that later on he gave such a clear account of the true facts. There is also a revival of the "auto-convection" gradient, both in an early chapter, and on the concluding page. Few meteorologists take this lapse-rate, which is approximately 3½ times the dry adiabatic lapse-rate, as having any very real significance, in view of the fact that near the ground on sunny afternoons lapse-rates at least 50 times as great as the auto-convection gradient are frequently to be found.

But such criticisms as are made above are on rather minor points, and the

book can be recommended to any reader who desires to learn something of the methods and results of synoptic meteorology as practised to-day in the United States.

D. B.

PHYSICS

Low Temperature Physics. By M. and B. Ruhemann. [Pp. x + 313, with 121 figures, including 2 plates.] (Cambridge: at the University Press, 1937. 18*s*. net.)

This book gives a very readable account of the domain of low-temperature physics. The authors state in their introduction that they have in mind as prospective readers physicists specialising in other fields and students who have not yet begun to specialise. For the former class of reader the book can be recommended. It contains first of all an interesting account of the history of the subject, a history going back just sixty years to the date when oxygen was first liquified. Thus the principal methods for the production of low temperatures are described, and also their industrial applications. The greater part of the book is, however, concerned with the properties of matter at low temperatures and with the methods that have been used to investigate them. An enumeration of these will give some idea of the scope of the book.

In a chapter on the crystal lattice an account is given of the investigation by means of X-rays of the crystal structure of condensed gases, with particular attention to the change of structure, if any, occurring at the "λ-points" in such substances as solid methane—*i.e.*, at the points where the specific heat shows a discontinuity.

The thermal energies of crystals are discussed in another chapter, where the anomalies occurring for solid hydrogen, gadolinium sulphate, the supraconductory elements, and liquid helium are among those illustrated. Further chapters deal with Nernst's third law of thermodynamics and the calculation of chemical constants, with paramagnetism and the magnetic cooling method, with conductivity at low temperatures and finally with supraconductivity.

Naturally, in a book of this size, none of these subjects can be treated exhaustively; the authors have, however, fulfilled their aim of providing a lively and readable account of the field in which they are interested. As a book for students, however, it has certain disadvantages. Many of the parts are so concise that it is difficult to follow them if one does not understand the subject already. The discussions of ortho- and para-hydrogen on p. 194 and of paramagnetic gases on p. 201 are open to this criticism. The authors, of course, have not set out to write a text-book on theoretical physics; but nevertheless the present reviewer finds that students reading a mainly experimental text-book are very discouraged by occasional passages of incomprehensible theory. References to other text-books in which adequate explanations of the theory are given would have increased its value.

The book was finished in December 1935, and therefore does not contain any account of the most recent work on supraconductivity, on liquid helium or on the heat capacity of metals at very low temperatures. As a book for the specialist it suffers through the absence of an index. Also no references are given in the text; there is a bibliography at the end of the book, but it is not always clear which reference there corresponds to a piece of work described earlier. For instance, on p. 215 a value of the Curie Temperature of $NiCl_2$ due to Woltjer is quoted; in the bibliography there are five papers due to

this author, and nothing to show which of these is meant. The present reviewer would very much prefer a definite system of numbering for the references in a book of this kind.

Perhaps the most valuable part of the book is the description of phase changes of the first and second order (in Ehrenfest's sense) that occur in solids at low temperatures, and of the changes in crystal structure and specific heat that accompany them. It is a field in which the authors have themselves carried out work of great importance, and we do not know of any other text-book giving an account of it from the experimental side.

N. F. M.

Thermodynamics. By E. FERMI. [Pp. x + 160, with 22 figures.] (London and Glasgow: Blackie & Son, Ltd., 1938. 12*s.* 6*d.* net.)

THIS book is based on a course of lectures delivered by the author at Columbia University in the summer of 1936. It contains a simple and painstaking account of the principles of thermodynamics, and the publisher's claim that "the aim of the book is to develop physical insight rather than to cultivate dexterity in mathematical manipulation" is thoroughly justified. The author appreciates the difficulties of the subject and knows how to meet them.

Chapters I to III deal with the first and second laws; the content of the others is indicated by their headings: Entropy, Thermodynamic Potentials, Gaseous Reactions, Thermodynamics of Dilute Solutions and the Entropy Constant—the last containing a discussion of some of the consequences of Nernst's theorem which is taken as a postulate.

The historical development of the subject is ignored. Joule is mentioned only twice; once in connection with his crude experiment on the expansion of a gas into a vacuum and once, quite casually, in connection with the heating effect of the electric current. His fundamental work on the conservation of energy is not mentioned, neither is the Joule-Thomson porous plug experiment. Such matters may quite properly be considered to be outside the scope of the book, but a discussion of the meaning of reversibility certainly is not and it is surprising that there should be no explanation whatever of the meaning of this term. The use of the letter L as a symbol for work seems very ill-advised in a book intended for English readers, while the use of the suffix $_1$ to indicate the higher temperature in heat engine cycles has become so familiar that its application to the lower temperature seems almost perverse.

These are small points but they are of importance in a book which, in other respects, is easily the best introduction to thermodynamics which the reviewer has seen. The publishers would be performing a useful service if they could persuade the author to prepare a chapter on Radiation for inclusion the next edition.

D. O. W.

Sound. By A. T. JONES. [Pp. xii + 450, with 141 figures.] (New York: D. van Nostrand Co., Inc.; London: Chapman & Hall, Ltd., 1937. 20*s.* net.)

THE output of books on sound in the last ten years bids fair to rival in quantity that on atomic physics! The present one is what is known in its country of origin as a "college physics," and is wholly experimental in character.

It is frankly designed for those whose mathematical equipment is a minimum, and it could easily be used in an English secondary school. The author, it is true, has—rather apologetically—to introduce the concept of a logarithm, but the meaning of this term is explained to the uninitiated in an appendix. We do not by this intend to belittle the book. It is written in an intriguing and critical style, and even an experienced teacher of the subject, who may have had his appreciative faculty coarsened by much lecturing to students, will find much that is stimulating and a good deal probably that is new. In particular, the biographical notes on the investigators whose names are mentioned will be found useful by the teacher who does not wish to make his lectures entirely objective. The book is written in the "classical" style; it has a distinct flavour of Tyndall and Helmholtz about it. Although a chapter is devoted to technical applications of sound, nothing is said of acoustic impedance, while supersonics is dismissed in a dozen lines. We are pleased, however, to find good chapters on musical instruments and the voice. Prof. Jones has himself contributed to research work in sound for some time past, but with unnecessary modesty makes little reference to his own work. The text is interspersed with questions—not arithmetical problems—for the student to ask himself. Some of these are thought-provoking; others mere revision; not a few naïve.

In short, the book will be found useful in the higher forms of secondary schools and—with some supplementary mathematical theory—for the pass courses in universities; but the price is high for an elementary text-book and this may mitigate against its general adoption in England.

E. G. R.

Sound Waves: Their Shape and Speed. By D. C. Miller, D.Sc., D.Eng., LL.D. [Pp. xii + 164, with frontispiece and 64 figures.] (New York and London: Macmillan & Co., Ltd., 1937. 12*s*. net.)

Several apparently disconnected acoustical subjects are treated in this book. They are, however, related in this way that most of them have been investigated by means of the phonodeik. This instrument was invented by the author and he describes it in considerable detail. The reader will not fail to be impressed by the care and skill expended on its design. The book is divided into two parts. In the first part the phonodeik is used to investigate the shape of sound waves produced by the human voice, musical instruments and noise sources. The photographs of these characteristic shapes will not all be new to readers, but they form an excellent collection for purposes of reference. A novel suggestion (p. 61) connects the characteristic shape of a musical sound wave with the profile of the human face. It should have great possibilities if it meets the eye of a music-hall comedian!

The second part of the book is devoted to the results of the author's important experiments on the pressure, wave form and velocity of sounds from the firing of large guns. It incorporates and extends previous incomplete reports made in several American and Canadian journals. An interesting method of least-squares solution is applied to the results of observations on the velocity of the sounds of explosions. The explanation of the abnormal velocity associated with these sounds is given in a particularly clear manner.

E. G. R.

Light: Principles and Experiments. By GEORGE S. MONK. [Pp. xi + 477, with 269 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1937. 30s. net.)

THIS book contains an attractive, if slightly superficial, account of the *whole* of optics to a standard approximating to that of the English honours degree. It lacks any detailed account of the electromagnetic theory, gives a quite inadequate treatment of double refraction in biaxial crystals and omits certain other details commonly included in courses in this country, *e.g.* the standardisation of the metre in terms of the wave-length of light, the use of the Lummer plate, and all reference to colour phenomena in polarised light. On the other hand, it contains an excellent account of the aberrations of lens systems, discusses modern reflecting telescopes, gives a short mathematical account of the formation of fringes in the Michelson interferometer and of the visibility of the fringes in the Michelson method for the determination of stellar diameters, tells its readers something of the elements of the theory of spectra and contains chapters dealing with photometry and with colour vision.

The author informs us that his object was to provide a text-book for use in "intermediate courses" in the U.S.A. which would obviate the necessity for using a whole series of books in order to cover a properly balanced course in the subject, and it seems likely that he may have achieved his purpose. Clearly, the standards of the various stages of learning in American Universities overlap those customary here; even so, English students reading optics for pass or honour degrees will find the book very helpful, the more especially as the author has given references to the older "classics" at those points where exigencies of space have forced him to undesirable brevity.

Seventy-odd pages are devoted to descriptions of experiments, some of an advanced type involving, for example, the use of a spectrophotometer, Michelson and Fabry-Perot interferometers and Babinet compensator. There are problems (with numerical answers) at the ends of most of the chapters and an adequate index. Altogether a useful, and sensible, additon to the Optics library.

D. O. W.

Fundamentals of Physical Optics. By FRANCIS A. JENKINS and HARVEY E. WHITE. [Pp. xiv + 453, with 272 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1937. 30s. net.)

THIS textbook is intended for use in "an advanced undergraduate course" in optics. Its scope is limited to classical wave optics presented with a minimum of mathematics, for "applications of the calculus have been purposely made very brief, but are always included for the benefit of those students with a mathematical turn of mind." This policy necessitates the quotation of many formulæ without proof, including, for example, the expression for the intensity of Fabry-Perot interferometer fingers and the Fresnel formulæ for the amplitudes of waves after reflection and refraction at the surfaces of transparent media. Such a procedure is not satisfactory from the point of view of English students at the honours stage and sometimes it is quite exasperating. Thus, on p. 113, we are told, anent the $1 + \cos\theta$ obliquity factor, that Kirchhoff's treatment may be found in Born's *Optics* (? *Optik*) and that the result in Drude's *Theory of Optics* is incorrect. A full and clear discussion of this fundamental matter would have been of very great value; it might even have produced a material increase in the sale of the book!

With all these restrictions the authors have ample space to explain the physics of many phenomena in considerable detail. This they have done with great success, so that the book is well deserving of a place in the science library, even though it cannot be recommended for purchase by a student able to afford only one or two books on Light.

There are particularly good accounts of diffraction phenomena of both the Fresnel and Fraunhofer classes, of the effects of motion on the velocity of light, of the phenomena associated with the reflection of light at both dielectric and metallic surfaces and of magneto- and electro-optics. There is a short description of the interference theory of dispersion, and special attention is paid to the importance of the group in various phenomena. The treatment of interference is skimped, and the limitation to wave optics has precluded any account of the theory of spectra.

The diagrams are good and there are many half-tone photographs of fringes, spectra, etc. There are footnotes giving brief biographical details of the leading personalities, problems at the ends of the chapters (no answers) and a good index with the principal references in heavy type.

D. O. W.

The Science of Seeing. By M. Luckiesh, D.Sc., D.E., and F. K. Moss, E.E. [Pp. x + 548, with 143 figures and 1 "visibility indicator" in pocket at back.] (New York: D. van Nostrand Co., Inc.; London: Macmillan & Co., Ltd., 1937. 25*s*. net.)

The authors of this book have, during the past few years, published a number of papers (most of them before the American Illuminating Engineering Society) in which they have urged that in studying the effects of lighting it is not sufficient to consider only the physical stimulus (illumination) and the behaviour of the eye when acted upon by this stimulus. "Seeing," they say, "is more than vision . . it is an activity of the human body and mind. . . . Thus, seeing involves a complex chain of events beginning with the external physical world which affects visibility and comfort, passing through the visual sense, eventually entering a complex realm of unobvious physiological and psychological effects and finally ending in an accomplishment of some kind." They are thus led to the conception of what they term "the human seeing-machine," in substitution for the eye, just as they would substitute "the science of seeing" for illuminating engineering as hitherto conceived.

This novel approach should prove highly stimulating to anyone whose ideas on lighting show a tendency to become fixed or to travel along the lines of convention, and it can scarcely be doubted that for this reason alone the book will prove of immense value and will give illuminating engineering a fresh impetus and urge it to still greater efforts and more ambitious aims. The authors have earned the thanks of all who have the interests of good lighting at heart and the publishers have well performed their part in producing a book which should be read by every illuminating engineer.

J. W. T. W.

Relativity and Robinson: A Treatise for Very Simple People. By C. W. W. [Pp. viii + 118, with 47 figures.] (London: The Technical Press, Ltd., 1937. 3*s*. 6*d*. net.)

Of the very simple people for whom this book has been written Robinson has been chosen for a place on the title page doubtless because he is an obliging person in the cause of alliteration.

A few years ago popular books on the subject of relativity were more numerous than they are now. That is perhaps because we have become hardened to the difficulties which relativity presents to the beginner and the non-specialist, and are now confronted with the less pleasant difficulties of the quantum theory. It may be that relativistically speaking there are fewer simple people in the world to-day and that the number of half a dozen who, according to the evening newspapers, really understood the theory, has grown somewhat. The author seems to have taken this point of view, for he has written his book for the superlatively simple. But in spite of its belated arrival this charming little work is welcome and there will be many who are simple enough to enjoy it.

The presentation of the subject is without even simple mathematics, even without the use of the multiplication tables and is based on analogy with geometry. The reader is taken by geometrical steps from one to four dimensions.

Only in one chapter have we felt the argument somewhat halting and that is in Chapter IX, where Robinson also raises an objection. It is on the question of changing time axes, and the difficulty seems due to the fact that the author has not sufficiently stressed the analogy with lower dimensions just at the point where it is most needed.

This book is to be recommended to general readers and to students who hope to become specialists in physics. It succeeds in getting on with its main purpose and though written in light vein sacrifices nothing for a phrase or forced joke.

H. T. F.

The Fundamental Principles of Quantum Mechanics. By E. C. KEMBLE. International Series in Physics. [Pp. xviii + 611, with 27 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1937. 36*s*. net.)

The Elements of Quantum Mechanics. By SAUL DUSHMAN, Ph.D. [Pp. xiv + 452, with 82 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. 25*s*. net.)

IN spite of the large number of books which have been written about quantum mechanics, it is not always easy to find one suitable for students who wish to learn the subject with a view to carrying out research on its applications to physical or chemical problems. Treatises such as that of Dirac, or Pauli's article in the *Handbuch der Physik*, are for the advanced research worker who desires to understand the difficulties inherent in, for instance, the theory of the interaction between matter and radiation, and the sort of advances that must be made before quantum mechanics can be applied to the nucleus with the same confidence as to problems of atomic spectra. Or else they are for those whose interest in the subject is mathematical rather than physical, and who are interested rather in deriving the laws of quantum mechanics with the maximum degree of elegance from the smallest number of postulates, than in applying them to elucidate hitherto unexplained facts in the realm of physics and chemistry.

A number of *specialised* books exist on the branches of science to which quantum mechanics has been applied; for instance, nuclear theory, band spectra, chemical bonds, collision problems, statistical mechanics, properties

of solids, and so on. But these books all assume that the reader has a fair working knowledge of both the fundamental ideas and the mathematical methods of quantum mechanics, and this a student has first to obtain from other sources.

There are, moreover, a number of books written for readers with rather limited mathematical resources, such as chemists or experimental physicists who find it necessary to understand what the new theories are about, but who do not wish to undertake theoretical research on their own behalf. But the student who does wish to carry out such researches will not, of course, find in books of this kind any detailed description of the mathematical methods that he will have to use, such as perturbation theory, the variation method, the method of Wentzel, Kramers and Brillouin, and so on.

For this reason we welcome the appearance of two straightforward, we might say, humdrum, books about quantum mechanics. Both these books, we feel, could be recommended unreservedly to a research student in theoretical physics; they are not, like so many of the earlier books, written for readers with a mature knowledge of classical physics.

Prof. Dushman's book is much the more elementary. As well as the chapters on quantum mechanics, there are sections dealing with allied mathematical problems which the student ought to understand first, such as the theory of vibrating strings, Lagrange's equations in dynamics, and coupled vibrating systems. Then the book describes the Schrödinger equation and its application to certain physical problems, such as the transmission of particles through potential barriers, the hydrogen atom, the forces between molecules, activation energies in gaseous reactions and so on. The mathematics is in all cases presented fully, but treated always as a tool for obtaining results which can be compared with experiment. Though the author does not claim any originality of treatment, he has produced a thoroughly useful and sensible book, which can be recommended to the young theoretical physicist as a help in learning the technique of his subject.

Prof. Kemble's book is more ambitious and goes more deeply into the fundamentals of the subject, and makes some attempt at mathematical rigour. To use his own words, he has attempted to bridge the gap between the exacting mathematical technique used in such advanced works as that of von Neumann and the usual less-rigorous formulations of the theory, but limiting himself to such mathematical methods as are the common property of physicists to-day. If by physicist is meant the young worker in theoretical physics who has taken an honours degree in mathematics, we think he has succeeded. His book will also serve as a connecting-link between applied quantum mechanics and those text-books which deal with general principles only. Even when dealing with the operators and matrices, he keeps quite close to the applications of the theory. The sections on the methods used for obtaining approximate solutions of special problems seem to us especially good, as do also the discussions of the interpretation of the mathematical formalism, which is given in terms of Gibbsian assemblages of independent systems.

A possible criticism of both books is that very scant attention is given to collision problems, that is to say, the scattering of material particles or of light by atoms or molecules. Both books emphasise the problems of atomic and molecular structure, in which the electrons are bound in stationary states.

N. F. M.

Atomic Artillery: Modern Alchemy for Everyman. By J. K. Robertson, F.R.S.C. [Pp. xiv + 177, with 26 figures, including 6 plates.] (London: Macmillan & Co., Ltd., 1937. 10s. 6d. net.)

This addition to the long list of books on Atomic Physics for Educated Laymen is well suited to those who are sufficiently interested in what is going on to demand a more extensive and accurate account than occasional newspaper reports, but who have little or no knowledge of mathematics or training in physics. To quote the preface, "it tells the story of electrons, protons, positrons, photons and cosmic rays, and explains step by step the games of shooting atoms and of turning one element into another." Such a story is not easy to tell in a convincing and yet elementary way. The newcomer to the subject may find difficulties with the chapter on wave and photon aspects of light, and the expert may quibble with an occasional loose statement, but on the whole there is no doubt that the author has succeeded in his purpose. After all, this is one of the adventure stories of physics, and it is stimulating to read of explorations made by the various workers (whose nationality is carefully mentioned).

School and Public librarians will find the book popular. Its value is enhanced by excellent cloud-chamber photographs and it is well produced, as may be expected from the rather high price.

F. A. V.

Glossary of Physics. By Le Roy D. Weld, Ph.D. [Pp. x + 255.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1937. 15s. net.)

As soon as a physicist strays from his own particular field and attempts to read papers in another special branch of the subject, he comes across unfamiliar terms and finds X's equation or Y's law taken for granted. The glossary before us collects most of the laws and technical terms likely to be encountered and gives brief definitions or descriptions, together with one or two references.

Prof. Weld has been assisted in its compilation by a committee of the Division of Physical Sciences of the American National Research Council and a number of special consultants. It is stated in the preface that "the descriptive character of the Glossary has been emphasised from the first, with deliberate attempt to avoid accurate definitions or of authoritatively standardising the use or meaning of terms. The sole purpose is to give information as to actual usage, and in such form as to be intelligible to students as well as to specialists." On the whole, there is no doubt that this purpose has been achieved, though in some cases the definitions seem too brief and indefinite to be worth including. For example, under the heading "electromagnetic waves" we have merely "waves of electromagnetic radiation" which does not take us much further. It is inevitable that the choice of subjects and definitions will not suit all physicists, but it is certain that every one will find the book useful and convenient.

F. A. V.

Engineering Mechanics. Vol. I: Statics; Vol. II: Dynamics. By S. Timoshenko and D. H. Young. [Vol. I: pp. xiv + 334, with 464 figures; Vol. II: pp. xii + 323, with 314 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1937. 15s. net each.)

These two excellent volumes are admirably suited to the needs of the engineering student. In their preface the authors call attention to the fact that the industrial engineer is continually being confronted by new problems which do not yield to routine methods of solution, and that the need of the engineer is for a sound understanding of fundamental principles and of general methods of applying these. They describe the aim of the book as being "to acquaint the student with as many general methods of attack as possible, to illustrate the application of these methods to practical engineering problems, but to avoid routine drill in the manipulation of standardised methods of solution." It is not too much to say that the books do very successfully fulfil that aim.

The fundamental principles of mechanics are stated and explained clearly, and illustrated by simple examples, and the problems involving applications are solved by direct reference to principles. While the subject is dealt with from the beginning, progress is necessarily rather rapid in the early stages, and the books are therefore best suited to students who already have some knowledge of the elements of mechanics.

It is perhaps to be regretted that the authors have not dealt more fully with the effects of impulses upon rigid bodies, or shown the application of the principle of angular momentum to such problems as those of clashing gears and the reactions in epicyclic trains. The connecting-link which these volumes supply in so many other cases would have been equally valuable here, and the problems themselves are not more advanced than some others that are considered.

This is, however, a comparatively small omission, and it is recognised that such a book cannot deal with all possible engineering problems. In other respects—notably in dealing with graphical statics, the analysis of space frames, and such problems as the balancing of rotors, the authors have connected theory with practical applications in a way that should ensure to an intelligent reader a thorough understanding of the subject.

H. T. J.

Applied Fluid Mechanics. By M. P. O'Brien and G. H. Hickox. [Pp. xiv + 360, with frontispiece and 166 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1937. 21s. net.)

The term "Fluid Mechanics" has strange interpretations; O'Brien and Hickox use it, although their text-book exclusively concerns the Hydraulics of Civil Engineering, or rather certain branches of it. Admittedly in bringing the treatments up to date the authors draw upon the Science of Fluid Mechanics, but their present contribution to that subject is negligible, for they dispose of scientific matters by the method of bare statement of fact without criticism or even the direct use of experimental data. To some extent the engineering end also suffers from this defect; but here one sympathises with the authors in their attempt to cater for time-starved students who always feel that craftsmanship is more important than theory, and so want plumbing in such detail that their engineering must be in potted form.

Frankly, even as a text-book of Hydraulics, the authors have overdone

the "potting" in many places, though in general they do know what they are writing about, and do give lists of references to fuller treatments. Each section, despite its dogmatic form, is stimulating, and the reader is left full of questions, rather than in the usual state of false security that he now knows all about it. He will refer to the original papers, but whether he will be able to read them when he sees them is more doubtful.

Perhaps the most important new feature is the inclusion of a couple of pages dealing with the "boundary layer" conception, which is mentioned for the first time in any text-book of hydraulics in the English language. Unfortunately the authors do not appear to be fully aware of its implications and lose the opportunity to explain many of the phenomena described in other parts of the book.

Despite the prosaic chapter headings the syllabus is definitely more attractive than any previous one written in English. The style perhaps is a trifle unpolished, but it is concise and tolerably clear. There are excellent photographs of flow patterns and of models. The 150 diagrams are good, but far too few in number to do justice to the breadth of the work. The index is poor.

In a book which introduces much new subject matter, one must overlook many faults, but it is a pity that (p. 104) right at the heart of the whole subject the authors should give two different curves of resistance for increasing and for decreasing speeds. Although they refer to Reynolds' basic work on this, they have drawn their information at second-hand and then from an erroneous source.

Equally one feels intolerant when after being taken at length through the recent accurate and logical work of Karman, Prandtl, and Nikuradse (pp. 107, 116 *et seq.*), one finds that (p. 266) the authors return to the horribly complicated and erroneous empirical formula of Kutter (1869) and proceed to enlarge upon it.

Pumps, turbines and propellers are completely omitted; and so are underground water and filtration; river hydraulics, or rather the transport of bed-load and silt, is hardly mentioned. Models are disposed of too briefly and not too well in 30 pages. One may well ask what remains; and surprisingly the answer is 300 pages of concise matter mostly essential to the Civil Engineer if he is to understand current practice. As, without exception, the other text-books in English merely summarise the ideas and methods in use at the end of the last century, one must regard the present one, despite its faults, as marking the first step towards that ideal of technical education which would prepare the student to face the problems of a generation ahead.

C. M. W.

Hydraulics: A Text on Practical Fluid Mechanics. By R. L. Daugherty, A.B., M.E. Fourth edition. [Pp. xiv + 460, with 318 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1937. 21*s*. net.)

More than a hundred pages of this, the fourth edition, are devoted to a treatment of the statics and dynamics of fluids, the text being presented in such a manner that problems dealing with any fluid may readily be solved. The emphasis, however, is laid on problems encountered when water is used for engineering purposes.

The remainder of the book deals with pipe and channel flow, and with the theory and design of turbines and pumps. A chapter which should be

useful to the student is that on non-uniform flow in open channels, and the well-illustrated descriptions of water power plants is welcome in a book of this type. The chapters dealing with turbines and pumps are illustrated by photographs of the type required by the student to enable him to appreciate the relationship between theoretical diagrams and actual machines.

Throughout, the presentation is clear and concise, and mathematical derivations are liberally interpreted by descriptive matter and diagrams. Space has been saved by the wise omission of charts, which often appear in a work on hydraulics. These may result in a saving of labour to the designer who is fully acquainted with his subject, but they are of doubtful value to the student, more concerned, as he is, with fundamentals.

More worked examples might have been included, but the 385 examples set seem to be of the type which the student may readily solve with the assistance of the text. In this connection, it would have been better if emphasis had been laid on the fact that there is a difference in volume between the U.S. and the Imperial gallon, in consideration of the sale of the book outside the United States. A welcome addition would have been a chapter dealing with the use of models in the solution of hydraulic problems.

The book is recommended to students as one which deals thoroughly with the fundamentals of the most important branches of hydraulic study. The notation used is that recommended by the American Standards Association.

W. Fisher Cassie.

Conveying Machinery. By William H. Atherton, M.Sc., M.I.Mech.E. [Pp. viii + 188, with 17 plates and 121 figures.] (London: The Technical Press, Ltd., 1937. 21*s.* net.)

The author claims thirty-two years' experience of conveying machinery and mentions eight papers and addresses which he has read before British provincial engineering societies. Few classes of mechanical appliance are more universally used, or exhibit so wide a range of methods of achieving a common end. Mechanical handling enters in some form into practically every constructional and industrial operation. In consequence one cannot expect to cover the whole range of the subject, and simultaneously to give all details full treatment, in a book of 180 pages.

The author refers to most types of conveyor, but the source of information seems mainly limited to one firm. This seriously detracts from the value of the book. For example, the belt conveyor is undoubtedly the most generally used of all types, and its applications have widened much in recent years; yet the author completes his statement on "Band and Belt Conveyors" in one section of 4½ pages and a few scattered references. Pneumatic and hydraulic methods are ignored, save for a general remark on blowing coal-dust into furnaces. The worm conveyor is dismissed with passing references in two paragraphs.

On the other hand, chain-operated conveyors and elevators are treated at length, and a special chapter of 32 pages is devoted to "Conveyor Chains and Wheels." Three-quarters of the chapter deals with the products of one firm, many of which are scheduled in tables; other makers' products are described generally in the remaining quarter.

The table of unit weights of bulk materials is unusually extensive. The figures could be used with more confidence if the conditions of measurement were given as well as some estimate of the range of variation. Some checking

of the individual figures seems desirable. For example, the unit weights of "Portland cement" and "ground cement" are given as 86 lb. and 120 lb. respectively, though most Portland cement, in conditions in which it is carried by conveyors, ranges between 90 and 96 lb. per cu. ft.

Descriptions are clear and well illustrated. Subject to limitations the book should be of use to those for whom the knowledge of conveying plant is only ancillary to that of the main machinery of production. The practising designer and constructor of conveying machinery will find some useful details, but he will have to supplement his information considerably from other sources.

E. G. WALKER.

The Battery Book. By HAROLD H. U. CROSS. [Pp. xii + 196, with 92 figures.] (London: The Technical Press, Ltd., 1937. 5s. net.)

IN these days of multiplicity of electrical "gadgets" on the modern car, the battery is brought forcibly to the notice of owner drivers. They and other battery users will find that Mr. Cross has written a practical manual in simple language, aided by many illustrations and containing a number of hints and suggestions for their guidance.

The lead accumulator is the main topic, but there is a preliminary chapter on the Leclanché and dry cells, and an interesting description of the alkaline (Ni-Fe) accumulator. A section on Service Station Repairs is included, and brief descriptions of rectifiers for use when charging from the A.C. mains. Very few inaccuracies mar the book, but future editions will be improved by redrawing Figs. 3, 43 and 44, and by tightening up one or two loose statements, such as "one ampere is the rate of flow of current . . .", "an atom cannot exist as such in the free state."

The binding and printing are good, and there is a useful index. The book is adapted from *Piles et Accumulateurs Électriques* by the same author.

F. A. V.

Alternating Current Electrical Engineering. By PHILIP KEMP, M.Sc.Tech., M.I.E.E., A.I.Mech.E., Mem.A.I.E.E. Fifth edition. [Pp. x + 611, with 421 figures and 1 folding plate.] (London: Macmillan & Co., Ltd., 1937. 15s.)

WHEN a text-book reaches its sixth edition there is no need for a reviewer to say that it is an excellent book or that it fills a need. Rather should he discuss what qualities make this particular book stand out above the average. The one under review has been written by a teacher who knows exactly what ground should be covered by a Higher National Certificate course and, what is more important, he also knows how the mind of the average student works; consequently the order in which the material is presented is that which can most usefully be followed by a lecturer and that which will most easily lead the student on from section to section. From long teaching experience the author has evidently come to know exactly where students need most assistance, and where, when and how they are liable to make mistakes. On such occasions he has inserted just that word or two of extra elucidation, or correction of a possible misconception, which can be so helpful to a groping novice.

Up to nearly the end only the simplest mathematics is used, and consequently much has to be treated rather superficially; where however it is impossible to do without elementary calculus, proofs are given in footnotes. This is done, for instance, to prove that the effective value of a sinusoidal

current is the peak value divided by root two. We cannot help thinking that it is impossible to give students much real understanding of Electrical Engineering when they have been provided with no better equipment than those for whom this book caters. This is, however, a reflection on our educational system and not on the book under review.

The book covers fundamental principles, measuring instruments, design of all kinds of electrical machinery and apparatus, protection, and symbolic notation ; after all this, a couple of chapters follow which seem to us quite outside the framework of the rest of the book. They deal with transients and the oscillatory circuit and, of course, cannot do without some higher mathematics ; which though elementary do involve at least second order differential equations. We wonder what benefit can be derived by a student for whom in the rest of the book the simplest integration has to be squeezed into a footnote.

The material is thoroughly up to date and includes such (for teaching purposes) recent subjects as valve voltmeters, cathode ray oscillographs, cascade connected transformers and symmetrical components. The latter subject is, however, brought before the chapter on Symbolic notation and is therefore treated geometrically only and, in our opinion, far too superficially.

Standard notation has been observed consistently in most things, but not in the method of presenting a vector quantity. However, here the author seems in good company, for it is rare indeed to see the standard method followed. There are nearly as many different methods as authors, and it is high time that this chaos was abolished, and books which run into five editions ought to take the lead.

REGINALD O. KAPP.

The German Patent Position of Arc Welding Electrodes : Coverings, Welding Rods and Fluxes. By WERNER H. SIMON, Dipl.Ing., A.M.Inst.W. [Pp. 80.] (Obtainable from the Author, 50 Edgedale Road, Sheffield 7. £2 10*s*. net.)

THIS book consists of 80 mimeographed pages. Approximately one-third consists of a tabulation of German Patents classified in three different ways, and the remainder of the work is a record of the main claims and dates of these individual patent specifications.

This book is merely a catalogue of 116 granted patents (half of which have been allowed to expire) and of 24 recent applications. There is no critical discussion of any kind. Technically, it is difficult to see what this compilation has achieved. Perhaps it will suffice to say that if the reviewer had to consult the German patent literature he would hesitate to spend £2 10*s*. on a volume that was read in one hour.

H. H.

CHEMISTRY

A Comprehensive Treatise on Inorganic and Theoretical Chemistry. Vol. XVI. By J. W. MELLOR, D.Sc., F.R.S. [Pp. x + 811, with 94 figures.] (London, New York, Toronto : Longmans, Green & Co., 1937. 63*s*. net.)

THE publication of this volume, the last of Dr. Mellor's Treatise, is a notable event in the history of British Chemistry. The last great treatise on chemistry written in England was Roscoe and Schorlemmer, and for many years it has been thought that each branch of the subject had grown so vast that here-

after we should have to be content with treatises produced by co-operative effort with all the disadvantages of differences of style and outlook.

Dr. Mellor has proved that there are still giants in the world, and has completed a task which should rank with Gibbon's *Decline and Fall of the Roman Empire* as one of the outstanding monuments of the genius and industry of a single man. The work is far from a mere compilation, and anyone who reads it cannot but be struck by the learning which has enabled the author to grasp the essentials of the vast mass of literature consulted and the literary skill with which he has put the results of his research before us. Praise must also be given to those responsible for reading the proofs, Messrs. L. S. Theobald, A. T. Green and F. H. Clews, to the printers, Messrs. William Clowes & Sons, Ltd., and to the publishers, for the freedom from misprints and the excellent get up of the work; all must feel proud to have been associated with so noteworthy an achievement.

As it is fifteen years since the first volume was published, it is intended to issue two supplementary volumes to bring the subject up to date, and a special volume is to be prepared to deal with a number of compounds on the borderline between organic and inorganic chemistry.

The present number deals with platinum and contains a general index of the whole sixteen volumes compiled by Miss E. M. Rigby and containing some 50,000 entries. In many ways platinum is an interesting element and always retains some of the romance associated with it by the chemist in his younger days. What youngster could fail to be impressed by its unusual properties and by the fact that it is the only precious metal commonly handled in the laboratory? Dr. Mellor has not failed to bring out this side of the story of the element in his customary interesting fashion in addition to giving us the usual mass of accurate information we now expect from him. There is little one can criticise in the book. The author, however, is not happy in his application of the electron theory to the structure of platinammines, but even Homer nods sometimes.

O. L. B.

Physikalische Methoden im chemischen Laboratorium. [Pp. viii + 267, with 89 figures.] (Berlin: Verlag Chemie, G.m.b.H.. 1937. Reduced price abroad RM. 2.70.)

THIS compact little volume contains a series of articles, which have already appeared in the *Zeitschrift für angewandte Chemie*, dealing with a large variety of technique. There is not any designed connection between the several topics, except that the methods are likely to be employed in any physical chemistry or analytical laboratory. The first article on X-rays by R. Brill and F. Halle is rather more descriptive of the recent results of X-ray analysis of materials, such as natural polymers—proteins and cellulose, the Fourier analysis of the crystal structure of organic compounds, than of the methods which might be applied to problems awaiting solution. Next comes an account by G. Schmid of the production of supersonic vibrations and their chemical effects, such as the acoustic excitation of vibrational energy in molecules and the measurement of the compressibility of liquids. While it is naturally difficult to deal adequately with the subject in the space of 30 pages, a fuller description of the construction of piezoelectric supersonic generators would have been more welcome in a book of this nature. The three following articles by G. Hesse, G. M. Schwab and K. Jockers, and G. M. Schwab and G. Dattler consider in a very practical manner

the identification and quantitative estimation of inorganic and organic substances by the colours to which they give rise on selective adsorption by solids (chromatographic analysis). D. Dadieu in the next section on the Raman effect in chemistry describes briefly the contribution made to the determination of molecular structure as far as this method will go. L. Rohde, P. Wuff and H. Schwindt describe with practical detail the physico-chemical implications of the accurate measurement of dielectric losses, while G. Scheibe and A. Rivas give an account of a micro method for the quantitative estimation of constituents of an alloy by emission spectra. A short article follows by E. Badum and K. Leilich with particular reference to antimony and tin in lead. The two articles by A. Winkel and G. Proske and by G. Maassen on polarographic methods of analysis are well written from the experimental point of view with full descriptions of the apparatus employed. The second article is devoted to the determination of copper, nickel and cobalt in steel. Likewise the article on photoelectric photometry is useful, as it contains a full account of such work. The application to the colorimetry of colloidal solutions is described by Juza and Langheim.

At the modest price of RM. 2.70 this helpful little volume ought to find a place on the bookshelves of many physical and analytical chemists.

H. W. Melville.

The Properties and Functions of Membranes, Natural and Artificial. A General Discussion held by the Faraday Society, April 1937. [Pp. 240, with numerous figures.] (London: Gurney & Jackson, for the Society, 1937. 12*s*. 6*d*. net.)

This volume is a record of the fifth discussion arranged by the Colloid Committee of the Faraday Society. On this committee are representatives of the Biochemical and Physiological Societies. In consequence the papers contributed to the discussion are predominantly of a biological nature. The first and greater part of the discussion is devoted to natural membranes of plants and animals. Here are papers on the structure, chemical and physical behaviour of such membranes. The second part will appeal much more to the physical chemist. An introductory paper by K. H. Meyer summarises the present theories of the permeability of artificial membranes. Several similar papers follow. There is a thorough account by G. S. Adair of the Donnan theory and its various ramifications. A possible method of bridging the gap between these two parts of the subject has been started by the study of the structure of protein monolayers by J. S. Mitchell.

H. W. Melville.

Systematic Organic Chemistry: Modern Methods of Preparation and Estimation. By William M. Cumming, D.Sc., F.I.C., I. Vance Hopper, Ph.D., A.R.C.Sc.I., F.I.C., and T. Sherlock Wheeler, Ph.D., B.Sc., F.R.C.Sc.I., F.I.C. Third edition, revised by W. M. Cumming and I. V. Hopper. [Pp. xxvi + 547, with 85 figures.] (London: Constable & Co., Ltd., 1937. 25*s*. net.)

The appearance of a third and revised edition of this standard text-book is welcome. There has been no alteration in the general arrangement and the excellent practice of annotating the practical instructions with theoretical comments has been maintained. Many of the methods have been brought up to date and there are frequent references to recent literature. The sections on quantitative analyses for carbon, hydrogen and nitrogen have

been re-written and a detailed description is given of the analyses in the "hemi-macro" (centigram) scale by Roger and McKay's methods. In this section the authors have had the collaboration of Dr. R. Roger. The emphasis placed on this scale of analysis is well-timed, because it appears fairly certain that quantitative micro-methods, although invaluable for professional analysts and specialists, are too delicate to replace macro-analyses for less-skilful workers, and are quite unsuitable for undergraduate classes. Analyses on the centigram scale seem to offer a reasonable compromise.

A few minor suggestions may be made. Many of the abbreviations used in the very full references to the original literature have an unfamiliar appearance. It would be convenient if these were brought into line with those officially used by the Chemical Society. A few formulæ of aromatic or heterocyclic compounds (as on pp. 167 and 237) have some but not all of the "double bonds" put in: this may confuse the less-experienced reader. In view of the general use of catalytic hydrogenation over platinum or palladium to-day, the authors might provide some examples of this type of reduction.

The book can be warmly recommended.

R. P. L.

Organic Chemistry. By F. C. Whitmore. [Pp. x + 1080.] (New York: D. van Nostrand Co., Inc.; London: Chapman & Hall, Ltd., 1937. 40*s*. net.)

My first impression of this book was that it was merely a collection of facts such as it is now fashionable to condemn, but as I turned over the pages I continually discovered odd pieces of information which I am sure are not to be found all together in any other book—short statements about new manufacturing processes in particular. And after reading page after page of fact, I asked "Are there no theories?"—just as on reading some modern books I begin to wonder if their authors really prefer the shadow to the substance. This at any rate, Prof. Whitmore cannot be said to do. I think he set out to collect facts and he has achieved his object. He shows admirable restraint over the sterols and their innumerable cousins and he gives just a bird's-eye view of the alkaloids. Again, the author's purpose is clear. After all, mere facts can be comforting. The author's remark, quite unamplified, that "dehydroacetic acid is 2-hydroxy-6-methyl-3-acetyl-γ-pyrone" is a suitable epitaph for a substance which once occupied the public eye for far too long.

When Dr. Whitmore allows himself to discuss theory he is not always up-to-date. Thus he regards the Armstrong-Baeyer benzene formula as a good one; he gives a modern explanation of the action of alkali on *o*-bromonitrobenzene, but Fry's explanation of the action of the same reagent on *o*-dinitrobenzene. He gives the melting-points of the famous ten dichloronaphthalenes, but omits to mention the Mills-Nixon effect. His outlook on some parts of the subject is shown by his statement that the most important Friedel-Crafts reaction is the condensation of phthalic anhydride with benzene and that the most important use of phenol is for making bakelites.

One excellent feature of the book is the giving of numerous references to the *Annual Reports of the Chemical Society* and to *Organic Syntheses*. Another is the good index, which I commend to the notice of the quick critic before he gives his opinion of the work.

With regard to the distribution of material, aliphatic has 614 pp., alicyclic

75, aromatic 182 and heterocyclic 82 pages. I personally think the aliphatic section is disproportionately long, but Prof. Whitmore says he made it long "in keeping with the present trend towards aliphatic chemistry, especially in British and American industry."

If the book gives us something we should not write ourselves it is good for us, and opinions expressed by so prominent an American chemist are ones we want to hear.

I think industrial and borderline chemists will welcome the book outright for its sheer information and I think that had it not been quite so expensive it would have found a place on the bookshelves of many who, had they written it, would have done it quite differently.

E. E. TURNER.

Modern Theories of Organic Chemistry. By H. B. WATSON, D.Sc., F.I.C. [Pp. viii + 218, with 21 figures.] (Oxford: at the Clarendon Press; London: Humphrey Milford, 1937. 15s. net.)

THE author in this book has undertaken a difficult task and achieved no small measure of success. His object has been to present a view of organic chemical reactions in the light of the electron theory which will appeal to those who are not specialists and will aid the student preparing for an honours degree to understand these modern developments.

After a review of the electron theory of valency and of new physical methods of investigation of the structure and behaviour of organic compounds, the applications of the electron theory to such problems as the strength of organic acids, substitution and addition reactions, tautomeric change, migration of groups and reactions velocity, are discussed. The exposition is notably clear and readable and the only criticism one can make is that, in his expressed desire to avoid overmuch detail, the author, by confining himself mainly to examples that work satisfactorily, has conveyed the impression that the electron theory explains everything with the greatest ease. Perhaps this is most obvious in the chapter on addition to unsaturated compounds where the difficulties are not sufficiently indicated. If, however, the reader realises that the modern theories are still far from giving a complete interpretation of all the complicated experimental data of organic chemistry he will find the present work a valuable account of what is now an important aspect of organic chemistry.

O. L. B.

Toxicity of Industrial Organic Solvents. By ETHEL BROWNING, M.D. Medical Research Council, Industrial Health Research Board, Report No. 80. [Pp. iv + 396.] (London: H.M. Stationery Office, 1937. 7s. 6s. net.)

A VERY large and always increasing number of volatile organic compounds is now employed, mainly for solvent purposes, in the arts, in industry, and even in the domestic household. The possible lethal effects of the vapours of these substances therefore require careful consideration. The Committee on the toxicity of industrial solvents set up by the Medical Research Council has pointed out in the preface to the present monograph that "what is not well known is which of these many compounds might be suspected before use as being dangerous—and which are, in fact, harmless. Moreover, a substance which causes acute toxic symptoms when breathed in high concentration may be suspected of having some injurious effect on health when

inhaled in a less concentrated form over a long period: whether this occurs, and if so what organs of the body may be specially affected by such chronic exposure, is not known." As a preliminary to undertaking investigation upon the possible effect of such substances, it was wisely considered desirable to collate the existing knowledge of the subject, which is scattered in the form of communications to a host of scientific and medical journals. This task was entrusted to Dr. Browning, and the monograph under review is the result of her work.

The Committee disclaim any pretension that it is an exhaustive record of the published knowledge of the subject—a precaution which seems to err, if at all, on the side of modesty, since perusal of the book leaves the impression that no important contribution is likely to have escaped the careful attention which the author has clearly given to all possible sources in the literature. Almost every known organic solvent will be found discussed under the successive groups of hydrocarbons, chloro-compounds, alcohols, esters, cyclohexanols and their esters, ketones, ethylene and related glycols, or "miscellaneous." The only seeming omission which has occurred to the reviewer is that of certain chloro-fluoro-methane derivatives which, it is understood, are coming into use as refrigerants.

A work of this kind is difficult to review, since it represents a synthesis of a vast quantity of detailed information upon the toxic properties of so wide a variety of substances. The method adopted is to give the constitution, properties and uses of each compound, and then to present a concise résumé of the published data as to its toxic effects in animals and in human beings. The discussion of each compound is followed by full bibliographical references to the literature. The treatment appears to be comprehensive and, at the same time, the relevant data are presented concisely and discussed critically. The impression given by the monograph is that it will not only serve its primary purpose as an aid to further research, but that in itself it forms a detailed scientific survey of the subject, as revealed in the literature at the end of 1935, which must be of the greatest value to those concerned (whether as factory executives or inspectors) with the use of these solvents and with the care of the health of workers who may be exposed to their vapours. The literary style is excellent, and the book is furnished with a good index.

T. P. Hilditch.

A Text-Book of Qualitative Chemical Analysis. By Arthur I. Vogel, D.Sc., D.I.C., F.I.C. [Pp. xii + 383, with 14 figures.] (London, New York, Toronto: Longmans, Green & Co., 1937. 7*s.* 6*d.*)

Writers on practical chemistry are always faced with the difficult problem of deciding whether or to what extent contemporary ideas as to what constitutes sound teaching, new or improved technique or current theoretical conceptions should supplant or supplement traditional practice. Nowhere is this difficulty so patent as in text-books of qualitative analysis where—ignoring the output of the more obviously plagiarised texts—some recent authors have championed spot-tests or semi-micro methods to the exclusion of the classical methods elaborated during the last century, whilst others have pleaded for a better understanding of the underlying physical principles or have advocated the abandonment of set "tables of separations."

Dr. Vogel is to be congratulated upon his solution to these problems.

He has selected carefully but extensively from the wealth of new material and incorporated it into the well-tried traditional schemes in such a satisfactory manner that the fresh material appears not so much as supplementary but rather as logically complementary to the old.

A pleasing feature is the giving of reasonable alternative methods of procedure where most text-books quote only the one which appears to appeal most to the author. For example, several possible treatments of Group II in the presence of oxidising agents are discussed, and five methods are critically examined for the treatment of Group III when phosphates are present. Among them is a good account of the still too little known formate-buffer method of T. B. Smith. The significance of pH in analytical work is reasonably stressed: cresyl blue is, for example, recommended for use as an indicator for adjusting the acidity before precipitating the Group II sulphides.

The increasing use of tungsten, molybdenum, vanadium and beryllium in the paint industry is rapidly erasing the former distinction between the common and the "rarer" elements. Dr. Vogel recognises this trend and includes a chapter dealing with their analytical reactions, together with those of thallium, gold, platinum, selenium and tellurium, thorium and cerium.

From the first 90 pages dealing with the theoretical aspects of qualitative analysis to the Tables in the Appendix, this book is packed with readily accessible information. Clearly printed, and with few serious typographical errors, it should more than satisfy the demands of the sixth-former and scholarship candidate, yet will be invaluable to the student preparing for examination by one of the professional bodies or at a University.

H. Irving.

Analytical Chemistry. Vol. I: Qualitative Analysis. By F. P. Treadwell and William T. Hall. Ninth English edition. [Pp. x + 630, with 18 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1937. 22*s.* 6*d.* net.)

Many changes have been made in the new edition of Treadwell and Hall's *Qualitative Analysis.* Part I (theoretical) has been largely re-written to bring the subject-matter more into line with modern theory. Part II (reactions of cations) has been re-arranged so that the order is now that of the analytical groups, instead of the reverse as in the previous edition. This is a welcome change, the old arrangement having obvious disadvantages. Several changes are noted in the rest of the book. A section on semi-microanalysis has been introduced and some sections of the former text discarded or shortened, the size and price of the book thus remaining unchanged. Only one scheme for the analysis of cations is now given, which simplifies matters for the beginner, and the scheme of Noyes and Bray for the analysis of rare and common metals has been omitted. This scheme, though excellent, is lengthy and not in general use, so the authors refer readers to the original text, where full details of the tests and manipulations are to be found. The section on spectroscopy has been shortened. Owing to the extensive developments in recent years it is now beyond the scope of a general text-book to give an adequate treatment of this branch of analysis.

These changes have simplified and improved the presentation of the subject-matter and Treadwell and Hall remains the standard text-book on Qualitative Analysis. Any student who possesses this and the companion volume on Quantitative Analysis is completely equipped for an honours course in practical inorganic chemistry.

J. N. S.

Qualitative Inorganic Analysis. By A. J. BERRY, M.A. [Pp. viii + 147.] (Cambridge: at the University Press, 1938. 6*s*. net.)

THIS book, which has been written primarily in the interests of the Author's own pupils, departs in two directions from established custom. The author maintains, firstly, that the ancient and arbitrary distinction between the common and some of the so-called rare elements can no longer be maintained, since many of the latter are reasonably cheap and of technical and scientific importance, and, secondly, that the extension of drop-reaction technique and the use of new reagents is rapidly developing. The book was therefore written with the excellent idea of incorporating these two items in a practical text-book, of not too advanced a character, and it is to be regretted that the usefulness of the book has been impaired by some lack of attention to practical details. The author has abandoned the traditional tabular method of presentation since "students are apt to place too much reliance on analytical tables," but the method adopted does not appear to be specially helpful to beginners. There are also cases of incomplete or misleading instructions and some important omissions. The following may be quoted: (i) the student is not reminded of the importance of dilution when precipitating Group II with hydrogen sulphide (p. 20), although he is advised to pass the gas for ten minutes; (ii) in Group III A he is instructed to test the alkaline liquid for chromate with lead acetate (p. 123), although lead chromate is "readily soluble in sodium hydroxide" (p. 31); (iii) the precipitate obtained in the Group III B separation (which is called Group IV in this book) will not "probably be manganous hydroxide" (p. 127), but certainly zinc sulphide if these instructions are followed; (iv) the possibility that the separation of Group III A from III B (the author's Group IV) may be incomplete is mentioned (p. 124), but the procedure to be followed when this happens is not indicated; (v) the important test, nitrate in presence of bromide or iodide, is omitted from the "mixed acids" section of Chapter VI; (vi) pp. 19–21 are misplaced and ought immediately to precede p. 119.

It is obvious therefore, that although the book has been written from a novel and interesting point of view, there is scope for revision and improvement in matter of detail, especially if the peculiar difficulties of the beginner are borne in mind. The type is very pleasing but the binding is rather unsubstantial.

J. N. S.

A Scheme of Inorganic Qualitative Analysis. By E. M. STODDART, B.Sc., Ph.D. [Pp. viii + 39.] (London: William Heinemann Ltd., 1937. 1*s*. 6*d*.)

THIS little book is essentially a set of analytical tables and tests for use at the working bench. It is assumed that students using it will be under the supervision of a teacher, consequently precise details for carrying out the tests are not given and all theoretical matter (even equations) has been omitted. Within these limits the author has written a satisfactory booklet which will serve the student as far as the Intermediate Science Examination or a little beyond. The book is pleasantly produced, is very free from slips (a 3 is omitted from the formula $BaSO_3$ on p. 27, and bicarbonates give a precipitate with magnesium sulphate *on boiling*, p. 23) and it has the advantage of being published at a very modest price.

J. N. S.

Chemical Analysis of Metals and Alloys. By E. GREGORY, Ph.D., M.Sc., and W. W. STEVENSON. With a foreword by DR. T. SWINDEN. [Pp. xvi + 375, with 51 figures.] (London and Glasgow: Blackie & Son, Ltd., 1937. 15*s.* net.)

WITH the increasing complexity of alloys a thorough knowledge of the fundamental reactions underlying analytical methods is becoming a necessity. This book, which approaches the subject from that point of view, will prove invaluable to those engaged in routine analytical work as well as to students and teachers.

Chapter I is devoted to fundamental chemical principles and covers the ground simply and clearly, with frequent reference to familiar analytical reactions. It may be questioned, in passing, whether it is justifiable to give the classical picture of the atom, in terms of electrons and protons, without qualification. In Chapter II the chemical properties of the elements are set forth and the reactions upon which the analytical methods are based are systematically described. This section has been admirably done and all the more reliable methods based upon the use of organic reagents are included. These two chapters constitute the distinctive part of the book.

Chapter III, "Preliminary Operations and Considerations," might with advantage have been extended, and gives the impression of miscellaneous notes rather hastily thrown together. The fact that the authors clearly are in a position to give many valuable hints as to procedure makes us regret that this ground is not more systematically covered.

The rest of the book is devoted to analytical methods, mainly for ferrous materials, including slags and ores. Here the authors have added little to what is already available in previous text-books, but have covered the ground thoroughly. The final section on non-ferrous alloys is so scanty (23 pp.) as to be of very little value, and hardly justifies the inclusive title given to the book.

The book is admirably produced and durably bound. The errors observed are mainly obvious misprints, etc., and comparatively few in number.

S. J. KENNETT.

The Structure of Steel Simply Explained. By ERIC N. SIMONS and EDWIN GREGORY, Ph.D., M.Sc., F.I.C. [Pp. xii + 115, with 45 figures, including 7 plates.] (London and Glasgow: Blackie & Son, Ltd., 1938. 3*s.* 6*d.* net.)

THIS little book has been produced for the use of all who have to do with steel whether as user, student, layman or worker. There is an introduction by Dr. F. C. Lea, in which he points out the importance of a particular "structure" that steel should have in order to meet particular conditions. The matter is based on a series of previously published monthly articles on steel and its structure, written in simple language by E. N. Simons. These have been restated, amplified and corrected by Dr. Gregory, who has also added the final chapter on X-rays and their use in the examination of the atomic structure of metal and for the detection of internal flaws. The authors have set themselves a by no means simple task and have succeeded very well indeed, not only in describing and illustrating the different types of structure met with in steel, but also the methods of securing these and the necessity of having them for different purposes. There are certain points on which a little further enlightenment is desirable, but the information given on the freezing of iron-carbon alloys, critical points, impurities of steel, alloy steels, heat-

treatment, testing of steel and corrosion linked up with stainless steel is excellent and should prove of great value to many in whose interest the book has been produced and for whom the low price will make it readily available.

C. O. B.

Colorimetric Methods of Analysis. Vol. II: Organic and Biological. By FOSTER DEE SNELL, Ph.D., and CORNELIA T. SNELL, Ph.D. [Pp. xxiv + 815, with 49 figures.] (New York: D. van Nostrand Co., Inc.; London: Chapman & Hall, Ltd., 1937. 45*s*. net.)

THIS volume is the logical extension of the authors' well-known treatise on colorimetric methods of analysis in the inorganic field.

In the 52 chapters, methods are described for estimations of chemical, biological and commercial importance. Alternative methods are given full consideration and the most suitable method for a particular estimation is indicated.

The description of the method employed in each case includes precise details for making up the sample and the necessary standard solutions. A very valuable feature is the indication which is given of the degree of accuracy to be expected in any particular application.

The organic groups treated include hydrocarbons, alcohols, aldehydes, ketones, aliphatic and aromatic acids and their esters. The clinical and biochemical aspects are fully dealt with in chapters on the determination in blood, serum, urine and tissues of important compounds such as carbohydrates, sterols, bile acids, bilirubin, hæmoglobin, hormones and vitamins. In addition, there are chapters on enzymes and proteins and the determination of the many biologically important nitrogen compounds—including the alkaloids. Estimations involved in tests of renal and hepatic function have not been overlooked.

In the chapters on colour, an immense amount of information on miscellaneous methods of commercial importance has been gathered together. The range may be judged from the fact that the absorption of dye by fibres, the colour of soiled fabric and the hiding power of paint all find a place.

A complete index to subjects and authors is provided. The large number of difficultly accessible sources quoted in the copious references to the original literature is a measure of the reader's indebtedness to the authors.

It is inevitable in works of this type that some portions are superseded almost as rapidly as they are written. In the description of Zimmermann's colour reaction for androsterone, for example, it has not been possible to include any mention of the extensions and improvements effected by Wu and Chou, Oesting, and Callow. Kober's improvement of the original phenolsulphonic acid colour test for female hormone by the use of naphthol-sulphonic acid is also too recent for inclusion.

This book should prove invaluable—not only as a work of reference but as a constant source of inspiration in suggesting profitable approaches to present and future problems of colorimetric analysis.

J. W. C.

Principles of Chemical Engineering. By W. H. WALKER, W. K. LEWIS, W. H. MCADAMS and E. R. GILLILAND. Third edition. [Pp. xii + 749, with 228 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1937. 30*s*. net.)

WHAT may be regarded as text-books on this subject are all too rare, and

earlier editions of this well-known work by Professors of Chemical Engineering at the Massachusetts Institute of Technology have proved of great benefit, not only to chemical engineering students, but also to industrial chemists in enabling them to develop a better appreciation of the processes which they are called upon to operate and control. It is with regret that one reads in the preface to the new edition of the death of Dr. Walker, and it was necessary for the subject matter to be revised and re-written without his collaboration. Nevertheless, the present edition well maintains the high standard of its predecessors.

It will be generally agreed that the practice of chemical engineering is becoming more and more complex. There is greater necessity than ever, therefore, for clarity in the presentation of fundamental concepts, and for the development of trustworthy methods based upon them for solving practical problems. The authors, have borne these points well in mind, and, in addition to setting out clearly the principles underlying chemical engineering operations, have provided plenty of illustrative problems of a practical character. A valuable feature is the inclusion at the end of the chapters of lists of references, which should prove extremely helpful to the reader in assisting him to extend his knowledge of the various branches of the subject.

The most extensive changes will be found in the chapters on Flow of Fluids, Flow of Heat, and in those involving diffusional processes. In all these cases the theoretical development of the subject has been clarified and emphasis laid upon detailed derivation of equations incorporated in the text. Thus, in Flow of Heat there is careful discussion of the mechanisms of convection and their relation to the more important correlations developed in recent years. The subject of mean temperature difference has received fuller treatment, as has that of radiation. A section on dimensional analysis has been added, and the utility of consistent units emphasised.

The chapters on Crushing and Grinding, Sedimentation, and Evaporation have been revised and brought up to date, that on Filtration has been re-written, and those on Absorption and Extraction, Distillation, and Air Conditioning have been modified in the light of the discussion on diffusion previously mentioned. The treatment of psychrometry is new and includes humidity charts based on recent data. The chapter on Drying gives important new data and developments of recent years, and is designed to make these more useful in the solution of practical problems.

The general arrangement and method of presentation of the subject matter are excellent, and the illustrations are admirably reproduced.

H. W. C.

The Principles and Practice of Lubrication: A Manual for Petroleum Technologists, Students, Engineers, Oil Salesmen, etc. By Alfred W. Nash, M.Sc., M.I.Mech.E., and A. R. Bowen, Ph.D., D.Sc., F.I.C., A.M.I.Chem.E. Second edition. [Pp. xii + 345, with 96 figures, including 26 plates and 2 folding plates.] (London: Chapman & Hall, Ltd., 1937. 18*s*. net.)

The authors have covered a vast field in a relatively small compass, but the result is not so satisfactory as would have been the case had they limited the circle of readers for whom the book was written. They deal with the physical and chemical examination of lubricants, the design and lubrication of bearings, the sources of lubricants and their chemistry, friction and friction testing

machines, industrial lubrication practice, and the storage and conservation of lubricants, but in every case the treatment is somewhat meagre on account of the limitations of space.

In consequence, an engineer who looks to this book for information on the lubrication of his plant will not find a great deal. A student, on the other hand, will find much that is new to him, and he may be encouraged to go more deeply into the subject, to which end the references given at the end of each chapter will be of assistance. They would be more helpful still if they were numbered so that references in the text could be traced easily.

The section in the chapter on the design and lubrication of bearings which deals with bearing metals badly needs further revision, and three references (one of which does not bear on anything mentioned in the chapter) is a scanty allowance for 48 pages of text.

The index is entirely inadequate for a book which is intended to be used, in part at least, for reference purposes, and is unworthy of a work over which the authors obviously have taken much trouble. Space for a fuller index could be found by omitting the Fahrenheit-Centigrade conversion table, and the table of the density of water between 4° C. and 100° C., neither of which is necessary in a book of this kind.

The printing and binding of the book are good and there are few printers' errors. The appearance of the pages is rather spoilt by the acknowledgment which appears beneath practically every illustration, and in the next edition these individual acknowledgments should be relegated to their proper place in a prefatory note.

The book can be thoroughly recommended to students and oil salesmen, but engineers and petroleum technologists will not find their needs so well met.

H. N. Bassett.

Modern Rubber Chemistry. By Harry Barron, Ph.D., B.Sc., A.I.C., A.I.R.I.(Sc.). [Pp. 342, with 70 figures.] (London: Hutchinson's Scientific & Technical Publications, 1937. 18*s*. net.)

This book is described as a comprehensive survey of the behaviour of rubber and latex in every phase of their commercial applications, but such a claim can hardly be justified; indeed, few commercial applications are described. At the present time there is a definite need for a modern book at a reasonable cost which can be placed in the hands of students and others interested in obtaining a fairly thorough knowledge of rubber technology. On reading this book one wonders for whom it was intended. For students it is unsuitable, as many chapters are too brief, the analogies are unnecessary, and the numerous references to original and sometimes little-known journals are useless. Incidentally, the abbreviations of the names of the journals quoted might at least have been restricted to those more generally accepted in the scientific press. For experts, the book does not contain sufficient detail to make any appeal. One can only imagine that the author has really written the book for those in the industry on either the manufacturing or commercial side who desire to acquire a general and superficial scientific knowledge of the basis of rubber manufacture. The book has been excellently printed and produced and no doubt a future edition will remedy certain errors in the text and in the diagrams, and will improve the English in many places.

T. J. D.

Polymerization and its Applications in the Fields of Rubber, Synthetic Resins, and Petroleum. By R. E. Burk, H. E. Thompson, A. J. Weith, and I. Williams. American Chemical Society Monograph Series. [Pp. 312, with 6 figures.] (New York: Reinhold Publishing Corporation; London: Chapman & Hall, Ltd., 1937. 37*s.* 6*d.* net.)

The authors state that their object is to collect facts and theories relative to the subject of polymerisation; and to arrange them in a way which will be interesting to workers in that field. It will be noted that they have not set out to discuss the facts in the light of general theory. This latter attempt would have been premature. For clearness, the authors have concentrated on certain fields—rubber, synthetic resins of the better-known types and polymerisation in the petroleum industry. Before these topics are discussed, there are chapters on the relation between molecular structure and the rate of polymerisation, catalysis and polymerisation, the mechanisms of polymerisation and one on the liquid state and the structure of polymers. In structure the book lacks homogeneity. It is rather like a plum cake with plenty of good plums in the form of the theoretical parts popped in among the facts. The lack of homogeneity is perhaps also in part due to the large number of authors. This criticism is particularly well illustrated by the chapter on the liquid state. There is still much work to be done before any profitable theoretical discussion can be given on the subject of polymers in the liquid state and of the viscosity of their solutions. This basic point is not made clear in the somewhat irrelevant and diffuse section. In spite of these criticisms or rather warnings, the authors have succeeded in doing what they set out to do. The book is an interesting commentary on this important field and can be read with profit by research workers, both academic and industrial.

A. S. C. L.

Synthetic Resins and Allied Plastics. Edited by R. S. Morrell, M.A., Sc.D., Ph.D., F.I.C., in collaboration with T. Hedley Barry, F.I.C., R. P. L. Britton and H. M. Langton, M.A., B.Sc. [Pp. xii + 417, with 41 figures.] (London: Oxford University Press, 1937. 21*s.* net.)

This book is avowedly an attempt to bring up to date the section on Synthetic Resins, first compiled in 1928 as part of *Natural and Synthetic Resins* by two of the present authors with A. A. Drummond. The intention is excellent, since many important developments have taken place in the last nine or ten years. Furthermore, any attempt to present in English a readable account of Plastics, as an auxiliary to Carleton Ellis' monumental work, must be of value.

The scope of the work is wide and covers, both theoretically and practically, all plastics of importance and many less well-known, while electrical testing and methods of identification are also treated at some length. An interesting section on synthetic rubbers is included in a chapter devoted to vinyl and acrylic resins. A bias towards materials utilisable in paints and varnishes is counterbalanced by fairly detailed accounts of moulding technique and plant used. The causes of resinification are extensively, if not lucidly, covered, but numerous references facilitate consultation of the original literature.

The several authors concerned appear to have written more or less inde-

pendently of each other, so that certain imperfections are caused. The planning is poor (for example, the section on p. 197 dealing with resins used for water treatment is out of place in a chapter on oil varnishes and enamels), there is a tendency to repetition (it is unnecessary, for instance, to give both the tables on pp. 52 and 282) and the general lay-out is muddled. Not only are misprints too numerous to be mentioned in detail, but so many formulæ and authors' names are wrongly given, that proof-reading would appear to have been a formality. Some passages, for example that on pp. 34–7 dealing with phenol and formaldehyde, contain so many inaccuracies as to be misleading. The actual style of writing not only leads at times to ambiguity, but reduces the pleasure of reading.

Despite its defects the book should prove valuable to most workers employed in the field of Plastics, either for reference purposes or for the many latent ideas contained in it. Students may, however, be repelled rather than encouraged by the unresolved complexities of the work.

N. J. L. M.

GEOLOGY

Ground Water. By C. F. Tolman. [Pp. xviii + 593, with 189 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1937. 36*s*. net.)

The problems connected with the circulation of the water in the rocks which form the surface of the Earth are naturally of great practical importance. The possibility of obtaining good water from the beds which can be reached by wells and bore-holes frequently determines the practicability of establishing human occupations in the area..

Many people will be indebted to Prof. Tolman for giving them in one text-book detailed information regarding the different aspects of the problems involved. Since, however, he states that the main portion of the book is based on the lecture course given at Stamford University, California, and at the University of Arizona, it is natural that great emphasis is given to the behaviour of ground water in those arid and semi-arid areas, and consequently nearly all the examples are taken from the U.S.A., and, apart from a few references to a German text-book or two, practically no mention is made of the large amount of information relating to ground water which has been obtained in Europe. Nevertheless, many readers in this country will be glad to have the theoretical considerations brought together in one text-book.

The arrangement of the book follows the natural course of dealing with small-scale details first, passing on to the larger questions and applying the details to them. There are useful definitions of porosity, permeability, etc., with experimental details given. A short chapter on soils, their classification and importance in ground-water studies, might with advantage have been rather larger. A couple of chapters deal with seepage and percolation in detail, mainly however through the examination of samples of incoherent materials, either in the laboratory or in the arid areas of California.

The study of the water table in rocks is divided into the water table in granular pervious material and that in fractures and solution openings in hard rocks. In this way the similarities and difference between the two types are emphasised.

It is unfortunate that there are so few maps of actual cases of water-table

levels, their seasonal fluctuations and the effects of pumping on water tables, moreover such maps as are given have no scale whatsoever and the contours shown, although numbered, give no indication of the unit used.

The chapter dealing with confined water, that is water in a water-bearing rock or "aquifer" sandwiched between two impervious confining formations or "aquicludes" contains much useful information and the question of the effect of compressibility and elasticity of the rocks is dealt with in some detail. It is claimed that under suitable conditions the reserve water may be squeezed out of the rocks, both permeable and impermeable, by compaction of the grains and that where there is only temporary loss of pressure by pumping the rocks may recover their former water content. Permanent compaction of the material with loss of water may lead to surface lowering and several cases in support of this are quoted.

The book ends with an account of the methods of drawing up ground-water inventories and a discussion of the major ground-water provinces in the U.S.A.

To British readers the work will be a valuable addition to the subject of ground water. Some may feel that there are so many disturbing geological factors when dealing with water in solid rocks that calculations based mainly on laboratory experiments on unconsolidated materials are of more theoretical than practical value.

This aspect no doubt is partly due to the difference between the semi-arid conditions of California and the more humid climate of this country where the percentage of thick unconsolidated alluvia is much less.

W. B. R. K.

Our Wandering Continents: An Hypothesis of Continental Drifting. By Alex. L. Du Toit, D.Sc. [Pp. xiv + 366, with 48 figures.] (Edinburgh and London: Oliver & Boyd, 1937. 18s. net.)

This is a bold attempt to reconstruct the history of the earth on the basis of Wegener's theory of continental drift. Wegener himself in conjunction with Köppen has already made a similar attempt, but Du Toit's reconstruction differs from theirs. Wegener imagined that all the masses of sial which form the present continents were originally united into a single sheet, with shallow seas upon its surface. Du Toit follows Staub in supposing that there were two independent sheets, Laurasia in the north and Gondwana in the south; but he has missed a great opportunity which that conception offers.

One of the strongest arguments in favour of the reconstructions of Wegener and Du Toit is that in the south they bring together regions which are now widely separated but which were once the home of the Glossopteris flora and the fauna associated with it. The force of this argument is greatly weakened by the fact that both flora and fauna occur in North Russia, far from the reconstituted Gondwana. Not only Glossopteris but also reptiles and freshwater shells very like those of South Africa are found there. Du Toit and others suggest migration; plants, reptiles and shells must have kept pace with one another for 6000 miles, including a sea-passage. If, however, Laurasia was an independent mass we may imagine that during the Glossopteris period its present northern coast lay against the southern coast of Gondwana, and North Russia was close to South Africa. Subsequently Laurasia travelled round the earth through the area now occupied by the Pacific Ocean—Antarctica was not then in the way—and finally reached its

present position, with its south border near the north border of Gondwana. It is a voyage of less than 19,000 miles, and according to modern estimates of geological time an average rate of a mile in two or three thousand years would have been ample. The supposed westerly drift of Greenland is much faster than this. For those who do not like either of these explanations the possibilities are not exhausted. The years since the war have been remarkably prolific in theories of the earth and there is now a wide choice.

It is seldom that Du Toit's imagination fails, but his reasoning is often far from convincing. He is so sure he is right that he forgets his reader. He is positive, for instance, that the movements in the Scottish Highlands are Taconian and not, as is usually thought probable, Caledonian. He gives no reason but even the least critical reader will see that he holds this view because it fits in better with his ideas. Like Wegener he selects facts that suit his theories and ignores those that do not; but he has not Wegener's skill in marshalling them. Wegener, right or wrong, is always interesting; Du Toit is so sometimes.

P. L.

Structural Evolution of Southern California. By R. D. Reed and J. S. Hollister. [Pp. xix + 157, with 9 plates and 57 figures.] (Tulsa, Oklahama: American Association of Petroleum Geologists; London: Thomas Murby & Co., 1936. \$2.00 or 9*s*.)

Of recent years the American Association of Petroleum Geologists has published a series of excellent memoirs designed to elucidate the stratigraphical and structural geology of oil-bearing regions in Western America. The latest of these volumes, under review, may be regarded as a sequel to the larger work on the Geology of California published in the same series a few years ago. Southern California was the last of the major geographical divisions of the State to be subjected to detailed geological examination, and that was in connection with prospecting for oil. Since 1920 nearly the whole of the southern region has been mapped in considerable detail, but publication has been sporadic and incomplete. Structural interpretation in Southern California is thus largely based on the work of oil geologists, and the adequacy of the data depends therefore on the attractions of the various districts for the prospectors. Thus the Tertiary areas are most fully and carefully mapped, Cretaceous areas less so, and Pre-Cretaceous areas hardly at all.

After a short introductory chapter stating the tectonic problems, the diastrophic history of the region is dealt with from Lower Mesozoic epeirogeny to Post-Pleistocene disturbances which are still proceeding with earthquake activity. Californian structural history is shown to have involved an alternation of widespread orogenic and epeirogenic phases which have affected all tectonically comparable areas. Chapters III to IX deal with the structures of various areas in more detail, and Chapter X provides a summary and conclusions, in which some theoretical points are treated, although the authors explain that they have been more interested in the geological history of the structures than in their mechanical causes.

The book is well illustrated by a number of clear, well-drawn diagrams, maps and sections, and with plates of photographs of scenery and structures. One of the latter is a fine aerial photograph showing the course of the great San Andreas fault. A well-executed geological map in nine colours, on the scale of 1 : 500,000, occupies a pocket.

G. W. T.

The Geology of South-Western Ecuador. By G. Sheppard, D.Sc., Ph.D., F.G.S. [Pp. xiv + 275, with 195 figures, including 14 plates.] (London: Thomas Murby & Co., 1937. 25s. net.)

This general description of the geology of south-western Ecuador is the first work on the geology of that state to be published in English, and supplements, with many new details, the monumental work of Dr. T. Wolf, the former State Geologist of the Republic. Much of the information dealing with this thinly populated and geographically difficult region has been afforded by the pioneer exploratory work of the Anglo-Ecuadorian Oilfields Co., to which full acknowledgments are rendered.

After an introductory chapter, Chapter II deals with Climate and Physiography, Chapter III with the Sedimentation of the Tertiary Formations, and Chapter IV with Stratigraphical Geology. Chapter V, on the Tertiary Larger Foraminifera of South-Western Ecuador, has been contributed by Dr. T. Wayland Vaughan. The succeeding three chapters deal respectively with Structural Geology, Cherts and Igneous Rocks, and Petroleum; and the volume closes with a comprehensive bibliography of the geology of Ecuador, and a full index.

The formations are all of Tertiary age, ranging from Lower Eocene to Pliocene and Pleistocene, with an unconformity between the Eocene and Oligocene. The author deals fully and successfully with the complicated problems of sedimentation and petrography in these highly variable sandstones and shales, which are regarded as having been formed under conditions ranging from shallow water, through inland lagoon, to estuarine and deltaic. The abnormal occurrence of huge boulders of Andean igneous rocks within the fine-grained sediments is explained as due to transport by slowly moving mud fans. The peculiar Ancon Clay Pebble Bed which, along with normal pebbles, contains polished, faceted, and occasionally rounded pebbles of hard clay embedded in a softer clay matrix, has been the subject of some controversy. While the stratum is primarily of clastic origin, considerable tectonic movement and thrusting has been concentrated within it, and has contributed to its peculiar features. Chert veins and aggregations within the Tertiary sediments are believed to be of hydrothermal origin, and to be connected with igneous episodes.

This book shows signs of acute observation and considerable originality in treating of the modes of formation of abnormal sediments, and is, moreover, a guide to the local geology, which will prove of value in any future exploration of the country. The illustrations are excellent, but the geological map which forms Fig. 1 is rather difficult to read.

G. W. T.

Economic Geology. By H. Ries, A.M., Ph.D. Seventh Edition. [Pp. viii + 720, with 63 plates and 267 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1937. 25s. net.)

The sixth edition of this work was published in 1930 and was reviewed in Vol. XXV, 1931, p. 713. In spite of the fact that the subject matter has been brought up to date, a number of changes made, new references added, and the chapter on Oil and Gas completely re-written, there has been a reduction in the size of the book by no fewer than 140 pages. This has been partly accomplished by a reduction in the space formerly devoted to statistics. There are some misprints and misspellings which have not been corrected

from previous editions. " P. Brun " (p. 359) should be " A. Brun " ; and " porphyry " is still misspelled no fewer than six times in Fig. 136 (p. 432). The references again betray insufficient knowledge or appreciation of non-American sources of information on economic geology. The chapters on Petroleum and Natural Gas, and on Ore Deposits, are particularly good, and the book maintains its position as one of our best texts on the subject of economic geology.

G. W. T.

Gold Deposits of the World. With a Section on Prospecting. By W. H. Emmons. [Pp. vii + 562, with 332 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1937. 36s. net.)

This book presents accounts of the geology of the principal gold-mining regions of the world, and of many of the smaller fields. As explained in the Preface, the treatment is chiefly descriptive, empirical and statistical, with reviews of the hypotheses that have been put forward to explain the geological relations of the particular fields. Nevertheless, in the introductory chapter, the author propounds his own general theory of the origin of gold deposits. He believes that they are formed by precipitation from fluids given off by cooling batholiths mostly of granitic and granodioritic composition. The veins are located in definite positions with respect to various parts of the batholithic structures, and especially the roofs and hoods of the intrusions. " Hood " appears to be a new term introduced to indicate the thin upper layer of a batholith which contains metalliferous veins ; below it is the " barren core," and the irregular surface which marks off the hood from the barren core is called the " dead line."

The introductory chapter in which the origin of gold deposits and their connection with batholiths is discussed is a distinct contribution to the subject of igneous ore deposits, and contains several illuminating diagrams. The implications of the above theory of origin provide data which may be of use in searching for gold, and the final chapter of the book deals with prospecting from this point of view, although it contains nothing on the mechanical technique of prospecting.

The remainder of the book deals with the gold deposits of the world on a geographical basis. As the author is an American professor, North America is naturally done in most detail, occupying 218 pages. Australasia and Africa come next with 89 and 65 pages respectively, while Asia with 51 pages, South America with 48 pages, and Europe with 31 pages, bring up the rear, which is, of course, only very roughly in the order of productiveness. The descriptions are written in the barest of detail consonant with clarity, and without critical comment, so that the work takes on the character of a compendium of the world's gold deposits.

The value of the work is greatly enhanced by numerous clear and simple diagrams, maps and sections, all done in a standardised style. In a work of worldwide scope such as this mistakes in the spelling of unfamiliar place names can hardly be avoided, as for example, among others, " Mount Pelee " (p. 17) and " Marionethshire " (p. 308). An excellent index has been provided.

G. W. T.

BOTANY

Introduction to Plant Pathology. By F. D. HEALD, M.S., Ph.D. McGraw-Hill Publications in the Agricultural Sciences. [Pp. xii + 579, with 200 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1937. 24*s.* net.)

THOSE who are familiar with this author's *Manual of Plant Diseases* will not need to be reassured as to the excellence of this Introduction. It is indeed a somewhat briefer treatment of the same material as the *Manual*, rearranged and brought up to date and with some shifting of the emphasis laid on certain parts, and there are some additional chapters, *e.g.*, on Control, but it is essentially the same book covering practically the same ground. The attempt is made to give a view of the relation of plant diseases to human affairs in general and not merely as independent phenomena designed to puzzle the examinee. It is divided into six sections. The first is Introductory, and covers the general considerations, such as historical development, definitions, symptoms, dissemination, etc., and this is followed by eleven chapters on parasitic diseases classified according to the systematic position of the organisms causing them, *e.g.*, there is a chapter given to ascomycetes, three to basidiomycetes and so on. The difficulty of classifying virus diseases is got over by creating a special section of two chapters, and this is followed by a three-chapter section on non-parasitic diseases. There is a section on control or prevention, and finally one on methods, which might well have been omitted: it is not really possible to deal adequately in 22 pages with the methods necessary in studying parasitic, virus and non-parasitic disease.

The information given is comprehensive and trustworthy, and the whole subject is covered satisfactorily and adequately. There are, of course, points where other opinions might be held than those advanced here: the Actinomyces scabies, for example, is removed from the position it held in the *Manual* as a bacterial disease and classified now under diseases due to imperfect fungi; and the yearning for systematic orderliness characteristic of the author's countrymen leads to groupings which sometimes seem rather forced. But on the whole this is a thoroughly sound book, useful to teacher and student, with a range that is not so conveniently covered in any other book of its size.

J. HENDERSON SMITH.

The Structure and Development of the Fungi. By H. C. I. GWYNNE-VAUGHAN, G.B.E., LL.D., D.Sc., F.L.S., and B. BARNES, D.Sc., Ph.D., F.L.S. Second edition. [Pp. xvi + 449, with frontispiece and 309 figures.] (Cambridge: at the University Press, 1937. 18*s.* net.)

THE new edition of this well-known text-book follows the general lines of its predecessor and will be welcomed by the mycological student as incorporating much of the significant recent literature. This has involved a substantial increase in size, an increase, moreover, which is fairly uniformly distributed between the various chapters and which is well illustrated by the lengthening of the index of references—from 30 pp. in the earlier edition to over 40 in the one under review.

To deal first with a major point, one can suggest that this book suffers from the defects of its merits. In view of the senior author's eminence as a student of fungal cytology, one is not surprised to find that especial emphasis

is laid upon cytological behaviour, and that in particular certain groups of the Ascomycetes receive rather detailed treatment. Broadly, the amount of text allocated to any one fungus is proportional to what has been written about its nuclear history. One may fairly ask whether this is a reasonable basis of presentation, more particularly with reference to the needs of the general student. Cytology is, of course, detailed structure, and cytological examination is necessary for the determination of those critical transition stages—from haploid phase to diploid and *vice versa*—which are accepted as being of fundamental importance throughout the plant kingdom. The broad results of cytological study are thus all-important, but one is tempted to ask what value is to be attached to much of the detail of fungal cytology. Though there is room for some difference of opinion on this point, it is indisputable that many of the details are as yet incapable of interpretation. The student either ignores them or, if a prospective examinee, commits them laboriously to memory. And this raises the greatest objection, in the reviewer's opinion, to the cytological bias in mycological presentation. The student must of necessity take so much of the information on trust. Only in a few laboratories has the student access to a reasonable number of good preparations, and it is doubtful if, in any laboratory, he has either the time or the skill to make any useful ones of his own. If then it is considered important that training in general mycology should involve the handling of material, the cytological approach introduces severe limitations. Judged on this basis, therefore, *The Structure and Development of the Fungi* is not a well-proportioned text-book for the general student, excellent guide though it may be for the advanced worker who is interested in cytological aspects of fungi.

The plant pathologist cannot justifiably complain because plant diseases are merely alluded to in passing, since the authors set out to describe fungi and not the diseases produced by them. But it is surprising to find that the large and important group of the Fungi Imperfecti are dismissed in two pages. Many of these are extremely common and important in their human relationships. The student is bound to meet them over and over again if he collects microfungi and more especially if he does cultural work. Though they are held to be devoid of sexuality, they show at any rate a great variety of structural features, and they offer a perennial problem to the expert in classification. One is therefore struck by the contrast on noting that, for example, seven pages are allotted to the Rhizidiaceæ, a group of water fungi, many of which have been recorded only once. Very few students, or possibly even teachers, have seen one of them. They are very inadequately studied and at most are of speculative interest only. In the circumstances it does not seem unreasonable to suggest that two pages of text is a very meagre allowance for the Fungi Imperfecti.

Here and there one notes statements which are loosely expressed or even incorrect. Thus cross-connections between hyphæ are said to be "nutritive in origin." They may be so in function, but they cannot be so in origin. Then "wood-destroying fungi flourish equally well on living and dead tissues." Much evidence could be quoted against this statement. More significant, as likely to lead to misunderstanding, is the definition of the words "hetero-" and "homothallism" as given on p. 6. According to this definition all fungi which have more than one strain are to be called heterothallic. This term would thus include nearly every fungus of which the cultural characters have been studied. The further discussion of heterothallism on p. 40 is obscure in parts (the wording of the first sentence, for example,

is unfortunate), but it goes on to fix that wider use of the word which has become customary of late but which is different from its original use by Blakeslee. The heterothallism of Mucors is not comparable to that of certain Ascomycetes, and the common terminology leads to loose thinking. The difficulty is not met by the introduction of the terms "monoecious" and "dioecious," for these terms should be restricted to conditions seen in sporophytic plants. Perhaps clarification of the terminology is not possible at the moment and must await a fuller understanding of the physiological processes involved.

The text-figures, and especially that large number of them which relate to cytological matters, are admirably executed. There is, as there was in the earlier edition, an excellently written account of the physiology of the fungi —given gratis, so to speak, since there is no suggestion of it in the title of the book—and there is a most useful appendix on mycological technique.

W. B.

A Textbook of Plant Virus Diseases. By Kenneth M. Smith, D.Sc., Ph.D. [Pp. x + 615, with frontispiece and 101 figures, including 1 coloured plate.] (London: J. & A. Churchill, Ltd., 1937. 21*s*. net.)

This is a sort of expanded and annotated bibliography, in which is briefly and clearly summarised most of the information available on all the recognised (and many suspected) diseases and the viruses causing them. As such, it is an exceedingly useful compilation, meeting a felt want. It brings together in convenient form data which are recorded in several languages in many journals, some of them difficult to procure, and it will save the specialist many hours of rather irritating labour. It includes a detailed account of the insects concerned in transmission, and an appendix which summarises under each host plant the chief symptoms of the various diseases found upon it. The illustrations are well-selected, helpful and numerous.

For such a book some system of orderly arrangement is of course essential, and the system adopted is a compromise between a classification by diseases and a classification by the viruses themselves. This is admittedly a difficult and controversial matter, which has been and still is under consideration by a special international committee under the presidency of Prof. James Johnson, who submitted a provisional scheme at the last Botanical Conference. Johnson's scheme has been adopted as a basis in this book, but it seems particularly unfortunate that the writer has in many cases departed from that scheme and assigned to the viruses numbers different from those given by Johnson. For example, Johnson's potato viruses 16 and 20 are here numbered 1 and 2. These two are the most important of the potato viruses and may perhaps claim priority of rank. But the principle of numbering viruses according to their economic significance must lead to eventual renumbering and consequent confusion in the case of newly discovered diseases in hosts less thoroughly studied than the potato. The author, however, hopes that "the book will lessen the confusion now existing in the study of plant virus diseases." There is no discussion of several important subjects, *e.g.* immunity, which one would expect to find in a text-book, and the title seems to be misleading in some respects; but the book will deservedly find a place on the shelves of every worker on plant viruses.

J. H. S.

An Introduction to Economic Botany. By JAMES GILLESPIE, B.Sc. The New People's Library, Vol. III. [Pp. 96, with 7 figures.] (London: Victor Gollancz, Ltd., 1937. 1*s*. paper, 1*s*. 6*d*. cloth.)

THIS is an informative little book. "Economic botany" may be held to cover an immense field and, limited by less than one hundred small pages, the author has selected as subjects for a series of chapters or essays aspects of plant breeding, fungus diseases, virus diseases, climatic control of vegetation, weeds and the improvement of the land. The first thirty pages, however, serve mainly as introductory matter for the non-botanical reader and deal in an elementary manner with plant anatomy, the chemical basis of plant life, energy relationships of plants, growth and development, the practical application of the scientific facts and principles involved being illustrated by familiar examples. The author has brought to his task a wide knowledge of the rapidly expanding scope of "applied" botanical work, coupled with a capacity for compressed statement which stimulates rather than tires the reader. The result is a small volume which contains a remarkable amount of information, thoroughly up-to-date and noteworthy for the variety of the topics described. Thus, we have reference to the gas-storage of fruit and vegetables, plant hormones, vitamins, growth dormancy, vernalisation, photoperiodism, Mendelism and cytology in relation to plant breeding, destruction of weeds, grass drying, silage, the ripening of fruits, disease. The book concludes with a chapter of suggestions for study and further reading. No attempt is made to enter the wide field of commercial plant products which are usually implied by the term economic botany.

S. E. C.

Weeds, Weeds, Weeds. By SIR CHARLES V. BOYS, LL.D., F.R.S., Hon. F.R.S.Edin. [Pp. 69.] (London: Wightman & Co., Ltd., 1937. 1*s*. net.)

THIS small book is a personal account of a weed-destroying contest, by one who is clearly a man of quite extraordinary energy and determination, and who has written his story in a simple but direct and lively manner. There are very definite signs of keen observation, as indeed one might expect from one of such attainments as the author. From the botanical point of view one could wish there had been fewer printing errors—*e.g.* ribwart instead of ribwort, and specific names with capital or small initials rather at random. An editor, also, might too easily find fault with the punctuation, or lack of it! This tends to make some passages "difficult." These mild criticisms, however, do not in any way cancel the fact that this little book is a very straightforward, interesting and practical account of an able scientist's devoted attempts to make and keep his own land both clean and productive.

Sir Charles Boys is not alone in his opinion that the Creeping Thistle (*Cnicus arvensis*) does not commonly propagate itself by seeds, but that the seeds are mainly infertile! Dr. G. H. Bates, who has made a close study of this species, some time ago came to the same conclusion. Nevertheless, in addition to Sir Arthur Hill, both Prof. John Percival and the present reviewer grew the species from seed many years ago. There are several other books on weeds, but all who would like to spend an hour or two with an intimate and interesting writer on the subject can be recommended to read Sir Charles Boys' side of the conversation at his imaginary tea-party under the apple tree. It will cost them little!

H. C. LONG.

The Chromosomes. By M. J. D. White, M.Sc. Methuen's Monographs on Biological Subjects. [Pp. viii + 128, with 20 figures.] (London: Methuen & Co., Ltd., 1937. 3*s*. 6*d*. net.)

Recent investigations on the chromosomes have been both numerous and fruitful, and while the results have been brought together, the biologist, whose interests are not primarily cytological, must have felt the need for some less comprehensive treatment on the subject. The present book provides such a summary. It is unfortunate that the limitations of space have made certain aspects of the subject appear more difficult than, in fact, they are; moreover, the attempt to deal with so large a field of research in so small a compass has inevitably led to some dogmatism. This feature, however, should add to, rather than detract from, the value of the book to the non-specialist reader, and it is to such, and to the advanced student of Botany and Zoology, that the book may be recommended as a lucid résumé of our present knowledge of chromosome cytology.

F. W. J.

AGRICULTURE AND FORESTRY

A History of Agricultural Experimentation and Research in the United States, 1607–1925. By Alfred Charles True. United States Department of Agriculture, Miscellaneous Publication No. 251. [Pp. vi + 321, with frontispiece and 11 figures.] (Washington: U.S. Government Printing Office, 1937. 25 cents, paper.)

In no country in the world has agricultural research been developed on a grander scale than in the United States, and there is probably no one better qualified than Dr. A. C. True to write its history. He has achieved a remarkable feat of condensation in compressing an enormous amount of material into some 300 pages, without omitting any of the great movements. Many references are given to fuller sources of information, and the student will have no difficulty in following up the different branches.

Some kind of agricultural experimental work was forced on the first English colonists, and Mr. Carrier's historical investigations published in 1923 in his *Beginnings of Agriculture in America* are quoted in support of the date 1607 as a starting-point. More definite experiments were made by John Winthrop at the instance of the Royal Society on the possibility of brewing beer from maize: these are published in the *Philosophical Transactions* for 1678. But it was not till the middle of the nineteenth century, when Lawes and Gilbert at Rothamsted and various workers on the Continent had shown what science could do for agriculture, that widespread attention was paid to the subject, and then it was taken up with great energy and enthusiasm. Experiment Stations began to be founded in the 1850's: then in 1862 the Agricultural Department was set up, and it expanded rapidly and continuously till now it is by far the best known and most comprehensive Agricultural Department in the world. Several important Acts, notably the Hatch Act and the Adams Act, have ensured automatic growth of revenue with the increasing need for agricultural investigation: their history is given in sufficient detail, and reference is made to the famous clause in the Hatch Act which was responsible for a good deal of voluminous publication, requiring each station to issue bulletins or reports of progress at least once in three months. The event showed that this condition had serious disadvantages, a result that has been of great value to other stations.

The book is illustrated by portraits of some of the leaders of agricultural science in America many of whose names become household words wherever agricultural science is studied. Students and experts all over the world will be grateful to Dr. True for this history, and they will hope that he may give them more out of the stores of information at his disposal.

E. J. RUSSELL.

Crop Management and Soil Conservation. By JOSEPH F. COX and LYMAN E. JACKSON, Ph.D. [Pp. xviii + 610, with frontispiece and 199 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1937. 13*s*. 6*d*. net.)

THIS class-book, dedicated to the future farmers of America, is a warning against the evils of soil deterioration and a guide to crop cultivation. The reckless exploitation of soil fertility in America goes back to the days of pioneer farming; it was aggravated during the War by the substitution of cash crops for soil-conserving crops such as grass and legumes, and later by the policy of maintaining production in the face of disappearing exports. Less and less was spent on fertilisers and lime, and little was done to help the soil to withstand the continuous drain on its resources. Such neglect has led to the deterioration of over 100,000,000 acres of land, mainly through the ravages of soil erosion.

The vital importance of soil conservation in American farming is reflected in the government control measures that exist to-day and, as might be expected, much of the text of this book is punctuated with warnings of the dangers of living on soil capital rather than by conserving its fertility. Very properly, therefore, the spectre of soil erosion keeps the reader company throughout the first part of the book, which deals with the principles of crop management, including chapters on manuring and fertilisers, crop-soil adaptations, seed raising, planting, harvesting, marketing, crop diseases and pests, etc. Useful suggestions for classroom discussions and for field work are given at the end of each chapter, with the object of encouraging the student to think out local crop management problems for himself. Having decided on the most suitable combination of crops, he can then refer to the second part of the book, in which a practical and informative account is given of the cultivation of specific crops. Although the information in this section of the book is based particularly on farm practice in America, it is likely to prove of considerable value wherever crop production problems are under consideration.

A. J. LLOYD LAWRENCE.

Forest Bibliography to 31st December, 1933. Part II. Compiled and published by the Department of Forestry, University of Oxford. [Pp. 79–199.] (Oxford: Hall the Printer, Ltd., 1937. 12*s*. 6*d*.)

THE first part of the Forest Bibliography appeared in 1936 and included Section A—General Forestry—and the first two subsections of Section B—Silviculture. Part II continues and completes references to literature in Silviculture under the headings, Natural Reproduction, Artificial Reproduction, Tending, Silvicultural Systems, and Notes on Trees.

A. S. W.

ZOOLOGY

Biology for Medical Students. By C. C. HENTSCHEL, M.Sc. (Lond.), and W. R. IVIMEY COOK, B.Sc., Ph.D. (Lond.). With a Foreword by G. E. GASK, C.M.G., D.S.O., F.R.C.S. Second edition. [Pp. xii + 664, with 444 figures.] (London, New York, Toronto: Longmans, Green & Co., 1937. 18*s*. net.)

THE second edition of this book has been revised and also extended greatly so as to include the changes brought about by the joint syllabus adopted by the Universities of Oxford and Cambridge. Of the newer portions of the work dealing with Zoological aspects it need only be said that they seem as adequate for the purpose as the similar portions of the older edition were. For the botanical side, while a number of mistakes in the former edition have been corrected, it cannot, unfortunately, be said that they are not now more numerous than should be expected. In Chapter XIV the wallflower is described and figured; it is stated in the text that each flower bud arises in the axil between the leaf and a stem; this, of course, is not so, as is very properly shown in the drawing.

The algal section is treated with some care but here again Fig. 197 is described in a very misleading way and the term unequal heterogamy applied on p. 362 to a condition seen in some species of spirogyra seems unnecessary and confusing. On p. 399 the description of types of branches is not made definite enough, and advantage is not taken in the figure of the lime twig (Fig. 234, p. 398), to indicate in the drawing or with labelling its sympodial nature. On p. 411 there is a reference to stipules (Fig. 246), and this would seem to suggest that the tendrils of the cucumber are stipular in nature. In this section on stipules the extraordinary statement is made that occasionally, as in certain grasses, they may become enlarged and take over the functions of leaves. Fig. 398 of the mucor sporangium would seem to suggest that the columella is pushed up into the sporangium, and in Fig. 406 *c.*, the wall delimiting the oogonism from the hypha bearing, it has disappeared.

The general get up of the book is good and the price a very reasonable one.

E. M. C.

Recent Advances in Entomology. By A. D. IMMS, M.A., D.Sc., F.R.S. Second edition. [Pp. x + 431, with 94 figures.] (London: J. & A. Churchill, Ltd., 1937. 15*s*. net.)

THE new edition of this valuable work of reference is sixty pages longer than its predecessor and has ten more figures; it costs half a crown more. A number of sections have been rewritten (*e.g.* Palæontology, Locusts), and eighteen of the old illustrations have been omitted. The book remains what it was when first published in 1931, a very useful and up-to-date summary of those branches of entomology in which progress has recently been most rapid. Only a few aspects of Physiology, in its broadest sense, are dealt with; but part, at least, of what is not touched on has been summarised in other recent books.

Certain comments and minor criticisms may be made. If the conclusions suggested by palæontology (p. 102) are to be accepted, the endopterygote type of metamorphosis must have been evolved more than once. Entomologists have not yet really faced the consequences of this possibility.

Although Ramsay's work on the theory of water-loss in insects is men-

tioned, its consequences have not been fully assimilated. It is no longer true to say (p. 234) that the Saturation Deficit is the index of the evaporating power of the air, for, as the temperature increases from 0° to 30° C., the evaporating power (for the same saturation deficit) increases by 25 per cent. Moreover, in actual experiments, several additional factors may make the relation between saturation deficit and evaporation still more complex.

The account of the biological races of mosquitoes (p. 291) is not altogether in accord with Hackett's (1937) latest summary of the subject. This field, however, is advancing so rapidly that it is difficult to keep pace with the new facts that are being brought forward.

One misprint has been noticed (p. 413, *Pantarthrum*).

O. W. R.

Wonders of the Great Barrier Reef. By T. C. ROUGHLEY, B.Sc., F.R.Z.S. [Pp. xvi + 282, with 36 coloured and 15 black and white plates.] (Sydney : Angus & Robertson, Ltd. ; London : Australian Book Co., 1936. 15s.)

ALTHOUGH many have seen the bleached skeletons of coral in museums, whence some idea may be obtained of their remarkable variety of form, yet few have had the opportunities of seeing a living coral reef. And those few are not so much amazed at the various forms displayed but rather at the wealth of colour. It is now possible for the less fortunate who cannot travel far afield to gain some idea of these wonderful hues, for Mr. T. C. Roughley has written a most delightful and instructive book very fully illustrated with colour photographs. The first thing the happy possessor of this book does is to turn the pages until all the coloured plates have been seen, because never before has such a series of coloured photos been published. The plates are reproduced from Lumière autochrome transparencies, and they are very true in hue. Although perhaps not quite so vivid as in nature, owing to the small size of their reproduction, they do portray that soft delicacy of shade so typical of coral colours.

The book contains 52 of these colour plates illustrating, besides the coral, many of the typical animals of the reef. There are also a number of black-and-white half-tone illustrations, amongst which an unusual photograph of a flying fish in flight is especially deserving of mention.

But in our zeal to examine these beautiful illustrations we must not forget that the book has printed pages. Mr. Roughley is an Australian zoologist of repute and wide in his interests. He has written this book as a nature lover's guide to the Great Barrier Reef of Australia, that great system of reefs which stretches for 1,250 miles along the Queensland coast. After three introductory chapters on the nature of the reef in general a number of chapters are devoted in turn to the different groups of the animal kingdom represented on the Reef. The features of the reef and examples of animals dealt with are well chosen. In fact, every item that remains in the mind of a naturalist who has studied on these coral reefs seems to have been included and an extraordinarily true and detailed picture of the reef presented. By way of interlude there are occasional passages of topical interest, including some of Mr. Roughley's own experiences, in which he shows himself very human as well as a good scientific observer. The book is written primarily for the layman to stimulate in him an interest in the reef. There is no doubt that it will do this, for the lives of the various animals are described in so interesting and simple a manner. But the descriptions are also accurate

and the book should perhaps act as an even greater stimulant to the trained zoologist, for whom a glossary of scientific names has been appended. Mr. Roughley is to be congratulated on writing so fascinating a book and the publishers, Messrs. Angus and Robertson, for producing it at the moderate price of fifteen shillings.

F. S. R.

Cold Spring Harbor Symposia on Quantitative Biology. Vol. V. [Pp. xvi + 433, with numerous figures.] (Cold Spring Harbor, L.I., N.Y.: The Biological Laboratory, 1937.)

THE Cold Spring Harbor Symposia have established themselves as literature to which biologists look forward, and the present volume, devoted in a general way to endocrinology, is an important addition. Sex-hormone effects receive attention in many of the articles, as do the relations between the pituitary and other glands. The chemistry of sterols which are physiologically active is dealt with, and a useful list of units in hormone assays is given. It is impossible to mention individually all the 43 papers which compose the book, and to make an exception would be invidious.

G. R. DE B.

Practical Zoological Illustrations. Part I: Vertebrates. By S. LOCKYER and D. R. CROFTS. (London: Macmillan & Co., Ltd., 1937. 28 Cards in Portfolio, 10*s.* 6*d.* net. Cellophane Envelope to take one Card, 1*s.* net.)

THIS work consists of a number of simple figures of dissections of the dogfish, the frog, the rabbit, and the heart of the sheep. They are executed in black and white, with red and blue for the vascular system of the rabbit. They are intended to help students to make the dissections for themselves and to save the time expended in reading wordy descriptions. While the drawings by their large size and clarity may reasonably be expected to assist elementary students to find their way about their first dissections, they can hardly be regarded as sufficient substitutes for the existing dissecting manuals and guides to practical work. There also remains the danger that the students may copy the figures instead of drawing original diagrams from their own dissections. The authors consider that they have guarded against this danger by making each illustration a faithful reproduction of an individual dissection and not a generalised diagram. It is, however, optimistic to think that this will by itself act as an effective deterrent to the hardened picture-copying student.

Apart from this, the idea of these cards is novel, and it will be interesting to see how they fulfil their purpose in practice.

G. R. DE B.

The Locust Outbreak in Africa and Western Asia in 1936. By B. P. UVAROV, D.Sc., and MISS W. MILNTHORPE, B.Sc. Economic Advisory Council Committee on Locust Control. [Pp. 55, with 9 maps.] (London: H.M. Stationery Office, 1937. 3*s.* net.)

THE present report covers the year 1936 and the early months of 1937. Two of the species, the African Migratory Locust and the Red Locust, have continued the decline in numbers which was noticed in previous reports. Although this is an immediate benefit to agriculture in the countries which

these species inhabit or invade, the benefit will be short lived if the absence of an acute emergency leads to any interruption of the research and of the intelligence system which have been built up. This is clearly shown in the report on the Desert Locust, *Schistocerca gregaria* (Forsk). This species has apparently started breeding up swarms in the countries bordering on the Red Sea. The situation is not yet serious, but may well become so, for instance, in Egypt. The new outbreak, if such it is, is beginning in an area already under close observation and news of the impending invasion has been sent to all the countries threatened. Unlike previous invasions, the present one will be met everywhere by Agricultural Services fully prepared to combat it. This is one of the greatest economic justifications of the intelligence system, which now only requires to be supplemented by an international agreement for keeping watch on all areas where outbreaks originate.

O. W. R.

MEDICINE

Applied Mycology and Bacteriology. By L. D. Galloway, M.A., and R. Burgess, M.Sc., Ph.D. [Pp. x + 186, with 1 plate and 3 figures.] (London: Leonard Hill, Ltd., 1937. 10*s*. net.)

It is obviously not possible to compress into so small a bulk an even remotely adequate account of fungi and bacteria, and this volume makes no attempt to do so. It is intended simply as an elementary review of its subject, which shall assist the non-specialist to a bird's-eye view of the bewildering mass of detail included under the terms mycology and bacteriology, and at the same time direct him to the literature where he may find a fuller account of any branch in which he is particularly interested. Human and animal diseases are scarcely touched and the main scope is the part these organisms play in industrial processes. It is divided into two parts. The first deals with the classification of fungi and bacteria, the apparatus and methods of handling and identifying them, their metabolism and their control. It is the barest outline, and on the technical or manipulative side can give only a general idea of the kind of operations involved without enabling a novice actually to isolate and identify any organism. The second deals with the part played by the organisms in the food industries (storage, transport, etc.), fermentation and textile industries, some aspects of hygiene (such as the examination of water and the disposal of sewage) and of agriculture, with a final chapter on miscellaneous subjects, such as timber, rubber, leather. All these are treated on the broadest lines, and little more is done than to indicate in what parts of the social organisation micro-organisms are important and the way in which they function. Within its limits the subject is accurately and competently treated and may meet the requirements of a public which wants to know why so much attention is paid to these seemingly insignificant forms of life. The industrial sections in particular are excellently handled.

J. Henderson Smith.

An Introduction to Bacteriological Chemistry. By C. G. Anderson, Ph.D.(Birm.), Dip.Bact.(Lond.). [Pp. viii + 278, with 5 figures.] (Edinburgh: E. & S. Livingstone, 1938. 10*s*. 6*d*. net.)

As stated in the preface, this book is the outcome of lectures delivered to students for the University of London Academic Diploma in Bacteriology and more recently to students taking Bacteriology as an Honours Subject

in the University of Edinburgh; it is intended to cover the requirements of students and research workers requiring some understanding of the metabolic behaviour and chemical nature of the organisms they are handling, and to keep the size of the book within reasonable limits the author has assumed a knowledge of elementary organic chemistry and of a certain amount of bacteriology. The book is divided into three parts entitled General Consideration, Metabolism, and Some Aspects of Immunochemistry. In the first of these are contained some commendably clear and concise accounts of hydrogen ion concentration, oxidation-reduction potentials, colloids and adsorption including the elements of the electronic theory of valency, enzymes and the chemical composition of bacteria, yeasts and the lower fungi. Pigments and growth factors are but lightly touched on in this section, but have special chapters devoted to them in the later portion of the book, though it must be admitted that the treatment of the bacterial pigments is somewhat meagre. Part II, devoted to metabolism, comprises fourteen out of a total of twenty-one chapters; the selection of material from the enormous amount available is altogether admirable and contains in succinct form a wealth of information which is only to be found in a much scattered literature. By a liberal use of structural formulæ and a lucid and interesting style of writing the author has contrived to amass into a small compass a surprisingly comprehensive amount of instruction which should serve as an excellent equipment for all who wish to obtain an insight into what the author describes as bacteriological chemistry, but which in fact includes the chemistry of yeasts and lower fungi as well.

The very complex subjects of antigens, haptens, antibodies and the mechanism of antigen and antibody reactions are ably dealt with in two chapters comprising Part III of the volume, and a short appendix at the end gives methods for the isolation and identification of metabolic products.

P. H.

The Biological Standardisation of the Vitamins. By KATHERINE H. COWARD, D.Sc. [Pp. viii + 228, with 44 figures.] (London; Baillière, Tindall & Cox, 1938. 12*s*. 6*d*. net.)

THE object of this book is to give, in simple straightforward terms, a detailed and comprehensive account of the techniques employed for the biological assay of the four vitamins, A, B_1, C and D. Within its strictly prescribed limits, the book is very comprehensive. Thus, for example, advice is given as to the best strain of animal to use, the management of the animals, their housing, and so on, as well as the actual arrangement of the assay experiments themselves. In all this the authoress draws on her own very wide experience of this type of work. Indeed, the book has been written primarily with the purpose of placing at the disposal of others the large amount of valuable information gained in the course of many years spent in vitamin research and assay.

The second part of the book, from Chapter VII to Chapter XII inclusive, deals with the statistical evaluation of the results of the animal experiments. An elementary but lucid account is given of the fundamental notions involved, and of the arithmetical calculations to be carried out. It would of course be inappropriate in a book of this kind that this part be in any way elaborate or complicated, but it might be suggested that some reference be made to the modifications required, when the number of animals is small, say

than 10 or 20. This point is of practical significance, as ten is not an uncommon number of animals to use in a biological assay.

This book will obviously be of the greatest value to all scientific workers interested in biological assay of the vitamins. In spite of the remarkable advances in vitamin chemistry which have been made during recent years, it would seem that the biological method of assay will still remain one of practical importance. This is because the compounds are so active and the amounts present are in consequence normally so small, that chemical methods fail. The importance of accurate methods of assay, both for ordinary foodstuffs and for special vitamin preparations, needs no emphasis. The present volume should help in establishing a high standard in this field of work.

W. O. K.

Civilisation and Disease. By C. P. DONNISON, M.D., M.R.C.P. [Pp. xvi + 222.] (London : Baillière, Tindall & Cox, 1937. 10*s*. 6*d*. net.)

STUDY of disease in native societies is handicapped by the difficulties of obtaining adequate clinical data and consequent unreliability of statistical results. In this book the author has limited the range of his subject by selecting four diseases : high blood pressure, diabetes melitus, Graves' disease and peptic ulcer, in each of which certain factors appear to exert a causal influence of a psychogenic nature affecting the centres of the hypothalamus, and endocrine and autonomic nervous systems, also the nature of response to psychologic stimuli under civilised conditions.

The task is further complicated by the determination of the level of civilisation or cultural development of the races studied. The guide to establishing a cultural level was the manner in which the race or tribe maintained a right relation with the powers of the universe, and whether they erected temples. As a result of this enquiry there emerged four levels —Rationalistic, Deistic, Manistic and Zoistic. It was also necessary to decide which of the several schools of psychology was most suitable to the study of psychoneurosis. After describing the experiments of Pavlov dealing with conditioned reflexes, the author gives a short clear account of the schools of Freud, McDougal, Jung, Adler and Sutter and considers that the individual psychology of Adler is most suitable. Adler's conception of a neurosis is a defect in social environment resulting in an accentuation of the striving for superiority or "will to power."

It is, of course, apparent that Dr. Donnison in this most interesting and thought-provoking book has set himself an almost impossible task, no less than the study of the psycho-somatic unity of the individual, and the psychological aspects of the social development of the human race ; a study which to-day claims increasing importance in medicine. This book is a valuable contribution towards the introduction of the subject. It is easy to read and free from "the smoke screen of complex verbosity" behind which most modern psychologists conceal the insecurity of their conclusions.

P. J.

The Structure and Composition of Foods. Vol. III: Milk (including Human), Butter, Cheese, Ice Cream, Eggs, Meat, Meat Extracts, Gelatin, Animal Fats, Poultry, Fish, Shell-fish. By ANDREW L. WINTON and KATE BARBER WINTON. [Pp. xxviii + 524, with 11 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1937. 40*s*. net.)

THIS is the third volume of an encyclopædic work which will be completed in four volumes. The two previous volumes dealt with cereals, starch, oil seeds, nuts, oils, forage plants and vegetables, legumes, fruits, and the fourth will deal with sugar, syrup, tea, coffee, spices, extracts, etc.

The book is invaluable, and almost indispensable, to those interested in food and nutritions, but like all such books it must be used with circumspection. After all, it is intended for American readers mainly (and, of course, America is much more generally alive to the importance of dietetics than almost any other country) and therefore makes more use of American foods and American food tables than of those from other countries. Consequently the old Atwater and Bryant tables will not usually be found satisfactory for English foods, particularly fish. Plimmer's tables of English foods do not seem to have been consulted. It is also odd to find analyses of meat ash by Lawes and Gilbert and very little since except for the minor mineral constituents. The chapters on meat products (cured and smoked meat, cooked meat and canned meat) are disappointing and no analyses by McCance and Shipp of cooked meats and fish are included, although they appeared some years ago.

On the other hand, the reader will find the chapters on milk and milk products and eggs of the utmost value, though we may be pardoned a smile at the statement "Cheshire [cheese] has long been made in Chester, England, from unskimmed cows' milk." It is easy to pick holes in an encyclopædic work and no one should be deterred by the pinpricks of this review from consulting it.

V. H. M.

A Laboratory Handbook for Dietetics. By MARY SWARTZ ROSE, Ph.D. Fourth edition. [Pp. xii + 322.] (New York and London: Macmillan & Co., Ltd., 1937. 12*s*. 6*d*. net.)

ANYONE who knows of Prof. Rose's work in Teacher's College, Columbia University, New York City, will know what to expect of this book. It has gone into a fourth edition, and because dietetics advances at an incredible rate, the book has had to be enlarged and modified. "In the eight years since the publication of the third edition there has been a steadily increasing appreciation of the mineral elements and vitamins as controlling factors in nutrition. . . . In consequence the tables giving mineral elements and vitamins have not only been completely recalculated but almost doubled in size."

Anyone who has used the third edition will purchase the fourth edition. Those who have not yet made the books' acquaintance should repair that defect in their education and work through the examples in Part II. Part I deals with Food Values and Food Requirements, to which the dietitian will bring his own prejudices for assay. Part III, much the largest part of the book, consist of tables of the most varied and valuable type. Apart from the strictly dietetic tables (usually in ready reckoner form) there are

tables of conversion from pounds into kilogrammes and vice versa, and inches to centimetres and vice versa and so on. The practising dietitian cannot get on without this book for there is nothing else to take its place, nor is it easy to imagine one that could be better.

V. H. M.

PHILOSOPHY

Scientific Inference. By HAROLD JEFFREYS, M.A., D.Sc., F.R.S. Reissue with Addenda. [Pp. viii + 272, with 12 figures.] (Cambridge: at the University Press, 1937. 10*s.* 6*d.* net.)

A SECOND edition of this well-known book is to be welcomed, not only for the 25 pages of additional material (including an account of the author's recent work and the correction of errors in the original publication), but also for the implication that Jeffreys's contributions to the practice of scientific method are receiving the attention they deserve. There is everything to be said for allowing absolute freedom to the minds of original geniuses, leaving their inspirations to be justified and their inconsistencies removed by the far larger band of exact thinkers who should constitute the main body of men of science. It is for this latter group that Jeffreys's work is valuable. He not only urges the importance of problems in the treatment of observations of which the majority of scientists are scarcely aware, but also provides a technique for their solution which merits general and careful consideration. Many points in his system are debatable, but its wide range of application, its internal coherence, and its success when applied to many of the most important problems in physical and general statistical science, give it, as he might say, high probability, both prior and posterior.

The weakness of the scheme, however, seems to us to lie in the foundation on which it rests, and it is not irrelevant to point this out because the author evidently intends his work, not merely as a trustworthy tool, but as having general philosophical validity. The discussion originates in the acknowledgment that "it is possible to learn from experience" (such learning, in fact, when properly conducted, constitutes "scientific inference"), and since logic has failed to prove this, the fact itself is taken as a primitive postulate and its consequences are developed. Now this evidently calls for a clear statement of what is meant by "learn," and such a statement is not given. The result is that we are in doubt from the beginning about what it is that the author is ultimately trying to do. Can we say, for instance, as someone has said of History, that we learn from experience that we can learn nothing from experience? It is fairly evident that Jeffreys would accept neither meaning of "learn" implied here, but when we ask what he does mean, the nearest we get to an answer is the statement on p. 254, near the end of the book, that the principle that we can learn from experience is equivalent to the principle that the probability of the truth of a proposition, p, on data, $q + h$, is not always equal to the corresponding probability on data h (q here being provided by experience). This, of course (the method of calculating probability having been compatibly defined), is true, but what do we learn from this about p? It is agreed that the probability can never reach certainty, and that the acquirement of q may increase or lower the probability. In what sense, then, do we "learn" from experience, and why should the change of probability be called learning rather than unlearning? It is surprising that Jeffreys, having gone so far towards realising that science is

a process of dealing rationally with experience which is to be justified, if at all, on grounds other than the attainment of certainty about a "real world," should yet insist so strongly on the importance of acknowledging the significance of such a world. Presumably even a baby learns to value its comforter for reasons other than the probability of its yielding inaccessible milk.

The fact is that Jeffreys's philosophy has intrinsically the strength of subjectivism, but is weakened by the vestigial "real world" or "true law" or "probability = 1" (the name depends on the particular application) which serves no purpose but that of inducing pathological conditions. Under less capable control, this defect would have had fatal consequences, and even Jeffreys's vigilance is insufficient to keep it innocuous. For instance, it prevents a satisfactory statement of the "simplicity postulate" (an assumption made in order to enable prior probabilities to be assessed), for the use (p. 176) of that postulate, backed by subsequent observation, to give almost complete verification to the proposition that the fundamental equations of dynamics are independent of the velocity of the origin, would justify at least equal verification being granted to the proposition that there are no fundamental equations of dynamics. And again, the attempt to establish mechanics on a basis of mensuration fails because the Doppler effect reveals motion by a single instantaneous observation, independent of any process of measuring time or space intervals, so that motion must be more primitive than such intervals, in terms of which it is usually expressed. Another manifestation of the same fundamental weakness is the appearance of a detailed treatment of "errors" of observation in a scheme of thought which professedly takes observations as primary data. It is much to be hoped that in the next edition this superstition of a metaphysical "real world" will be removed, so that the actual value of Jeffreys's work may compel the recognition due to it.

H. D.

MISCELLANEOUS

Mapungubwe: Ancient Bantu Civilization on the Limpopo. Reports on Excavations from Feb. 1933 to June 1935. Edited on behalf of the Archæological Committee of the University of Pretoria by Leo Fouché. [Pp. xiv + 183, with 4 coloured plates, 40 black and white plates, 26 figures, 2 maps and 6 folding diagrams.] (Cambridge: at the University Press, 1937. 50*s*. net.)

This magnificent book, illustrated with plans, half-tones and some coloured plates, gives a very detailed account of excavations on a flat-topped hill in the northern Transvaal where some treasure hunters, confirming native legends, actually did find treasure—gold ornaments—which fortunately were reported to the University of Pretoria. The book is divided into the following parts. There is a general account of the actual excavations. The pottery is considered in considerable detail and the whole field of Zimbabwe pottery is discussed. The beads and gold ornaments have the attention which such treasures deserve; slag and vegetable remains are described. The human remains are described in detail, together with a special chapter on their dentition. At the end, the editor writes a short note on general conclusions. It is obviously impossible to discuss so wide a field in detail. It may be stated that on the evidence, especially of pottery, the conclusion is reached that the period covered falls roughly between the beginning of the

sixteenth and the middle of the eighteenth centuries and is parallel to Zimbabwe. The people are considered to be a race of pastoralists and very special interest attaches to their bones. Owing to the nature of the climate, only the remains of eleven individuals and those in a fragmentary condition were available for examination. They are described in considerable detail. Dr. Galloway considers that they belong to a homogeneous people, with consistent racial features, and belonging to what he describes as the Bush-Boskop group. They are remarkable for the absence of Negro features and are akin to the post-Boskop inhabitants of the coastal caves. Dr. Galloway supports his contention by a very detailed examination of all the skeletal features, point by point. He is fully aware of the danger of generalising on a few individuals and has accordingly made up for the deficiencies of numbers by a very large number of observations on each skeleton. If his conclusions are true they conflict, and there is, of course, no reason why they should not, with the cultural conclusions which posit a close connection with Zimbabwe, and the ethnologists are satisfied that the culture is due to two distinct peoples living together, the one probably of Sotho stock and the other undoubtedly of Shona stock. But Shona and Sotho are definitely Negro, in fact what is loosely called Bantu. Hence, as Aristotle would say, an aporia. This the editor cheerfully faces in his conclusions and suggests the only way out is to carry on more work, *solvatur excavando*. In fact, the whole principle of the book, on which the editor must be congratulated, is the way in which workers have been allowed to say exactly what conclusions the evidence in their own field leads them to. The editor at the end follows the rule of a good chairman and says a few words, drawing attention to the main points in the previous discussion, and is singularly self-effacing about his own work, which must have been very considerable.

L. H. D. B.

Animals for Show and Pleasure in Ancient Rome. By George Jennison, M.A., F.Z.S. [Pp. xiv + 209, with 11 figures and 8 plates.] (Manchester University Press, 1937. 12*s*. 6*d*. net.)

Mr. Jennison's heavily documented work is a revelation of the extent to which animal-keeping formed part of the culture of the ancient Romans and Egyptians. It is interesting to find that while qualitatively different, Roman zoos were probably as large or larger than their modern counterparts in Europe and America of to-day.

Of course, the objects of these amazing collections were quite different. Often processions of animals, performing and otherwise, and arena shows, were staged for the purpose of raising the prestige of a rich ruler (like Pompey) or private citizen. Nearly every Roman leader of note seems, for instance, to have owned lions. So, to-day, does Mussolini.

There can be no doubt that the demands made by Roman shows actually had the effect of depleting animal life in Northern Africa. At the dedication of the Colosseum by Titus in A.D. 80, 9000 animals were killed in a hundred days; 5000 were exhibited in one day. The London Zoo has 1000 mammals.

From the zoological point of view perhaps the most important part of this book is that devoted to the identification of the animals that were introduced and kept in captivity in ancient Rome. In this respect much of value to the study of geographical distribution can be learned. It would be, perhaps, a little pedantic to cavil at the use of archaic (*Mustela* for the

martens) and sometimes misspelt Linnean names (*M. fovina* for the Beech Marten, small letter for some generic names). One can suggest, too, that the Greek χολοίος of Aristophanes may have referred to the Jackdaw and not to the Chough. But apart from these insignificant mistakes the book should receive a great welcome in the widest circles.

JAMES FISHER.

Animals and Men: Studies in Comparative Psychology. By DAVID KATZ. [Pp. xii + 263, with 37 figures.] (London, New York, Toronto: Longmans, Green & Co., 1937. 12*s*. 6*d*. net.)

DR. KATZ, who spent some years in this country, and is now Professor of Psychology and Education in the High School of Stockholm, is a distinguished psychologist well known for his work on colour perception and the psychology of needs. When professor at Rostock, he carried out with his pupils some highly interesting work on animal psychology, giving special attention to dogs and fowls. The present book is to some extent written round these special researches, but it does at the same time cover a wide field. Beginning with a discussion of the so-called thinking horses and the important lessons to be learned from the critical study of these cases, he considers next the relation of animal psychology to the other sciences and the difficult problem of method. There follows a study of animal perception and of orientation in space. The next section, on "Needs, Drives, Instincts," is of particular interest and value, as one might expect. A discussion of the social psychology of animals and the psychological comparison of animals and man complete the book. It is written rather definitely from the point of view of the professional psychologist and deals mainly with vertebrates; not much attention is paid to the ecological background of behaviour. But the author does score a point against the purely biological point of view by demonstrating that in some cases sensory acuity appears to be far greater than biological requirements would demand.

The book is a very useful one and deserves to be widely read, especially by zoologists, who will find many valuable observations on animal behaviour of which they are probably unaware. It can be read also with profit by the wide public which is interested in animals from a professional or an amateur point of view. It can be specially recommended to those who are interested in dogs, and the poultry keeper will find in it points of practical interest. As a general introduction to the study of animal behaviour it has many merits; it is very much up to date, and the references to recent literature are full and detailed.

E. S. RUSSELL.

Science and Music. By SIR JAMES JEANS, M.A., D.Sc., Sc.D., LL.D., F.R.S. [Pp. xii + 258 with 10 plates and 64 figures.] (Cambridge: at the University Press, 1937. 8*s*. 6*d*. net.)

BY his popular exposition of "time, space and the mysterious universe around us," Sir James Jeans has turned many of us into ardent gazers at the "stars in their courses." "Ruining along the illimitable inane" in these celestial excursions has doubtless made him especially sensitive to the music of the spheres, for now he directs our attention to "Music and Science," wherein his brilliant powers of description and exposition are wedded once again to his unrivalled felicity of metaphor and illustration.

With characteristic modesty he protests that "a considerable fraction

of my book is simply Helmholtz modernised and rewritten in simple language": so might Fitzgerald have deprecated his translation of Omar Khayyâm.

The advent of broadcasting has at one and the same time simplified and complicated the task of writing on the topic of Science and Music. For though we are so familiar with wave-lengths, tuning-in, carrier-waves and the like, yet our auditory experiences are now so commonly more extensive, ranging as they do from the sounds of balalaika and bombardon to the cembalo and mighty Compton cinema organ—not to mention the incidental music of typewriters, motor-cycles and pneumatic drills. Yet all such noises, sounds and combinations of sounds form Sir James's theme, which he elaborates with unqualified success. From a study of tuning-forks he develops the idea of a pure tone, resonance, frequency and beats. Particularly ingenious is his ruse of discussing the familiar recording barograph as an example of a time-displacement graph, from which by easy stages we reach the graphical representation of simple harmonic motions. The compounding and analysis of complex harmonic motions follows, and the subject of timbre is a logical sequel. Passing from pure tones to the study of vibrating strings we learn what causes the soul of the cat to protest when the tyro violinist is at practice: the rôle of the formant and the X-ray examination of the structure of old violins are also treated. From a chapter on vibrating columns of air we pass on to the orchestral wind-instruments and the organ. Harmony and discord, scales, equal temperament and the music of the future all receive their due share of attention and the end of the book deals with the psychological side of hearing. This phase is particularly well treated for a book of this size, and many will welcome the discussion of loudness, the threshold of pain, and difference and summation tones.

When, as in this volume, there is so much to praise it seems unkind to point out omissions. Sir James has so charmingly described the courtship of grasshoppers by telephone and the secret of the policeman's whistle; is it fair to question his explanation of why I sing in *my* bath and to deplore the absence of any reference to the ubiquitous wa-wa mute? H. IRVING.

Motion and Time Study. By RALPH M. BARNES, M.E., Ph.D. [Pp. x + 285, with 123 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1937. 18*s*. 6*d*. net.)

FOR centuries the problem of human movements has proved provocative, although the tendency has been to treat it as a philosophic challenge. Some scientists, however, realised that movements could be measured, and that the result of that measurement would yield valuable results. So from Galileo to Borelli, and on to Coulomb, sporadic efforts were made to understand and measure simple and complex movements.

The practical aspect of movement measurement came from America towards the end of the nineteenth century, and what is now known as Time and Motion Study is almost entirely due to the work of two American engineers, Taylor and Gilbreth, who were concerned about the waste of human effort due to ignorance of the scientific principles of skilled movements.

Rule-of-thumb methods must be replaced by a scientific study of a man's work in all aspects, and one part of this study involves knowing, (*a*) what are the constituent elements of a movement or series of movements, and (*b*) how long it takes to perform each part. The result of such a study

leads to the elimination of unnecessary, wasteful or fatiguing movements. The large-scale developments of many industrial operations during the present century has necessitated a study of the most economical ways of doing a particular job with the object of fixing a wages' scale.

Prof. Ralph Barnes in his book gives a comprehensive account of the nineteenth-century pioneers and the later developments. He presents a clear and interesting account of the efforts of Taylor and Gilbreth and then applies time and motion study to various engineering operations, *e.g.* gear hobbing and soldering cans. He supplies careful details for making stop-watch studies as well as for micromotion work, and shows what use can be made of such studies in regard to the human being, to the design of tools and equipment and to wage incentives.

Although his illustrations are from engineering, the general principles are so clearly expressed that the application can easily be made to other kinds of skilled work. He also avoids the error of believing that output is affected only by faulty or inadequate methods adopted by the worker.

A series of problems and an adequate bibliography complete what will prove to be an extremely valuable addition to this branch of study.

M. S.

Psychology Down the Ages. Vols. I and II. By C. SPEARMAN, Ph.D., Hon. LL.D., F.R.S. [Vol. I: pp. xii + 454, with 27 figures and tables; Vol. II: pp. viii + 355, with 26 figures and tables.] (London: Macmillan & Co., Ltd., 1937. 30*s*. net.)

As the author states in his Prologue this is a history of psychology of a peculiar kind. How peculiar it is may perhaps be gathered from the fact that while Thomas Aquinas is quoted or referred to 26 times and Hamilton 24 times, Stout is referred to 9 times, Ward 8 times, and Lloyd Morgan once. One cannot help regarding it as a history of Spearman's own development of psychology, rather than of the science in general. From beginning to end the impression is given that there is only one useful line of approach to psychological problems, and only one true point of view.

The book is divided into five parts, entitled respectively: "What Psychology is About," "What the Psyche can Do," "How the Psyche is Constituted," "What Follows What," and "What Goes with What." These parts do not represent any temporal or developmental continuity or sequence, nor is such sequence shown in any one part, except in the most general way. It is only when Chapter XL is reached—"The Discovery of 'G'"—that any meaning at all can be read into the sequence, and then it is at once seen to represent the development of the author's own psychological views.

A critical estimate of the work as a whole is not possible without going into greater detail than the present notice permits. At the same time it does seem necessary to call attention to one or two points. The practice of quoting short passages—sometimes single sentences—out of their context, to which the author is very prone, can hardly fail to be misleading, and when such quotations occur, as they often do, without any sort of temporal, or even logical, order, the effect is by no means happy. Here and there, too, Spearman discusses with a superficial flippancy views which he does not wish to understand. For example, on p. 398 of Volume I he quotes Ward with regard to the "*Me* known" and the "*I* knowing," and goes on: "But what can he really mean by thus setting up the 'ME' against the 'I'?

These two seem to differ solely in respect of grammatical case. Every schoolboy can run through 'I, me, of me, to me, by me.' All refer to precisely *the same thing.*" Lastly the chapter heading "The Confusion that is Gestalt Psychology" is very unfortunate, and the criticism the chapter contains not very fair.

Though the book is not one which the teacher of psychology would advise the interested layman to read in order to gain some knowledge of modern psychology, nor is it one which he would care to prescribe to his students as a text-book in the history of psychology, there are chapters which are distinctly valuable from the point of view of the advanced student, and the true setting of the views of the "London School" is, as one would expect, very clearly brought out. Moreover, the book is an admirably written, scholarly production. The author's knowledge of the philosophical and psychological literature of all periods is encyclopædic, and his criticism is at times very acute, however prejudiced it may be. The second volume in particular has the further merit of presenting the latest and most mature views of the leader of an important British school of psychology in the field he has made his own.

JAMES DREVER.

An Introduction to the Theory of Statistics. By G. UDNY YULE, C.B.E., M.A., F.R.S., and M. G. KENDALL, M.A. Eleventh edition. [Pp. xiv + 570, with 55 figures and 4 folding plates.] (London: Charles Griffin & Co., Ltd., 1937. 21*s.* net.)

SINCE the publication in 1910 of the first edition, Mr. Yule's book has stood out as perhaps the most comprehensive and readily intelligible elementary text-book in the English language on the mathematical theory of statistics. Based in the first instance on a series of lectures given in the years 1902–9, it was essentially the work of a teacher who had learnt from experience how to present his subject clearly to his audience. But, while much of the original exposition dealt with certain fundamental ideas and methods which no development of theory could modify, recent editions had suffered increasingly from the fact that the author felt unable to give more than a few brief references to the advances in method developed during the last dozen years. It is therefore a matter for very general congratulation that Mr. Yule has been able to find in Mr. M. G. Kendall an enthusiastic younger collaborator who has set about the task of welding the new on to the old.

This task is not an easy one. Unless the book were to be completely rewritten on a fresh basis, which many who have learned to know it would regret, its general plan was already fixed, a plan which left the theory of sampling and therefore the introduction of the theory of probability until two-thirds of the way through the volume. It is to this final third that Mr. Kendall has added his main contribution, commencing with an excellent opening chapter on "Preliminary Notions on Sampling," and extending with fuller illustration what Mr. Yule had previously written on large sample theory.

While there is an essential continuity between the work of Karl Pearson and Udny Yule on the one hand and R. A. Fisher on the other, it is inevitably difficult to bring this out in a book whose plan has been to deal first with descriptive methods and only afterwards to introduce the problems of sampling. Mr. Kendall's final chapter on "The Sampling of Variables—Small Samples" is therefore perhaps his least-satisfying contribution, though

it admittedly aims only at giving a brief survey of a rapidly extending field. To deal convincingly with this branch of the subject needs perhaps more first-hand experience of wrestling with the application of the methods of analysis of variance than Mr. Kendall has yet found time for in a busy life.

Nevertheless, a description of most of the simpler methods of this analysis is given; there is an admirable list of references for reading, and finally there are included for the first time tables and charts giving the usual percentage significance levels of χ^2, t and z.

The book maintains its tradition of breadth of view; the authors have taken from many schools the methods which they believe to be of value. They have reproduced from scientific memoirs, often not easy of access to the average student, statistical results of many years' standing, which are generally omitted in most text-books. Such results, often of great practical service, are in danger of being missed altogether by a younger generation of statisticians who are led to believe that what was written before 1918 is out-of-date. The new sections are again admirably illustrated with a variety of carefully chosen examples.

It is, of course, a well-recognised fact that scarcely any two teachers of mathematical statistics approach their subject in the same way; few perhaps would follow the exact order of development given by these authors. But a teacher of the elements of statistical theory should have no difficulty in picking out from this book chapters which will help greatly in the development of the subject on his own lines. The present reviewer, for example, would probably advise a student to omit at first reading the five chapters on the theory of Attributes, that is to say to follow the Introductory chapter by Chapters VI–XII; after this it might be well to take the four chapters XVIII–XXI on sampling. The final new chapter on Interpolation and Graduation written by Mr. Yule himself should be read too at an early stage. If such a reading does not provide the student with information as to how best to plan and interpret *small-scale* experiments, it should give something which is invaluable as a preliminary to further study—a sound knowledge of fundamentals based on clearly illustrated reasoning.

E. S. P.

Life as a Whole. By J. W. Bews, M.A., D.Sc. [Pp. x + 347.] (London, New York, Toronto: Longmans, Green & Co., 1937. 15*s*. net.)

After reading the eulogy on the dust-cover by General Smuts and seeing from the title-page that the author is Principal of Natal University College, it is with much regret we are compelled to express our disappointment with this book. The author's preface and introduction prepared us to expect a new approach to the story of man's life as a whole, some really scientific study of the nature of "Holism" and its relation to environment or human ecology. After toiling through several hundreds of pages it would seem that Prof. Bews has no serious interest in "Holism," has no new aspect to explore, and is content to serve up an elementary-school version of the often-told history of human progress—evolution without Darwin—of no scientific value, expressed with frequent repetitions and inflated with catalogues which must do duty for reasoned explanations. Generally the book is conspicuous by the absence of continuity in narration or any connected whole. Such a book as this supplies no need. There are many others which express the same story with fewer digressions.

P. J.

The World and Man as Science Sees Them. Edited by FOREST RAY MOULTON, A.B., Ph.D., Sc.D. [Pp. xx + 533, with 178 figures.] (U.S.A.: University of Chicago Press; Great Britain and Ireland: Cambridge University Press, 1937. 13*s.* 6*d.* net.)

THIS book is described as the "ideal one volume text for an introductory general science course." These introductory notes, many of which make most interesting reading, have been compiled by members of the University of Chicago and are edited by Mr. Moulton, the Secretary of the American Association for Advancement of Science. The sections are as uneven in style as they are in value of content, and will appeal to students in direct ratio to the previous individual acquaintance with the particular subject. The book cannot be read as a text-book, but rather as a reference encyclopædia.

The notion to make a continuous story of science as applied to human life is praiseworthy, though of doubtful use. It is a long journey from galaxies to isotopes. The electricity section is rather elementary than introductory; the biological chapters are overcrowded.

P. J.

The Subject Index to Periodicals, 1937. [Pp. xxix + 584 columns.] (London: The Library Association, 1938. £3 10*s.* net.)

THE increasing extent to which those responsible for the administration of the great libraries in this country, as elsewhere, attempt to help all engaged in scientific and scholarly work is illustrated by the constant expansion of the loans of books and journals made from one library to another and by the reduction of the formalities which were once insisted upon before a stranger was admitted to the reading-rooms. The Library Association is thus well in step with the times in providing in the preliminary pages of its new annual volume of the *Subject Index to Periodicals* a full "Location List" showing in what libraries each of the journals indexed may be found. Notes, printed with suitable prominence, call attention to the right procedure in connection with applications for the loan of any periodical as well as to the justifiable assumption that all those published in the British Isles may be found in the six copyright libraries. The list itself includes public libraries, large and small, University and College libraries, the libraries of Government Departments and Institutions, and those of certain learned societies. Its value is so obvious that it may legitimately be hoped that every possible effort will be made to ensure its strict accuracy by correcting next year the occasional errors and omissions that may be noticed in the present issue.

In other respects the new volume of the *Subject Index to Periodicals* maintains the high standard which all who have used it in the past have been led to expect. It continues to supplement in most useful fashion the more specialised bibliographies issued in many fields of knowledge. Indeed, it is well known that not the least of its merits is its deliberate exclusion of the material already provided elsewhere for the specialist in science and technology.

J. W.

BOOKS RECEIVED

(Publishers are requested to notify prices.)

A Course in General Mathematics. By Harold T. Davis, Indiana University. Bloomington, Indiana: The Principia Press, 1935; London: Williams & Norgate, Ltd., 1938. (Pp. xii + 316, with 43 figures.) 10*s*. 6*d*. net.

Calculus. By Edward S. Smith, M.E., Ph.D., Professor of Mathematics, Meyer Salkover, Ph.D., Associate Professor of Mathematics, Howard K. Justice, C.E., Ph.D., Professor of Mathematics, University of Cincinnati. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. (Pp. xii + 558, with 286 figures.) 16*s*. net.

Coordinate Solid Geometry. Being Chapters I–IX of "An Elementary Treatise on Coordinate Geometry of Three Dimensions." By Robert J. T. Bell, M.A., D.Sc., LL.D., Professor of Mathematics in the University of Otago, Dunedin, N.Z. London: Macmillan & Co., Ltd., 1938. (Pp. xiv + 219, with 46 figures.) 7*s*. 6*d*.

Advanced Analytic Geometry. By Alan D. Campbell, Professor of Mathematics, Syracuse University. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. (Pp. x + 310, with numerous figures.) 20*s*. net.

Solid Mensuration with Proofs. By Willis F. Kern and James R. Bland, Assistant Professors of Mathematics at the U.S. Naval Academy. Second edition. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. (Pp. viii + 172, with frontispiece and numerous figures.) 10*s*. net.

The General Field Theory of Schouten and Van Dantzig. By N. G. Shabde, D.Sc., Professor of Mathematics, Nagpur University. Lucknow University Studies, No. X, Faculty of Science, Session 1937–38. Published by authority of the University's Executive Council, 1938. (Pp. iv + 58.)

Duodecimal Arithmetic. By George S. Terry. London, New York, Toronto: Longmans, Green & Co., 1938. (Pp. viii + 407, with 1 figure.) 30*s*. net.

A Hundred Years of Astronomy. By Reginald L. Waterfield. London: Gerald Duckworth & Co., Ltd., 1938. (Pp. 526.) 21*s*. net.

Starcraft. By William H. Barton, Jr., Executive Curator, Hayden Planetarium, New York City, and Joseph Maron Joseph, Head, Science Department, Smedley Junior High School, Chester, Pennsylvania. New York and London: McGraw-Hill Publishing Co., Ltd., 1938. (Pp. viii + 228, with coloured frontispiece and 139 figures.) 10*s*. 6*d*. net.

The Air and its Mysteries. By C. M. Botley, Fellow of the Royal Meteorological Society. With a Foreword by Sir Richard Gregory, Bart., F.R.S. London: G. Bell & Sons, Ltd., 1938. (Pp. xvi + 296, with 1 coloured plate, 16 black and white plates and 23 figures.) 8*s*. 6*d*. net.

Flight Handbook. A Guide to Aeronautics. By W. O. Manning, F.R.Ae.S., and the Technical Staff of "Flight." London: Flight Publishing Co., Ltd., 1938. (Pp. iv + 146, with 106 figures and 1 folding plate.) 3*s*. 6*d*. net.

Airplane Structures. Vol. II. By Alfred S. Niles, A.B., S.B., Professor of Aeronautic Engineering, Leland Stanford Jr. University, and Joseph S. Newell, S.B., Associate Professor of Aeronautical Structural Engineering, Massachusetts Institute of Technology. Second edition. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. (Pp. viii + 177, with 50 figures.) 13*s*. 6*d*. net.

The Elements of Physics. By Alpheus W. Smith, Ph.D., Professor of Physics, The Ohio State University. Fourth edition. New York and London: McGraw-Hill Publishing Co., Ltd., 1938. (Pp. xx + 790, with 4 coloured plates, 5 black and white plates and 789 diagrams.) 21*s*. net.

A Course of Physics. By Henry A. Perkins, Sc.D., Professor of Physics, Trinity College, Hartford. London and Glasgow: Blackie & Son, Ltd., 1938. (Pp. x + 820, with 1 coloured plate and 621 figures.) 15*s*. net.

Statistical Physics. By L. Landau, Institute for Physical Problems, Moscow, and E. Lifshitz, Ukrainian Physico-Technical Institute, Kharkoff. Translated from the Russian by D. Shoenberg, Royal Society Mond Laboratory, Cambridge. Oxford: at the Clarendon Press; London: Humphrey Milford, 1938. (Pp. viii + 234, with 54 figures.) 20*s*. net.

Physik. Für Studierende an Technischen Hochschulen und Universitäten. Von Ingenieur Dr. Paul Wessel. Herausgegeben von Dr. V. Riederer von Paar. München: Verlag von Ernst Reinhardt, 1938. (Pp. xii + 550, with 277 figures.) RM.4.90.

On Understanding Physics. By W. H. Watson, M.A., Ph.D., F.R.S.C., Assistant Professor of Physics, McGill University. Cambridge: at the University Press, 1938. (Pp. xii + 146.) 7*s*. 6*d*. net.

Concise School Physics. Heat, Light and Sound. By R. G. Shackel, M.A., Head of the Physics Department, St. Olave's Grammar School, London. London, New York, Toronto: Longmans, Green & Co., 1938. (Pp. viii + 301, with 187 figures.) 3*s*. 6*d*.

A Complete Revision Physics. Notes and Questions. Hydrostatics, Heat, Light, Sound, Magnetism and Electricity. By S. R. Humby, M.C., M.A., and F. W. Goddard, M.A., F.C.S., The Science School, The College, Winchester. London, New York, Toronto: Longmans, Green & Co., 1938. (Pp. viii + 303, with 166 figures.) 3*s*. 9*d*.

A Treatise on Light. By R. A. Houstoun, M.A., D.Sc., F.Inst.P., Lecturer on Physical Optics in the University of Glasgow. Seventh edition. London, New York, Toronto: Longmans, Green & Co., 1938. (Pp. xii + 528, with 2 coloured plates, 8 black and white plates and 345 figures.) 14*s*. net.

Light. By F. Bray, M.A., late Science Master at Clifton College. Second edition. London: Edward Arnold & Co., 1938. (Pp. xii + 369, with 8 plates and 245 figures.) 7*s*. 6*d*.

Electron and Nuclear Physics. By J. Barton Hoag, Ph.D., Assistant Professor of Physics, University of Chicago. Second edition of "Electron Physics." New York: D. van Nostrand Co., Inc.; London: Chapman & Hall, Ltd., 1938. (Pp. x + 502, with 264 figures.) 20*s*. net.

Electromagnetics. A Discussion of Fundamentals. By Alfred O'Rahilly, Professor of Mathematical Physics, University College, Cork. With a Foreword by Professor A. W. Conway, F.R.S. London, New York, Toronto: Longmans, Green & Co.; Cork: Cork University Press, 1938. (Pp. xii + 884, with 73 figures.) 42*s*. net.

Engineering Electronics. By Donald G. Fink, Managing Editor, Electronics. New York and London: McGraw-Hill Publishing Co., Ltd., 1938. (Pp. xiv + 358, with 217 figures.) 21*s*. net.

Radio-Frequency Electrical Measurements. A Guide for Radio Engineering Laboratory Instruction. By Hugh A. Brown, M.S., E.E., Associate Professor of Electrical Engineering, University of Illinois. Second edition. New York and London: McGraw-Hill Publishing Co., Ltd., 1938. (Pp. xvi + 384, with 177 figures.) 24*s*. net.

Electrical Engineering Practice. Vol. I. By J. W. Meares, C.I.E., F.R.A.S., M.Inst.C.E., M.I.E.E., late Electrical Adviser to the Government of India and Chief Engineer, Hydro-Electric Survey of India, and R. E. Neale, B.Sc.Hons., A.C.G.I., A.M.I.E.E., David Salomons Scholar, Siemens Medallist. Fifth edition. London: Chapman & Hall, Ltd., 1938. (Pp. xii + 780, with 48 plates and 92 figures.) 25*s*. net.

Costs and Tariffs in Electricity Supply. By D. J. Bolton, M.Sc., M.I.E.E. London: Chapman & Hall, Ltd., 1938. (Pp. xii + 214, with frontispiece and 49 figures.) 12*s*. 6*d*. net.

Problems in Electrical Engineering. With Answers. Edited by S. Parker Smith, D.Sc., M.I.E.E., Assoc.M.Inst.C.E., Professor of Electrical Engineering, The Royal Technical College, Glasgow. Third edition. London: Constable & Co., Ltd., 1938. (Pp. xxiv + 267.) 6*s*. net.

Motion Picture Sound Engineering. A Series of Lectures presented to the classes enrolled in the courses in Sound Engineering given by the Research Council of the Academy of Motion Picture Arts and Sciences, Hollywood, California. New York: D. van Nostrand Co., Inc.; London: Chapman & Hall, Ltd., 1938. (Pp. xx + 547, with 372 figures and 35 charts.) 30*s*. net.

Das Aufbauprinzip der Technik. Von Ingenieur Dr. techn. P. Wessel, Assessor für Physik und Mathematik. München: Verlag von Ernst Reinhardt, 1937. (Pp. 39, with 14 figures.)

Modern Aspects of Inorganic Chemistry. By H. J. Emeléus, D.Sc., A.R.C.S., and J. S. Anderson, Ph.D., A.R.C.S. (of the Imperial College of Science and Technology, London). Twentieth-Century Chemistry. London: George Routledge & Sons, Ltd., 1938. (Pp. xii + 536, with 53 figures.) 25*s*. net.

General Chemistry. An Introductory Course of Lessons and Exercises in Chemistry. By Eugene P. Schoch and William A. Felsing, Professors of Chemistry, The University of Texas. International Chemical Series. New York and London: McGraw-Hill Publishing Co., Ltd., 1938. (Pp. x + 524, with 74 figures.) 18*s*. net.

Gmelins Handbuch der anorganischen Chemie. 8. Auflage. Herausgegeben von der Deutschen Chemischen Gesellschaft. System-Nummer 22: Kalium, Lieferung 5. Berlin: Verlag Chemie, G.m.b.H., 1938. (Pp. 142, with 11 figures.) Reduced price abroad, RM. 19.50.

Gmelins Handbuch der anorganischen Chemie. 8. Auflage. Herausgegeben von der Deutschen Chemischen Gesellschaft. System-Nummer 25: Caesium, Lieferung 1. Berlin: Verlag Chemie, G.m.b.H., 1938. (Pp. 104, with 3 figures.) Reduced price abroad, RM.12.–.

Gmelins Handbuch der anorganischen Chemie. 8. Auflage. Herausgegeben von der Deutschen Chemischen Gesellschaft. System-Nummer 63: Ruthenium. Berlin: Verlag Chemie, G.m.b.H., 1938. (Pp. xxvi + 124, with 1 figure.) Reduced price abroad, RM. 16.50.

An Introduction to Chemistry. By John Arrend Timm, Assistant Professor of Chemistry, Fellow of Trumbull College, Yale University. Third edition. With a Foreword by John Johnston, Director of Research, United States Steel Corporation. New York and London: McGraw-Hill Publishing Co., Ltd., 1938. (Pp. xx + 568, with 162 figures.) 21*s*. net.

A Higher School Certificate Inorganic Chemistry. By G. H. J. Adlam, O.B.E., M.A., B.Sc., Senior Science Master, City of London School, Editor of "The School Science Review," and Leslie Slater Price, M.A., B.Sc., A.I.C., Bradfield College. London: John Murray, 1938. (Pp. viii + 624, with 87 figures.) 8*s*. 6*d*.

Crystal Chemistry. By Charles W. Stillwell, Ph.D., Research Chemist, Dennison Manufacturing Company. International Chemical Series. New York and London: McGraw-Hill Publishing Co., Ltd., 1938. (Pp. x + 431, with 72 figures and 1 folding plate.) 25*s*. net.

Inorganic Colloid Chemistry. Vol. III: The Colloidal Salts. By Harry Boyer Weiser, Professor of Chemistry at The Rice Institute. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. (Pp. viii + 473, with 74 figures.) 30*s*. net.

Chemical Kinetics. By Farrington Daniels, Professor of Chemistry in the University of Wisconsin. The George Fisher Baker Non-Resident Lectureship in Chemistry at Cornell University. Ithaca, New York: Cornell University Press; London: Humphrey Milford, 1938. (Pp. viii + 273, with 41 figures.) 15*s*. net.

The Elements of Physical Chemistry. By F. W. Goddard, M.A., F.C.S., and E. J. F. James, M.A., D.Phil. London, New York, Toronto: Longmans, Green & Co., 1938. (Pp. viii + 251, with 67 figures.) 5*s*.

Reaction Kinetics. A General Discussion, held by The Faraday Society, September 1937. London: Gurney & Jackson, for the Society. (Pp. ii + 268, with numerous figures.) 12*s*. 6*d*. net.

Textbook of Organic Chemistry. By George Holmes Richter, Assistant Professor of Organic Chemistry, The Rice Institute. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. (Pp. viii + 711.) 20*s*. net.

Problems in Organic Chemistry. By Ernest Hamlin Huntress, Ph.D., Associate Professor of Organic Chemistry, Massachusetts Institute of Technology. International Chemical Series. New York and London: McGraw-Hill Publishing Co., Ltd., 1938. (Pp. xii + 270.) 12*s*. 6*d*. net.

Organic Syntheses. Vol. XVIII. Reynold C. Fuson, Editor-in-Chief. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. (Pp. vi + 103.) 8*s*. 6*d*. net.

Experiments in Organic Chemistry. By Lucius Junius Desha, Professor of Chemistry, and Larkin Hundley Farinholt, Associate Professor of Chemistry, Washington & Lee University. International Chemical Series. New York and London: McGraw-Hill Publishing Co., Ltd., 1938. (Pp. xii + 233, with 23 figures.) 10*s*. 6*d*. net.

Chemistry of the Proteins. By Dorothy Jordan Lloyd, M.A., D.Sc., F.I.C., Director of the British Leather Manufacturers' Research Association, and Agnes Shore, B.Sc., A.I.C., Demonstrator, Physiology Dept., London (Royal Free Hospital) School of Medicine for Women. Second edition. With an Introduction by Sir Frederick Gowland Hopkins, O.M., M.B., D.Sc., F.R.C.P., F.R.S. London: J. & A. Churchill, Ltd., 1938. (Pp. xii + 532, with 101 figures, including 3 plates.) 21*s*. net.

British Chemical Industry. Its Rise and Development. By Sir Gilbert T. Morgan, O.B.E., D.Sc., Sc.D., LL.D., F.R.S., F.I.C., A.R.C.Sc., Director of Chemical Research, and David Doig Pratt, M.A., Ph.D., Principal Scientific Officer of the Chemical Research Laboratory, Teddington; Department of Scientific and Industrial Research. London: Edward Arnold & Co., 1938. (Pp. xii + 387, with 32 plates and 79 figures.) 21*s*. net.

The Elements of Ferrous Metallurgy. By Joseph L. Rosenholtz, Ph.D., Rensselaer Polytechnic Institute, and Joseph F. Oesterle, Ph.D., The University of Wisconsin. Second edition. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. (Pp. viii + 258, with 140 figures.) 15*s*. net.

A Manual of Foundry Practice. By J. Laing, A.M.I.Brit.F., Works Manager, Messrs. John Hall & Son (Oldham), Ltd., Oldham, and R. T. Rolfe, F.I.C., M.I. & S. Inst., M.Inst.Met., Chief Metallurgist, Messrs. W. H. Allen, Sons & Co., Ltd., Bedford. Second edition. London: Chapman & Hall, Ltd., 1938. (Pp. viii + 312, with 162 figures, including 47 plates.) 18*s*. net.

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SCIENCE PROGRESS

SOME BIOLOGICAL ASPECTS OF THE STORAGE OF FRUIT

BY V. H. BLACKMAN, Sc.D., F.R.S.

THE methods of canning and of refrigeration have brought about a great extension of the time during which food can be kept in good condition. Food supplies are in this way rendered available in countries far distant from the place of origin of the plant or animal product. Previous to the development of these processes the only methods available were the crude processes of salting and drying; and our ancestors were largely dependent in winter upon salted and dried meat, eked out with such fresh supplies as the fishponds and dovecotes provided. How poorly even noble families fared in winter is seen in the quotation from the *Northumberland Household Book* to which Sir William Hardy drew attention some years ago:

> Braikfast for the Nurcey for my Lady Margaret and Maister Ingeram Percy. Item a Manchet a Quarte of Bere a Dysch of Butter a Pece of Salt-fisch a Dysch of Sproites or iij White Herryng. Breakfast for my Ladis Gentyllwomen. Item a Loof of Brede a Pottell of Bere a Pece of Saltfisch or iij White Herryng.

Even the sprats and herrings would be salt, and fresh plant products were poorly available in winter until the development of winter vegetables by Bakewell in the eighteenth century.

The processes of canning and refrigeration are of course very different. In canning and bottling heat is used to destroy the micro-organisms which are responsible for the deterioration and decay of the material, the access of fresh living ones being prevented by sealing the hot and now dead material in metal or glass containers. The process of refrigeration is entirely distinct, for it depends on the opposite process of exposure to a low temperature which, though not destroying, markedly slows down the action of the micro-organisms of decay. This, however, is not all the difference. While canned material is dead, and the chemical processes

which would otherwise continue in it are completely or all but completely stayed, the refrigerated material is frequently living, as in the case of fruit, and is thus the seat of more or less active physical and chemical changes. Care must of course be taken to avoid so low a temperature that death ensues. For with death the catalysts (enzymes) are released from the "restraint" that directs them along highly specialised paths of co-ordinated activity, though of the nature of this restraint we are profoundly ignorant. The catalysts when no longer restrained produce in the dead material post-mortem chemical changes which markedly affect its consistence, its flavour and its resistance to the attack of micro-organisms.

The discovery of the process of canning markedly antedates the use of refrigeration. The stimulus to the discovery came from Napoleon who, no doubt for military reasons, offered in 1795 through the French Government a prize to anyone discovering a new method of preserving food. The prize, as is well known, was won by Nicholas Appert, who developed a method of sealing food in glass containers; though an Englishman, Durand, seems to have been responsible for replacing with tin plate the glass of the container.

Canned food though of high nutritive value is necessarily, from its heat preparation, in a condition markedly different from that of the fresh state. The aim of refrigeration of fruit and meat, on the other hand, is to achieve the quality of *freshness*, *i.e.* to deliver food products to the consumer in the condition in which normally it has the maximum gastronomic attractiveness and also the maximum of certain food values. The freshness of edible plant products is usually very transient since they are living and therefore in a state of constant change, either moving upward to maturity or progressing slowly to old age.

The edible fruits with which man concerns himself are generally those with a succulent tissue, the "flesh," in which the seeds are either enclosed (*e.g.* the apple) or supported (*e.g.* the strawberry). A fruit thus has no "future" and undergoes no further development; its only biological advantage to the plant which bore it is as a distributor of the seeds. The mature fruit if not eaten falls more or less rapidly into a state of decay; it is thus a *senescent* organ. The object of storage is to slow down the rate of the biological processes responsible for decay. The processes to be retarded are frequently those which lead up to maturity as well as those which follow it, for the fruit is frequently picked and stored in an unripe condition. The result of such retardation is that the changes normally occurring in a single day are spread

over a week or weeks, so that if the material has to be carried to a distant centre the effect is the same as if the period of transport had been similarly reduced.

Relation of Fruit to Its Environment

Any modification of natural conditions which is directed to slow down the various processes in the fruit must be related to the living nature of the material. The fruit like all active living things is in constant interaction with its environment. There are five main interactions. The fruit (1) gives off water vapour from its surface, (2) absorbs oxygen, (3) gives off carbon dioxide, (4) emits heat, and (5), a discovery of more recent years, gives out small quantities of volatile products which may be of great physiological importance. In a store, accordingly, there tends to be an accumulation of water vapour, of carbon dioxide and of volatile products, and in addition a depletion of oxygen. The absorption of oxygen and the excretion of carbon dioxide are part of the phenomenon of respiration, which is characteristic of all active living organisms, though in some few cases we find carbon dioxide production without the concomitant absorption of oxygen.

Respiration.—The process of respiration has been found to be a measure of the " vital " activity of the organism ; as respiration is slowed or quickened so are the general chemical processes (*i.e.* the metabolism) of the plant. The production of carbon dioxide in respiration can thus be used as a measure of biological activity, and a retardation of respiration indicates a delay in the occurrence of senescent conditions. Furthermore the level of the respiration rate often gives an indication of the stage of maturity of the fruit, certain stages of ripeness being associated with certain respiratory levels. It is thus not surprising that the process of respiration has been intensively studied. In addition, as will be seen, the process of respiration has itself been turned to account in fruit storage, the products of the process being employed to retard the process.

The Effect of Low Temperature on Respiration and on Storage

It has been known for a long time that the rate of various physiological processes in the plant (such as respiration and assimilation) show a relationship to temperature somewhat similar to that exhibited by chemical reactions, many of which exhibit a marked " temperature coefficient." The similarity of the relationship is

only approximate, since the processes in the living organism are not dependent on single chemical reactions but on many related reactions, the rates of which are dependent not only on temperature but on the concentration of the reacting substances, and also, in all probability, on the condition of special surfaces in the living cell. We do, however, find that, within a comparatively small range of temperature, the rate of respiration is about doubled or trebled for a rise of 10° C., or, in other words, the temperature coefficient (Q_{10}) is 2–3. The respiration and general metabolism of the apple have been most exhaustively studied by Kidd and West over a period of many years at the Low Temperature Research Station, Cambridge, and it has been demonstrated that there is a marked similarity between the effect of temperature on the rate of respiration and on the "storage life" of apples. Some of their data are quoted below for the variety of apple Bramley's Seedling, the rate of respiration being expressed in cubic centimetres of carbon dioxide per 10 kg. per hr.

Temperature (° C.) .	− 1	+ 1	3	5	10	15	22·5
Respiration Rate	14·5	19·6	22·5	29·8	47·0	64·9	115·2

It can be seen that the curve is roughly a logarithmic one, the rate a little more than doubling for each rise of 10°. The data, indeed, fit fairly closely the equation

$$\log \text{respiratory rate} = 1{\cdot}249 + 0{\cdot}0375\,\theta,$$

where θ is the temperature. Calculating from this the value of Q_{10}, the temperature coefficient, is found to be 2·37.

When apples are stored at different temperatures we find that the time they continue in a satisfactory condition, that is their "storage life," increases as the temperature falls if there are no injurious effects of temperature. Sooner or later, however, the fruit is attacked by fungi, and as the individual apples show great variability in this respect it is necessary to take some arbitrary level of wastage as the limit of the storage life of the population. Commercially this is taken as the time of storage when there is a wastage of 10 per cent.; the "mean" storage life would be at a level of 50 per cent. wastage. The data given below show for different temperatures the storage life at the 10 per cent. and the 50 per cent. level for four different varieties of apple.

		Storage Life (Days).						
		− 1°	+ 1°	3°	5°	7·5°	12°	18° C.
Cox's Orange Pippin .	50% loss	98	103	142	145	155	92	50
	10% ,,	75	75	75	81	86	53	24
Allington Pippin . .	50% ,,	110	177	250	214	143	63	42
	10% ,,	58	87	103	96	61	37	21
Blenheim Orange . .	50% ,,	49	95	200	181	137	89	56
	10% ,,	17	50	140	136	103	64	42
Lane's Prince Albert .	50% ,,	169	198	197	184	147	108	60
	10% ,,	99	130	115	113	95	53	28

At the lower temperatures the results are complicated by a direct, injurious effect of temperature known as "low-temperature breakdown" and the storage life is usually less than at *slightly* higher temperatures. If, however, the mean life (50 per cent. loss) at 18° C. is compared with that at 3° or 5° C. the period of 6–8 weeks at the higher is lengthened to 5–8 months at the lower temperature. These data show also a logarithmic relation and can be treated in the same way as the respiration data and an appropriate equation fitted. For the variety Allington Pippin and the temperatures 3°–18° C. the following equation is found :—

$$\log \frac{1}{t} = -2{\cdot}55 + 0{\cdot}0510\,\theta,$$

where t is the time in days and θ is the temperature. From this the temperature coefficients for the *rate of deterioration* can be calculated, Q_{10} for Allington Pippin being 3·27, Cox's Orange Pippin 2·88, Blenheim Orange 2·45, and Lane's Prince Albert 2·32. It is to be noted that these coefficients are of the same order as that for the relation between temperature and respiration rate where Q_{10} was found to be 2·37. The reason for the marked difference in the "life" of the different varieties is still obscure.

Phases of Respiration.—Fruits naturally pass through various stages during their development and later life. We can distinguish the three main phases indicated below :

PHASE OF GROWTH ⟶ TRANSITION PHASE ⟶ PHASE OF SENESCENCE

In the phase of growth the fruit is still attached to the tree and is supplied with water, salts and nutritive organic substances. There is increase in size due in part to cell division and in part to increase in size of pre-existing cells, and in the apple the concentration of starch, cane sugar and reducing sugar increases. In the final phase the fruit is an independent structure and the consump-

tion of stored substances such as malic acid and carbohydrates indicates that it is on the downgrade towards death; this is the *senescent stage*. Between these two we have the transitional stage.

The rates of respiration in the fruit which has been most closely studied, the apple, show very interesting and remarkable phenomena. As the fruit increases in size on the tree the rate of respiratory activity—as measured by carbon dioxide production *per unit of fresh weight*—falls rapidly at first and then more slowly, and during the last stage of growth is falling very slowly indeed. Sooner or later after gathering, and at a time dependent upon the maturity of the fruit when gathered and upon the external conditions, the rate of respiration begins to increase, rises rapidly or slowly to a maximum, and then falls as the fruit progresses towards death. This striking rise in respiration which appears sooner or later in all apples and is due to internal causes, marks the beginning of the senescent phase and is known as the *climacteric*. We can thus distinguish three stages in the respiratory life of the apple which are shown diagrammatically below:

PRE-CLIMACTERIC.	CLIMACTERIC.	POST-CLIMACTERIC.
Respiration rate falling	Respiration begins to rise	Respiration rises to a maximum and falls again

By the time the rise in the respiration rate, which begins at the climacteric, has reached its maximum in the post-climacteric stage the level is 50 per cent. to 150 per cent. above the pre-climacteric level immediately preceding the climacteric. The climacteric rise in apples represents a remarkable outburst of metabolic activity. Some change must occur in the living cells at this stage but its nature is still obscure. We do know, however, that this climacteric rise is associated with additional output of volatile products from the fruit.

In relation to low-temperature storage it is important to note that the time of appearance of the climacteric rise is mainly determined by the age of the fruit, being little affected by temperature reduction. On the other hand a low temperature markedly flattens out the rising respiration curve in the post-climacteric phase, and *so greatly delays the development of the maximum respiration rate.* It is during the post-climacteric phase that full ripeness of the fruit develops, so that with the delay in the development of the respiratory maximum there is associated the prolongation of storage life already described as the result of lower temperature.

The Action of Ethylene

The importance of ethylene in the ripening of fruits was first realised a little under thirty years ago and its discovery is one of the most interesting chapters in biology. It begins with the attempts to colour their fruit by the lemon growers of California. This lemon is picked when it reaches a certain size and there is always a certain proportion of green fruit. The growers, therefore, attempted to expedite yellowing by placing the fruit in chambers or tents heated artificially; in these the proper colour was attained in 1 or 2 weeks. It was naturally assumed that the result achieved was due to the warm, moist condition of the chambers, but there was the curious fact that the success seemed to depend on the *method* of heating. In 1912 it was demonstrated that the important factor was not the *heat* developed but the *products of combustion.* In fact the effect could be achieved without the use of heat by passing the stove gases over the lemons, and the exhaust gases of a motor-car were found to have a similar effect. It was obvious that there was some common substance or substances in these combustion gases which had a remarkable physiological action. In attempting its discovery the yellowing of lemons was used as the biological test of its occurrence, the gases being passed through various solutions and then over the fruit. Sulphuric acid, sodium hydroxide, potassium permanganate and silver nitrate failed to hold back the active principle, but on the other hand bubbling through bromine water deprived the gases of their specific power of coloration. This suggested the presence of some unsaturated volatile hydrocarbon and it was found that minute amounts of ethylene would produce all the effects. A concentration of 1 in 200,000 would colour lemons in 5–8 days, with 1 in 2×10^6 it took 6–10 days and with 5 in 10^7 it required 14 days. It was also observed that ethylene increased the rate of respiration 150 to 250 per cent., the significance of which in relation to the climacteric rise of apples was not then realised. Since the discovery of the potency of almost incredibly small quantities of ethylene the substance has been widely used commercially for treating horticultural produce. The compressed gas can be slowly released from cylinders in railway trucks and so mature the product during transport.

Recently another chapter has been added to the story of ethylene which seems to show that after all man has been merely taking a leaf from nature's book when using the gas for speeding the process of ripening of fruit. The discovery, as so often happens, was due to a chance observation. In 1932 Elmer in America reported that

some apples which had been accidentally included in a potato clamp inhibited the sprouting of the tubers or caused them to sprout abnormally. This clearly indicated that the apples gave off some volatile substance, some emanation, which acted on the potatoes. These results were confirmed and remarkably extended in the same year by workers at the Low Temperature Research Station, Cambridge (Report of the Food Investigation Board, 1932). The work of Elmer was confirmed by Huelin and it was shown that the same result could be obtained by exposure to air containing 1 part in 100,000 of ethylene as the results given below indicate.

Action of Ethylene on Potatoes after 4 Weeks

Concentration.	Max. Length of Sprouts (cm.).	Wt. of Sprouts (12 Tubers).
Control	3·0	7·5
0·001	1·7	4·5
0·01	0·8	1·1

An important step forward was also made by Smith and Gane, who showed that if a ripe apple was enclosed in a vessel with germinating peas, or if the air which had passed over ripe apples was led over such peas, there was a remarkable effect. Either germination was almost entirely inhibited or the shoots grew abnormally, becoming "short, stumpy and swollen." A valuable *biological* indicator for the presence of the emanation was thus available, and using this "pea test" the effect of various solutions could be tested. Solutions such as those of caustic soda, potassium permanganate, silver nitrate, olive oil, etc., did not stop the effect, but bromine water, ozone, and combustion over copper oxide removed the active constituent; results which are consistent with ethylene as the responsible agent. At the same time Kidd and West, by the use of the pea test, made the important discoveries (1) that the production, or at least the main production, of the exhalation from the apple coincides with the rise in respiration at the climacteric, (2) apple vapour passed over other fruit in the pre-climacteric stage quickly induces the climacteric rise. The surprising fact is thus revealed that *in a population of stored apples there is some sort of "social" interaction, those that ripen first influence their fellows by their exhalation and bring them rapidly to maturity.* Accordingly, we find that apples stored together all ripen at the same time, while with the same fruit isolated as individuals the appearance of the climacteric is spread over a considerable time. The difference between "bulk" storage and separate storage is

well seen in the data below, which give the number of days after storage at which the maximum of the climacteric respiratory rise was achieved in the case of 30 apples all stored together, and for the same number of apples stored in fifteen separate pairs; all at 12° C.

	Time to Climacteric Maximum.
Bulk storage	23 days
Storage in pairs	33 ,,

It was observed that ethylene had the same effect as apple vapour in bringing on the climacteric rise and that the vapour of bananas, peaches and pears (but not oranges or grapes) gave off the same activating vapour as did apples. The observations relating to bananas gave the explanation of a biological phenomenon in the overseas transport of that fruit which has for some years been a mystery. As is well known the fruit is stored in the immature green state in the ship's hold, and it has been found by experience that if any fruits mature in transport they should immediately be removed or ripening will set in throughout the population. This extraordinary action-at-a-distance, which should immediately have suggested the working of some volatile substance, is now explained, for it is easy to bring on the climacteric in bananas by exposing them to the vapour from another individual in the ripe condition.

Since the discovery of the similarity of the action of apple vapour and of ethylene the latter substance has been demonstrated in apple vapour, and it has been shown that its production is not confined to the climacteric and post-climacteric stages but that it occurs also in the immature pre-climacteric stage though in less amount. The tissues of the apple, and also the pear, are thus exposed to self-produced ethylene for some time before the climacteric rise sets in. The conditions which initiate the climacteric under natural conditions are obscure. It may be that with slow metabolic change in the tissues of the fruit there is an increased production of the substance, thus raising the concentration to a sufficiently high level, or with the metabolic change there may be a heightened physiological response so that the tissues react to a concentration of ethylene to which they were formerly inactive. When a small part of the tissue has been stimulated it will produce more ethylene and this will diffuse through the intercellular spaces and so " activate " other parts of the fruit and the process of *auto-stimulation* will continue at an accelerating rate on the compound-interest principle.

Quantity of Volatile Substances.—The gases given off by fruits

are water vapour, carbon dioxide and various volatile organic substances which include esters, essential oils and, in some cases, ethylene. The volatile organic substances can be determined by passing the vapours over red-hot copper oxide and determining the carbon dioxide thus produced. There is evidence that the production of the volatile organic substances runs parallel with that of the carbon dioxide of respiration as is shown by the data (from R. Gane) given below.

CARBON DIOXIDE FROM RESPIRATION AND FROM VOLATILE ORGANIC SUBSTANCES OF PEARS AT 15° C.

(mg. per kg. per hr.)

Days from Storage.	CO_2 of Respiration.	CO_2 of Combustion.
11	8	0·05
25	16	0·2
40	40	0·7
50	18	0·1

Eleven days after storage 1 ml. of ethylene was added to the gas stream passing over the fruit so as to bring all the fruit to maturity together. The maximum both of respiration and of the production of organic substances was reached at 40 days. In Newton Wonder and Worcester Pearmain apples at 15° C. the production of combustible substances was almost constant over 20 days. The ratio of the carbon dioxide from the combustion of the volatile products to that of respiration was 1 : 70 and 1 : 65 in the two cases.

Scald.—Whenever fruit is kept in closed chambers whether in cold store or gas store there is an accumulation of volatile organic substances. One of the results of closed storage is the appearance in susceptible varieties of an injury to the apple, a browning of the surface, known as "scald." The nature of the injury is still unknown, but it appears to be due to an accumulation at the surface of the fruit of some volatile substance formed in the inner tissues and probably passing out through the lenticels. Curiously, however, the empirical fact is known that by suitable "wrapping" of the apple the injury can be avoided, a discovery made in U.S.A. The most suitable wrap is a paper impregnated with a mineral oil (which should be tasteless and odourless) to an amount of 15–20 per cent. of the original weight of the paper; paper itself if sufficiently thick (4 gm. of paper per apple) will produce the same effect. The theory is that scald is due to some volatile product accumulating in excess in the surface tissues, and that low tem-

perature favours this accumulation while the oil wraps facilitate its escape, presumably by absorbing it and so increasing the gradient round the fruit. The fact that intermittent warming of the fruit (1 day at 15° C. every 14 days) will control the injury in a store held at 3° C. supports this view. The real nature of scald is a fascinating biological problem.

Gas Storage

As has been pointed out there is a close negative correlation between the respiratory activity of fruit and its storage life; thus the slowing of respiration and the concomitant increase in expectation of life can be brought about by a lowering of temperature. Accepting the process of respiration as a chemical one in which complex organic substances are oxidised to carbon dioxide and water one would expect that an accumulation of the end products would reduce the rate of the reaction. A "back pressure" of carbon dioxide, *apart from temperature reduction*, should slow down respiration and so slow down metabolic processes and prolong life. This has been found to be the case, and the effect is the basis of modern "gas storage" which has been developed by Kidd and West of the Low Temperature Research Station. In the process of gas storage the fruit is stored in a closed chamber with the result that carbon dioxide accumulates in the surrounding atmosphere and oxygen diminishes. Necessarily, since the volume of carbon dioxide produced is roughly equal to that of oxygen absorbed the *sum* of the percentage concentrations of the two gases will remain about 21, the original concentration of oxygen. If the carbon dioxide has accumulated to 13 per cent. then the oxygen will be found to be about 8 per cent. provided there is no leakage. The principle of gas storage is to allow the fruit to *narcotise itself* by the accumulation of one of its own waste products, namely carbon dioxide. The store, however, cannot be simply left closed as one gas would then pile up and the other be depleted to a dangerous extent, for the fruit is injured if the carbon dioxide and oxygen rise and fall within more than a limited range. This difficulty is got over by "regulated ventilation" which admits a certain amount of air from the outside, thus raising the concentration of one gas and lowering that of the other.

It is found that the lowering of the respiratory rate is due not only to the accumulation of carbon dioxide but the reduction of oxygen also plays a part, as is to be expected on general chemical grounds. It has also been discovered that although the system of controlled ventilation is adequate for Bramley's Seedling apples

which are suited by an atmosphere of 10 per cent. carbon dioxide and 11 per cent. oxygen, most other important varieties unfortunately require an atmosphere which cannot be achieved by replacing the oxygen of the air by an equal volume of carbon dioxide. That valuable variety, Cox's Orange Pippin, requires, for example, an atmosphere containing approximately 5 per cent. carbon dioxide and 5 per cent. oxygen. To achieve such a reduction of oxygen by the respiration of the fruit leads to an excess of carbon dioxide, so it is necessary to introduce some system by which the unwanted excess of gas is abstracted by chemical absorption. Furthermore, it has been found most satisfactory to combine gas storage with some degree of refrigeration, the effect of the special atmosphere surrounding the fruit being greater at a lower temperature; thus the modern method is now "refrigerated gas storage."

Refrigerated gas storage has a number of advantages over ordinary cold storage in air. There is the *combined* effect of the special atmosphere and the low temperature both increasing the life of the fruit. With storage in air many varieties of apples are liable to "low-temperature breakdown," a direct physiological effect of the reduced temperature; in gas storage the temperatures employed are not low enough to produce the breakdown. With these and other advantages, such as the destruction of *Tortix* larvæ, it is not surprising that the gas stores in this country have risen in capacity from a few thousand cubic feet in 1929 to over 2 million in 1936.

The physiological aspects of gas storage are still somewhat obscure. The chemical reactions of the respiratory process are so complex and the conditions under which it occurs are so highly specialised that there is not likely to be any simple explanation of the inhibiting effect upon respiration which the special atmosphere exerts. One interesting physiological result has recently come to light which shows carbon dioxide in another rôle than that of depressant. Commercial gas stores have on occasions shown a concentration of carbon dioxide unexpectedly high during the first few days after closure. Laboratory experiments have shown that in the pre-climacteric stage concentrations of carbon dioxide of the order of 10 per cent. may cause a *rise* in the respiratory rate which lasts, however, for a few days only and is greater with more immature fruit. In some Bramley's Seedlings stored at 10° C. in an atmosphere of 10 per cent. carbon dioxide and normal oxygen content the transitory rise was of the order of 50 per cent. in fruit so treated on August 25, and of 30 per cent. on September 4. The fact that increased tension of carbon dioxide though usually depress-

ing respiration may at some stages of growth actually raise it show how complex must be the respiratory relationships in the living fruit. This complexity is also indicated by the analytical data given by Kidd and West for the twelfth and thirtieth day of storage of a single mature Bramley's Seedling apple held at 22·5° C. The numbers relate to 100 gm. of fresh weight.

LOSS OF MATERIAL AND RESPIRATION OF APPLE

Loss of dry weight	0·85 gm.	CO_2 produced	619
" " carbohydrate	0·62 "	O_2 uptake	450
" " acid	0·35 "	$CO_2 - O_2$ ratio	1·38

On the basis of oxidation of the carbohydrate and of the acid, 692 c.c. of carbon dioxide should have been produced and 635 c.c. of oxygen taken up (ratio 1·09); both gases, however, are lower, the oxygen very considerably. The fact that the loss of dry weight is less than the amounts of carbohydrate and acid that have disappeared also suggests that neither of these substances is completely oxidised.

INTERNAL ATMOSPHERE OF THE APPLE AND ITS ANATOMY

It might be thought that the structure of the fruit was of little importance in scientific problems of fruit storage, which would seem at first sight to be solely physiological. Physiological conditions may, however, depend upon structure, and this is clearly seen in relation to the internal atmosphere of the fruit. In a gas store *it is not the atmosphere of the store to which the fruit reacts but its own internal atmosphere.* The living cells of the flesh of the fruit receive their oxygen from, and pass their carbon dioxide to, a system of intercellular spaces which permeate the flesh and communicate with the external atmosphere by means of pores (lenticels) in the waxy and more or less impermeable skin, though there may be some interchange of gases at the "calyx end" of the apple. Since the channels of the system of intercellular spaces are very narrow and their communication with the external air not very free, it is not surprising to find that in these spaces the concentration of carbon dioxide is always higher, and the concentration of oxygen always lower, than that of the external atmosphere.

Owing to the importance of apple structure, considerable studies have been made of the fruit anatomy by W. H. Smith of the Ditton Laboratory of the Department of Scientific and Industrial Research. There is a considerable variation in the size of the cells of the flesh of the apple, the size tending to increase radially from the core. A correlation between size of fruit and of cell was found in a population of Bramley's Seedling apples which ranged in weight

from 67 to 463 grammes. The mean cell size for a number of varieties has been estimated together with the calculated number of cells per gm. of cortex, the sp. gr. of the cell being taken as 1·10 and the volume of intercellular space allowed for. The respiratory activity is given in terms of c.c. per 10 kg. per hr. at 12° C.

	Mean Wt. (gm.).	Cell Size. (mm.^{3}).	No. of Cells per gm.	Respiratory Activity.
Bramley's Seedling	220	0·0053	180,000	50–55
Allington Pippin	135	0·0033	275,000	85–90
Cox's Orange Pippin	87	0·0029	315,000	90–95
James Grieve	120	0·0024	380,000	85–90
Grimes Golden	65	0·0018	505,000	110
Worcester Pearmain	82	0·0018	505,000	120
Beauty of Bath	71	0·0014	650,000	155

It will be seen that as the number of cells per unit of weight increases the respiratory activity rises. The summer varieties tend to have large numbers of cells and a high respiratory rate, while the winter varieties have a smaller number of cells and a lower rate.

From the above data it is possible to calculate the respiratory activity per cell, and some results are shown below.

Calculated Respiratory Activity per Cell at 12° C.
(mg. per hr. $\times 10^8$)

Allington Pippin	6·4	Beauty of Bath	4·8
Bramley's Seedling	5·8	James Grieve	4·6
Cox's Orange Pippin	5·8	Grimes Golden	4·4
Worcester Pearmain	4·8	Lord Derby	3·2

There is some suggestion that smaller cell size means greater activity, and the greater relative cell surface of the small cells may play a part in the respiratory differences; there may also be characteristic physiological differences in the cells of different varieties. It must also be remembered that the respiration is that of the apple as a whole and the apples may vary in the relative amount of the less active core.

Intercellular Space and Atmosphere.—By removing cylinders of apple flesh and placing them in closed chambers in which pressure-volume changes can be followed in comparison with similar cylinders of non-porous material the volume of intercellular space can be calculated. Some of the results for different varieties are given below, and for comparison that of a potato.

Intercellular Volume of Apple
(Per cent. of volume of tissue)

Sturmer Pippin	22·2	Bramley's Seedling	28·1
Lane's Prince Albert	24·4	Lord Derby	35·9
Cox's Orange Pippin	26·6	Potato	2·8

The differences of Lord Derby over Sturmer Pippin are over 50 per cent., and the great efficiency of the aerating system of the apple compared with that of the potato is well seen.

With regard to the intercellular atmosphere differences of 2–5 per cent. have been found in Bramley's Seedlings at 15° C. in the concentrations of carbon dioxide and of oxygen in the external and internal atmosphere. In Sturmer Pippin differences as high as 8–9 per cent. have been observed at 15° C. In this variety it has been calculated that with rise of temperature on a hot summer day the oxygen might fall almost to zero and the carbon dioxide rise to 15 per cent. Since the internal concentration represents a dynamic equilibrium between production and loss by diffusion in the case of carbon dioxide, and between consumption and rate of entry in the case of oxygen, it is found, as would be expected, that the curves of respiratory activity and of these differences of concentration are roughly parallel when plotted against time. The ratios of the corresponding values on the two curves give some measure of the *porosity* of the apple at that time. Such values were found to be approximately constant up to the climacteric rise and then gradually to decrease; changes of porosity with age have also been observed in tropical fruits. It is obvious that two varieties in gas store even if they have the same respiratory activity will have different internal atmospheres, *i.e.* different *effective* atmospheres, if their porosities are different.

Variability of Living Material and Storage

That bugbear of the experimental biologist, the "previous history" of his material, is always an important factor in experimental work in relation to storage. Apart from the marked differences between varieties—in gas storage a suitable atmosphere has to be worked out for almost each apple variety—and the variation in a population of the same variety, a large number of factors acting on the tree and the fruit may affect storage behaviour. It is a remarkable example of the delicately balanced equilibrium of living material.

Effect of Manuring on Storage Quality.—In recent years Kidd and West, in collaboration with the East Malling Research Station, have designed experiments on the storage life of Cox's Orange Pippins grown on different East Malling stocks and given eight different manurial treatments. In the season 1936–37 sufficient fruit was for the first time available for a storage trial, the samples being, however, from one stock only. The most striking result was the effect of potash manuring, whether given alone or combined

with nitrogen or phosphate. Additional potassium added to the soil not only increased the size of the fruit but also markedly added to the storage life at 4·5° C. A few of the results are given below, where N represents the addition of ammonium sulphate (1½ cwt. per acre), P the addition of superphosphate (5 cwt. per acre) and K that of potassium sulphate (2 cwt. per acre).

WEIGHT AND STORAGE LIFE OF COX'S ORANGE PIPPIN WITH VARIOUS MANURIAL TREATMENTS

Treatment.	Weight (gm.).	Storage Life (Days).	Treatment.	Weight (gm.).	Storage Life (Days).
NPK	84·5	188	NP . . .	64·1	152
NK	84·8	174	N	68·6	141
PK	89·0	208	P	60·8	129
K .	80·8	212	Nil . . .	63·1	156
Mean	84·8	195	Mean . . .	64·1	144

The effect of "previous history" on the fruit is very pronounced. Not only is the average weight of the apple increased by potash manuring of the trees but the fruits are brought into a different physiological condition, with the result that the mean storage life (based on 10 per cent. wastage) is increased on an average by 50 days over those without potash. The wastage in this case (at 4·5° C.) was solely fungal rotting, but with similar material held at 1·1° C. there was considerable low-temperature breakdown which, however, was almost confined to fruit from the trees *receiving* potash. Potash is thus remarkable in increasing the effect in the one case and reducing it in the other.

FUNGAL ATTACK OF STORED FRUIT

Fruit in cold and gas store may not only be rendered unmarketable by physiological diseases, such as low-temperature breakdown and scald, but sooner or later it is invaded by fungi, though this may be only after it has passed beyond the period of commercial storage. The question naturally arises as to the conditions in a store which affect the amount of fungal invasion, and attempts have been made to relate it to temperature but without much success. The difficulty is due to the fact that wastage in store is controlled not only by the natural resistance of the fruit, but also by the degree of infection of the surface of the fruit when stored. There are thus at least two factors at work, and a given level of wastage in store might be due to a high level of infection or to a low resistance, or to some combination of the two.

Rate of Fungal Invasion.—It is obvious that in the study of fungal wastage some method is required by which the natural resistance of the fruit can be estimated directly. Such a method has been worked out for the apple by Gregory and Horne (*Proc. Roy. Soc.*, B, **102**, 427, 1928). It consists in inoculating the fruit under standard conditions by removing cylinders of flesh at two opposite equatorial points by means of a cork-borer, inoculating with a portion of fungal culture, and replacing the plugs; the fruit is then kept under standard conditions. The rotted tissue softened by the fungus is scooped out after an appropriate time and the weight of the apple before and after gives a measure of the amount of invasion. The "raw" values of the weight of the rotted tissue cannot be used directly as these vary with the size of the apple, since invasion tends to advance in the form of a spherical shell. When combined, however, with the diameter of the particular apple and the period of time, such data will give, by an equation developed by the authors, the *rate of radial advance* through the apple from the point of inoculation. This gives a standard for the activity of the fungus and is usually expressed as millimetres per day. The mean rate of radial advance of a sample of apples gives a measure of the resistance—or its inverse, susceptibility—of the apple for the particular fungus employed.

Using this method, and a standard fungus *Cytosporina ludibunda*, Horne has carried out numerous studies of resistance to fungal invasion as affected by variety, manurial treatment of tree, nature of stock on which the variety is grown, chemical composition of fruit, and age of fruit. Reference can be made only to the fact that nitrogenous manuring, reflected in a higher nitrogen content of the fruit, reduces resistance to fungal invasion. On the other hand, the use of potash fertilisers, again reflected in a higher potassium content of the fruit, heightens resistance to fungal invasion. The increase in storage life resulting from potash manuring pointed out earlier is thus shown by these analytical studies of Horne to be almost certainly due to a physiological effect of potassium which in some way retards fungal invasion.

Using the method of "radial advance" it can be shown that resistance often changes markedly with time.

BRAMLEY'S SEEDLING IN STORE

	Oct. 17.	March 17.	Difference.
Mean rate of radial advance (mm. per day)	0·297	0·684	0·387 ± 0·031

The rate of radial advance has more than doubled during a period of 153 days, the difference between the two dates being highly significant. With lapse of time the resistance of the apple to fungal invasion has thus fallen heavily.

Reference has been made only to a few of the more interesting biological investigations relating to fruit storage which have been carried out in this country in recent years. Full records are to be found in the interesting and valuable Reports of the Food Investigation Board published annually by the Stationery Office. These reports serve to show how the demands of applied science inevitably give an impetus to the further development of fundamental work.

THE LIGHT OF THE NIGHT SKY

By Professor JEAN CABANNES
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Introduction

The sky during moonless nights gives more light than all the visible stars put together. For a very long time this general luminosity seemed to be self-evident. It was assumed without discussion that the additional light was coming from weak stars only. Yntema first showed in 1909 that the brightness of the sky is much too large to be attributed to the stars. Thus the problem of the night sky arose.

Stellar counts enable us to estimate the illumination given by stars up to the twentieth magnitude. The weakest telescopic stars contribute very little to the total, and we can be sure that the effect of the unobservable stars in our own galactic system is so small that the extrapolation formula used to estimate its value is unimportant. Dufay, now Director of the Lyon Observatory, gave the best criticism of stellar statistics and the first accurate measurements of the light of the night sky. All the telescopic and unobservable stars contained in a square degree of the sky in the neighbourhood of the Pole Star give the same illumination as one star of magnitude 4·4, but the observed illumination is seven times larger.

These results must be corrected for atmospheric scattering. The light from the Milky Way and the visible stars scattered by the atmosphere must be added to the luminosity of the sky itself whatever be the region observed. Above the terrestrial atmosphere the proportion of light given by weak stars would be larger—about one-fifth larger for radiations affecting a photographic plate.

We have then to look somewhere else for the origin of the most important part of sky light and two assumptions could be made. We could think of a scattering of the light given by the sun or the stars in the intersidereal space, scattering due to electrons, molecules or cosmic dust ; or else we could think of a luminescence of the high atmosphere much like polar auroræ.

If we analyse the complex light from the night sky with a spectrograph, the first radiation appearing on an orthochromatic plate is the green line of polar auroræ λ 5577. As early as 1919 Slipher observed that this line is always present, even for low latitudes. McLennan showed that it was emitted by the neutral oxygen atom. In 1922–23 Lord Rayleigh gave much longer exposures (50–200 hours), using a small objective of great aperture, and obtained three photographs which showed not only the green line but a continuous spectrum cut by the H and K absorption lines. Moreover, two of the plates showed two bright bands of unknown origin, one in the blue and the other in the violet. These have been called X_1 and X_2. Slipher, from the Lowell Observatory, confirmed the existence of these radiations in the sky.

At about the same time, Dufay, using a spectrograph with quartz prisms and lenses, obtained a continuous spectrum showing dark lines which he identified with those in the solar spectrum.

Thus, as early as 1923, the light from the night sky appeared to be extremely complex. This is well known to-day. One part of the light, emitted in the high atmosphere, gives a complicated spectrum, composed of bright lines and bands. To a certain extent this luminescence can be compared to polar auroræ. It is related to solar activity and to its atmospheric manifestations, such as ionisation intensity and the amount of ozone produced. This luminescence is doubtless distributed on several layers of different altitudes and probably extends even beyond the terrestrial atmosphere into intersidereal space.

Superposed on this spectrum, composed of lines and bands, is a continuous spectrum of just as complex an origin. One part of this emission comes, as we have pointed out already, from weak stars. Another part is related to zodiacal light, which it is difficult to localise in the solar system; scattering of stellar light in the galaxy also plays an important rôle.

The problem of night sky is thus made difficult by the extremely weak light, by the superposition of an emission spectrum on an absorption spectrum and by the variation of the intensity in time and space. But a great number of other problems of geophysics and astrophysics are involved in the research. Many workers have been attracted by its importance and we are glad to acknowledge that a Committee of the International Astronomical Union is from now on going to deal with its problems. I was myself, in August 1933, able to gather some of my pupils on the Pic du Midi (Pyrenees), and Dufay and myself then decided, with the collaboration of Garrigue and Gauzit, to undertake a systematic research in Mont-

pellier, Lyon and on the Pic du Midi. This collaboration has been fruitful and is still lasting.

In order to obtain spectra of the light from the sky every night, with a few hours' or even a few minutes' exposure, we had to build spectrographs of a new type, both of great aperture and of a sufficient dispersion. The instruments best adapted for such a study are actually those devised by Cabannes and Dufay, by Garrigue, by Arnulf and Lyot, and the spectrograph mounted by Struve on the tube of the great refractor of the Yerkes Observatory. In all such instruments of great aperture the useful diameter of the photographic objective is at least equal to its focal length.

Scattering of Light in the Galaxy

The continuous spectrum of the night sky shows Fraunhofer's dark lines. These lines might come directly from the spectrum of weak stars, as most of the stars are of the solar type. But if we measure *grosso modo* the intensities of the H and K lines in the night sky's spectrum we find that the effects of the weak stars could not explain the large intensities actually found. The absorption lines must then be part of the continuous spectrum of unknown origin which is superposed on that of weak stars. We have thus a hint as to the origin of this continuous spectrum. It comes probably from a scattering of the light of the Sun or the stars. But, as we have to deal with scattered light, we have two questions to answer: firstly, what is the origin of this scattered light and, secondly, what are the scattering particles?

We have two ways in which to attack this problem. The first is to study the energy distribution in the spectrum of the scattered light, and the second, if the rays are polarised, to measure this polarisation. It is very difficult to determine accurately the energy distribution in the spectrum of the night sky; such measurements have not yet been made. However, some interesting results have been obtained by using blue, green and red filters. It has been found that the night sky is extraordinarily red, just as red as the opening of an oven at a temperature of 2200° K. That is far from the 5500° K. necessary for the emission of white light. But this "red of the night sky" is due to luminescence inside the terrestrial atmosphere. There are numerous and intense emission bands in the red part of the spectrum, but nothing can be deduced from the observations concerning the distribution of intensities in the continuous spectrum.

With photographic plates not very sensitive to red light Rudnick obtained a colour-index not very different from that of the Sun.

So, if it were not for the atmospheric luminescence, night sky would be white. Some molecules and atoms would scatter a blue light, the "blue of the day sky." The intersidereal particles which scatter, night and day, the light coming from the Sun or the stars would then be electrons or solid dust.

Polarisation measurements are much more advanced. An infinity of planes passes through a beam of light. If all the azimuths have the same properties, if they are all alike, the beam of light is not polarised. If there are some privileged azimuths with peculiar properties, we say that the beam of light is polarised. Both cases may occur in the sky. If we consider the scattering of solar light, the plane defined by the visual ray and by the Sun has obviously peculiar properties. It is probable that the scattered light will be polarised. On the contrary, when we deal with a scattering of stellar light we can assume a roughly uniform distribution of all the stars in the sky, there is no privileged plane and the light is not polarised. Dufay, however, has found in the light from the night sky a very weak, but measurable, polarisation.

Another approach to the solution of the problem is provided by a study of the zodiacal light, whose brilliancy is two or three times greater than that of the sky and which is strongly polarised. On the other hand, luminescence phenomena are no more intense in zodiacal light than in the other parts of the sky. Zodiacal light is due to scattering of the light emitted by the Sun.

We could then ask ourselves if the continuous spectrum of the night sky is not simply an extension of the zodiacal light. If this were so the polarisation of the light should be of the same order of magnitude. But the number we find is five or six times smaller. That leads us to think that, if the Sun plays a rôle in the light from the night sky, the stars play their part too. There is scattering of the stars' light by particles distributed in the galaxy.

Elvay and Roach were able to separate the different components of the night sky's light by studying the distribution of the brilliancy throughout the firmament by means of a self-registering photometer with a photoelectric cell, thus obtaining observations very rapidly. They measured atmospheric luminescence and scattering, brilliancy of zodiacal origin, and the "cosmic component," which includes the light from weak stars, and also the scattering in the galaxy. As the light of weak stars may be calculated by stellar counts, we obtain by difference the amount of scattered light in the galaxy. By this method the existence of such scattering is given definite proof.

It is interesting to compare the results of these workers with

Dufay's theoretical predictions. An absorption of light by the galaxy had been measured by numerous methods. There is an increase of one magnitude per kilo per sec.; that means roughly a diminution of one-half for 5000 light years. Dufay, and more recently Fasenkov, showed how to calculate the diffusion of light in the galaxy from the absorption data. There is a surprisingly good agreement between these calculated values and those resulting from the later measurements of Elvay and Roach. As for the extragalactic nebulæ, according to the counts by Hubble, they have practically no part in the luminosity of the sky.

Forbidden Lines of Oxygen

Identification of the radiations emitted in the high atmosphere is difficult because of the small dispersion of spectrographs with great aperture, as used up to now. However we arrived at a total of well-established results. The most intense radiations in the visible spectrum are definitely identified: they are lines of the neutral oxygen atom and bands of the neutral nitrogen molecule. This luminescence spectrum presents two essential characteristics: on the one hand, transitions accompanying the emission of lines and bands are "forbidden transitions"; on the other hand, the necessary energy for exciting such "permanent aurora" is less than 7 electron-volts and consequently much inferior to the energy involved by sporadic aurora-manifestations.

The history of forbidden radiations is curious. When an astronomer is discovering a new radiation in the sky, he measures its wave-length and looks in catalogues for its origin. Often he notices that it is not there. The first observers had then thought such new radiations were emitted by unknown particles and they invented, for their cause, chemical elements such as the nebulium of gaseous nebulæ, the coronium of solar corona, or the aurorium in the case of polar auroræ. But, with the progress of atomic physics, it has become clear that possibilities of emission were not exhausted by our sources of artificial light, and that there could be, under different conditions of excitation, emissions forbidden in the laboratory. And that is effectively what is happening.

An atom, such as the oxygen atom, is normally in a certain minimum state of energy w_0. But when we excite it, there is a change in the electronic configuration; a new order establishes itself; the atom has passed to a higher energy level w_n. One finds a great number of these excited levels above the normal level. From each of them the atom may return spontaneously, from fall to fall, to the normal level. On each fall the atom liberates an

excess of energy $w_n - w_m$, and this energy is radiated. Thus are explained the different radiations emitted by one atom on returning to its normal state; their frequencies $\nu = \frac{w_n - w_m}{h}$ are proportional to the energy lost by the atom.

But the spontaneous return from an excited level to a lower energy level is not always easy. Thus, a little above the normal level ^3P of the oxygen atom, we find successively two "metastable" levels ^1D and ^1S. Probability of the returns $^1S \rightarrow {}^1D$, $^1D \rightarrow {}^3P$, and $^1S \rightarrow {}^3P$ is small; these transitions are forbidden by the ordinary selection rules of spectroscopy. While generally not more than one ten-millionth of a second is needed for the spontaneous fall of the atom to a lower level, in the case of the oxygen atom about one second is needed to pass from the second to the first metastable state with emission of the line λ 5577, and to pass from the first metastable state to the normal state with emission of the line λ 6300, about two minutes are needed. During that time, many accidents may happen; for example, collisions may destroy the metastable atom and reduce it to the normal state before it has time to radiate. One says the 5577 and 6300 lines are forbidden. More exactly, they are difficult to produce in the laboratory. Large volumes of extremely rarefied gas are needed such as occur in the high layers of the atmosphere, in comets, and in novæ. There, collisions are infrequent and the atoms have time to live their life. Our progress in the knowledge of electron levels of atoms is thus connected with the discoveries of astronomers.

The forbidden oxygen lines 5577 and 6300 are the most intense in the visible part of the night-sky spectrum. The first one is the famous green line of the polar auroræ, identified by McLennan, who succeeded in reproducing it in the laboratory by submitting oxygen diluted in an inert gas to a convenient electric field. The red line had for a longer time remained little known. Being generally weaker in the polar auroræ than the green one, and coming out on panchromatic plates only, it has attracted less attention. We were able to measure its wave-length in the night sky and in this way to establish the identity of the red line in the sky with the radiation of oxygen, predicted and calculated *a priori* by Frerichs in 1930 and later on observed with difficulty in the laboratory by Paschen and Hopfield. The identity of the line λ 6300 of the night sky is confirmed by the presence of a weaker line λ 6364. It is known of course that the fundamental level ^3P of the oxygen atom is a triplet level. The most intense line accompanies the transition $^1D \rightarrow {}^3P_2$; the other, the transition $^1D \rightarrow {}^3P_1$. A third,

weaker line, $^1D \rightarrow {}^3P_0$ could be expected. It has not yet been observed.

Systematic comparison of the intensities of green and red oxygen radiations could lead to important results. The spontaneous transition $^1D \rightarrow {}^3P_2$ which furnishes the red line is two hundred times less probable than the transition $^1S - {}^1D$ to which the green line is due. A variation of density of the air in the luminescent layers, causing a change of the frequency of collisions between the oxygen atom and neighbouring particles, is not acting in the same manner on the intensities of both lines. Such comparisons have often been made with the novæ and it is found that the green line attains its maximum intensity about three weeks before the red line. Thus, during three weeks we see the excitation and density of the gaseous mass decrease. We follow the evolution of the star.

Garrigue made the important discovery that the intensity of the red line diminishes during sunset, and increases during sunrise. We recognise here a direct action of sunlight. It dissociates the oxygen molecule into one normal atom ^3P and an excited one ^1D. It is interesting to follow this action into higher and higher layers of atmosphere when the Sun is descending below the horizon. Emission seems to be a maximum at a height of about 120 km., but we have observed it up to the record altitude of 1000 km. The green line does not show any such change.

Sodium in the Upper Atmosphere

Between the green and the red oxygen line, our spectrographs show a yellow line the wave-length of which coincides with that of the doublet of sodium. It was discovered by Slipher in 1929. Interference methods enabled us to separate the two lines D_1 and D_2 and to show that D_2 is twice as intense as D_1. The presence of sodium in the high atmosphere is thus definitely established. The doublet of sodium is intensified at sunset as is the red oxygen line, but the altitude is not the same: Bernard observed that intensification of the yellow doublet ceases abruptly as soon as the sun-rays exceed an altitude of 60 km. We already knew that it was not due to the interstellar sodium, since during the night the intensity of the radiation increases from 1 to 3 if we observe successively the zenith and the horizon, but this variation of intensity gives an altitude of about 130 km., which is higher than that given by Bernard's observations. Perhaps luminescence of interstellar sodium joins that of the atmospheric sodium? Perhaps the luminescent atmospheric layer is not the same at sunset as during the night?

To explain the unexpected presence of sodium atoms in the high atmosphere we thought of the fall of meteorites; the weight of matter we receive from the sky attains 4 grammes per square kilometre every year. There is 1 per cent. of sodium in it. The yellow radiation of night sky would then reveal simply the continual bombardment of the earth by cosmic dust. But there must be explained the surprising diminution of sodium light that we noticed during summer months. Also the presence of the calcium and aluminium lines would have to be verified; these elements are, indeed, even more abundant than sodium and their resonance-lines are in an easily accessible spectral region.

Forbidden Bands of Nitrogen

Passing now to the blue and violet radiations emitted by the night sky, we reach a part of the spectrum altogether more difficult to identify, because it contains a very great number of rather faint lines and bands. It is well known that the spectrum of the polar auroræ is characterised by a small number of strong bands which are attributed to the second positive system and chiefly to the negative system of the nitrogen molecule. Their intensity makes them evident, isolates them from the whole spectrum, and their origin is not to be doubted. Besides these strong bands we observe much weaker bands, and, strange to say, just these weak radiations are found permanently in the night sky from which the strong bands of the auroræ have disappeared. Apart from the accidental auroræ, the excitation of the high layers of atmosphere is thus insufficient for emission of the second positive system and, *a fortiori*, of the negative system of the nitrogen molecule; it is therefore less than 11 electron-volts. But we might ask ourselves if the spectrum of the night sky does not coincide with a new system of bands of the nitrogen molecule arising from quantum levels lower than the bands of polar auroræ. Now Vegard had discovered in the luminescence of the nitrogen crystals, and Kaplan, in a gas-tube, forbidden bands which are emitted when the molecule falls back from the metastable level $A(^3\Sigma)$ to the normal level $X(^1\Sigma)$. Also, we know that nitrogen molecules in the state A exist in the sky. One finds, indeed, in the red and infra-red, bands of the first positive system emitted at the transition $B \longrightarrow A$. The easiest to observe and to identify is the red band with two maxima $\lambda\lambda$ 6500–6550, accompanying the transition $B(v' = 7) \longrightarrow A(v'' = 4)$. Kaplan was the first to ask himself whether Lord Rayleigh's blue and violet radiations, which we called X_1 and X_2 at the beginning of the present article, did not

belong to the system of bands $A \longrightarrow X$. We tried to verify this hypothesis, and the result was really conspicuous: we find in the sky all the Vegard-Kaplan bands, of which the initial quantum v' is near 2 and which belong to the sequences $v'' - v' = 10$, 11, 12, 13. In the spectrum they catch the eye, from the blue band λ 4838[$A(v' = 2) \longrightarrow X(v'' = 15)$] to the ultra-violet band λ 3759[$A(v' = 2) \longrightarrow X(v'' = 12)$]. Thus, the emission spectrum of the night sky in the blue, violet and the beginning of ultra-violet is essentially constituted by the "forbidden" bands of Vegard-Kaplan. The luminous particle is the neutral nitrogen molecule carried to the metastable level A with the vibration quantum number 2 or 3. This important result fixes the available energy near 6·7 electron-volts.

Periodical Variations of the Night Sky Light

We have followed at Montpellier for more than one year the variations of intensity of the nitrogen bands X_1 and X_2. The fortuitous changes noticed from one night to another do not obscure a slower and more regular variation. When making the harmonical analysis of this slow variation, we find a yearly period with a maximum in June and a minimum in December, and a half-yearly period with maxima at the beginning of March and September, and minima at the beginning of June and December. Lord Rayleigh's observations made by coloured filters are in accordance with ours and complete them. These observations lasted some years, and they were made at three stations very distant from one another, in England, Australia and in the Cape. The periodical variations are parallel on each station in the three spectral regions which have been examined (blue, red, and λ 5577). Furthermore, the half-yearly term follows the same law of variation at all of the three stations, whilst the yearly variation gives proof of seasonal influence; this effect reverses itself with the seasons when passing from the northern to the southern hemisphere.

The accidental changes show no correlation whatsoever between one station and the other. They are local phenomena. It is quite clear that luminescence depends at the same time on the excitation and on the physical properties of the excited layers (density and temperature). The irregular variations of luminescence arise from local meteorological conditions; perhaps the yearly effect is also connected with changes in the high atmosphere. But the half-yearly variation, like that which accompanies the solar cycle of eleven years, arises certainly from changes in the intensity, and perhaps in the nature also, of the solar excitation.

Maxima of the half-yearly term appear at times when the Earth has the greatest or the smallest heliographic latitude (March 6, and September 8), whilst minima occur when it passes through the plane of the solar equator. These observations favour the hypothesis of corpuscular excitation. The luminescence of the high atmosphere would be excited, at least partially and more or less directly by corpuscules issued from the zone of the sun-spots, the average latitude of which is $\pm$ 15°. The same periodical variations are found in the brilliancy of the night sky, in the frequency of the auroræ observed at low latitude and in the ionisation of Appleton's F layer near 200 km.

Altitude of the Luminescent Layers

CH and CN Bands

From the beginning of our researches we noticed that emission of nitrogen and oxygen lines was more intense on the horizon than on the zenith. There is only one possible explanation : the luminescence takes place in the high atmosphere, and the measure of the relation between the intensity observed on the horizon and the intensity observed on the zenith may give an idea of the altitude of the luminescent layer. In this way we find altitudes over 100 km. But the hypothesis of a homogeneous, single and relatively thin layer, which is the basis of this calculation, does not permit us to place too much confidence in that result. On the other hand, the certain presence of radiations very much absorbed by ozone, near λ 3000, in the spectrum of the night sky, suggests inevitably the existence of luminescent layer at a much lower altitude.

Comparing carefully two spectra obtained at the same time from the horizon and zenith, we observe a different distribution of the intensities on both, a proof that not all radiations are emitted from the same altitude. Vegard-Kaplan's bands are weakened enough on the zenith to permit new bands to appear whose intensity has not sensibly diminished. For many years we had likened these bands to the characteristic radiations of the nuclei of comets the origin of which was yet unknown. The recent progress by Spring and Dufay in the analysis of light of the comets enabled us to identify them : they are bands emitted by the molecule CH. The probable presence of CH bands in the sky spectrum and the analogy of the latter with cometary spectra led us to search for other band systems associated with carbon. It seems that bands of the CN molecules are found among the night-sky radiations whose intensity does not appear to increase from the zenith to

the horizon. These emissions thus occur at a very high altitude or even outside the terrestrial atmosphere.

Conclusion

It is interesting to consider what may be the origin and mechanism of luminescence in high atmosphere. One hypothesis was proposed by Chapman in 1930. During the day, ultra-violet light from the sun dissociates the oxygen molecules, and luminescence of the high atmosphere might accompany their recombination during the night. Two normal oxygen atoms liberate an energy equivalent to 5·1 electron-volts when they recombine. For such recombination to occur, a third particle must absorb the energy released : a triple collision is necessary. If the third particle is an oxygen atom, it is carried to the level ^1S, and is able to emit the green line, on falling back on level ^1D. Thus, emission of line λ 5577 accompanies the formation of an oxygen molecule and the excitation of an atom to the level ^1D. Some of these atoms will emit for their part red lines $\lambda\lambda$ 6300–6364 ; others, meeting at the same time a normal oxygen atom and a nitrogen molecule, will liberate by recombining an energy of 7·1 volts, carrying the nitrogen molecule to the level A with vibration quantum number 2 or 3, from where it may return to the normal state with emission of Vegard-Kaplan bands.

The difficulty of this theory is its need for triple collisions, rare phenomena in the quite rarefied atmosphere of the luminescent layers, moreover it ignores completely the rôle of free electrons in the atmosphere. It is known how easily they fix themselves on to oxygen atoms. A few minutes after sunset all free electrons would thus have formed O^- ions, if for some undetermined reason the electron does not separate from the atom. Martyn thought of the collision of a neutral oxygen atom with an ionised O^-. There might form a molecule O_2, the electron being projected with great velocity carrying off the disposable energy. This ingenious explanation has the merit of granting a rôle to electrons in night-sky luminescence. We think indeed that the electrons so ejected at great velocity would be able to excite oxygen atoms and we return to Chapman's mechanism, but without the necessity for triple collisions. Thus it is easy to conceive that intensity of luminescence is connected with intensity of ionisation.

Examination of night sky light has made important progress in recent years. I have tried to summarise it in a few pages. But there remain many questions to solve. The origin of certain intense radiations, *e.g.* the ultra-violet line λ 3556, is still unknown.

Construction of spectrographs of the same aperture as those we are using to-day, but with greater dispersion, would facilitate their identification. Laboratory researches, such as Kaplan's about active nitrogen, will enable us to reconstitute, at least partially, the spectrum of the night-sky luminescence and perhaps to state more precisely the mechanism of emission. Finally, spectro-photometrical measures need to be multiplied. Up to now the use of coloured filters has been preferred to spectrographs. But the atmospheric luminescence gives a discontinuous spectrum ; one must profit by this fact to try and isolate it. We have shown that it is possible.

It was with the spectrograph opened at F/0·7 that Dufay and I obtained the most important of our results. We want to express here our thanks to Mr. W. N. Cromwell, President of the Franco-American Committee of New York, to whose generosity we owe this apparatus which has so well fulfilled our expectations.

THE CHEMICAL STUDY OF SILICEOUS INDUSTRIAL DUSTS

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SILICEOUS dusts have long been known as a grave industrial hazard. Before our day their danger was clearly recognised in the popular mind by giving the disease they cause names like "potters' asthma," "miners' consumption" or "grinders' rot," which forcefully express its dread character, and it was indeed known in ancient times as a characteristic disease of miners.

With the steady improvement in factory conditions which has been brought about during the last few decades, the risk of silicosis in factory workers has greatly diminished even in such especially dangerous trades as the manufacture of pottery, partly because every dusty operation is nowadays performed under a strong draught which carries the dust away from the operative, and partly because, in some cases, dangerous siliceous materials, such as the flint dust used for packing china in the kiln and the sandstone of the grinders' wheel, have been replaced by non-dangerous materials such as alumina dust and carborundum.

There are, however, many trades, like that of the miner, the quarryman or the mason, where such precautionary measures are wholly or largely inapplicable, and so it is that silicosis continues to be a major problem of industrial hygiene. The example which is best known and has been most lavishly studied is that of silicosis in the Rand gold-miner, to which attention was imperatively directed by the discovery when the mines reopened after the War, that only a small proportion of the old miners were available, because their fellows had meanwhile died of miners' phthisis. A very slight contact with potters or miners makes one see how their lives are still shadowed by the fear of disablement and death through the agency of their old enemy, dust.

Here is reason enough why the scientist who likes to work for the good of mankind should seek for the cause and the remedy; but until about five years ago it did not look like a job for the

chemist. Medical men had studied the nature and the development of silicosis, engineers and physicists had investigated the concentration and size of the air-borne particles in dangerous dusts, and all concerned were satisfied, as many still are, that the causative agent was free silica. The strength of this opinion is clearly shown by the fact that the whole of the legislation for prevention and compensation was based upon it ; so that, for example, compensation was payable only where it could be proved that a man had worked with materials containing not less than 50 per cent. of free silica.

Yet there were well-known facts which should, in scientific minds, have thrown some doubt upon this hypothesis. Asbestosis, a disease closely akin to silicosis and differing from it in ways which may be largely explained by the fibrous form of asbestos, is caused by a material which contains no free silica. On the other hand cases were known, like the mines of Cripple Creek, Colorado, where miners working in rock rich in quartz seemed to run little, if any, risk of contracting silicosis. Nevertheless, those who were chiefly concerned continued to say that silica was the cause of a disease, silicosis, which they would diagnose only where there was a history of exposure to silica dust.

The credit for breaking the vicious circle of this argument belongs entirely to a geologist, Dr. W. R. Jones, who, in a paper published in 1934, showed that certain South Wales anthracite miners whose lungs, on post-mortem examination, showed typical silicotic changes, had worked throughout their lives in rock and coal almost free from uncombined silica. He showed also that in many cases the industrial working of materials like sandstone or Rand quartz, which consist almost entirely of free silica, produces dusts which consist largely of a fibrous mica, sericite, and thus differ entirely in their chemical nature from the parent rock as a whole.

My natural interest in a colleague's work led me to think seriously about this problem, and I saw that there might, after all, be something here for a chemist to do. Evidently the old view that silicosis was due to mechanical damage by sharp particles settling in the lung was untenable : harmless alumina is as hard and sharp as harmful quartz, and in any case particles so small that they take hours to settle out of a foot or so of still air must fall on the lung surface like feathers on a jelly.

The very fact that there are safe dusts, like alumina or carborundum, to set in contrast with the dangerous dusts suggests very strongly that some chemical characteristic of the latter is implicated. Moreover, the development of fibrous tissue, so characteristic of silicosis and asbestosis, occurs, for the most part, without direct

contact with the particles responsible for it, as these are enclosed in other tissues or coated with the characteristic covering of the asbestosis body. The action of the mineral is action at a distance, and it seems that it must be due either to radiation or to the chemical action of solutions produced from the mineral. In the absence of any evidence that dangerous dusts emit characteristic radiations, we are left only with the latter alternative.

Because the medical man does not easily take the chemist's viewpoint it is perhaps desirable to emphasise the simple logic of this argument. The physiologist and the pathologist see the very various and complex "reactions" of the organism to dusts and are inevitably apt to feel that a particle may do damage in equally various ill-defined ways. Yet, unless we are prepared to import considerations of mind or magic, it surely must be true that an effect, whatever it may be, of a particle on a cell separated from it (even by a thousandth of a millimetre) can only be attributed to the effect of soluble substances diffusing thence from the particle.

Impelled by such considerations, my colleagues and I began the experimental study of dusts from the chemical standpoint.[1] We had a dim idea, hardly more than a "hunch," that to produce their observed effects upon the organism dust solids must possess peculiar reactivity, but we really had no notion how great and remarkable that activity would prove to be. Luckily, however, we had the sense to see that if our search for new properties was to be effective, we must collect the dust in such a way that it was unaltered in the process. Our slogan must be: "the dust as the worker breathes it." We stipulated, therefore, that the dust must be secured without contamination, that it must not be wetted with water and that it must not be heated above about 100°.

One major difficulty in sampling industrial dusts is inherent in their very low concentration: many dangerous atmospheres such as those of flint-crushing plant, pottery workshops, metal mines and asbestos factories, frequently contain only about 1 or 2 milligrams of dust per cubic metre. Even very dense dust clouds, like that in which a collier works at the coal face, contain only about 100–200

[1] This beginning was made possible by the award of the Pedler Fellowship by the Institute of Chemistry to Dr. Janet Matthews who was at first my only collaborator. Later the Institution of Mining and Metallurgy and the Medical Research Council assisted our researches by the provision of apparatus and materials, by enabling Dr. Matthews to carry on her work and by giving me also the collaboration of Dr. P. F. Holt, Dr. Phyllis M. Sanderson and Dr. N. Spoor. It is a pleasure to acknowledge the dependence of our work upon these grants and to express my own warm appreciation of the loyal and enthusiastic labour of my colleagues.

mgm. of solid per cubic metre, and in most parts even of a fairly dusty coal mine the dust concentration is between 5 and 20 mgm./m.^{3}. For a complete micro-analysis in duplicate we like to have about 50 mgm. of dust, and it is usually necessary to restrict the period of collection to about 2–3 hours; so the collecting device must be such that in this time 40 or 50 cubic metres of air can be passed through it, under a suction which should preferably not exceed a few inches of mercury. Moreover, it is evidently desirable that a sample for analysis shall be a complete sample and it is therefore necessary that the efficiency of collection shall be nearly 100 per cent.

A study of the known methods of dust sampling soon showed that none fulfilled our conditions even approximately, and disclosed the rather startling fact that no one had ever seriously attempted to obtain *unaltered* specimens of atmospheric dust solids. The first two years were, therefore, devoted largely to the development and testing of suitable methods for collecting dust samples. To recite this tale in detail would be tedious, but it may be useful to describe briefly the principal methods we now have available as a result of this work. These methods fall into three classes:

(i) *Volatile solid filters*, using a filter-bed of a crystalline solid such as naphthalene, acenaphthene or anthracene which can be sublimed away from the collected dust at a relatively low temperature.

(ii) *Soluble solid filters*, using a filter-bed of a crystalline solid soluble in a non-aqueous solvent (*e.g.* salicylic acid soluble in alcohol, or acenaphthene soluble in benzene), so that the collected dust can be recovered by centrifuging the solution of the filter, washing the residue with a volatile solvent (ether) and drying it at a low temperature.

(iii) *The "labyrinth,"* a purely mechanical method of collection, using an assembly of flat baffle-plates on which a swift current of the dusty air deposits its solids by impingement, so that the dust may subsequently be scraped from the baffles.

It may here be remarked that all these methods are fundamentally the same in principle. The porous bed of crystals does not really act as a filter: its interstices are much too large to hold back the very minute solid particles it actually catches, and the bulk of the dust accumulates in the filter-bed and not on its surface. Therefore we may conclude that the bed really acts as an assembly of minute labyrinths of irregular form, in which the deposition of dust occurs by the mechanism discussed later.

The foregoing classification of methods corresponds quite closely with the three main types of sampling which have so far been involved in our work, classified according to their purpose:

A. The collection of small samples, 0·02–2 mgm., from 5 to 10 litres of air, for the purpose of determining the mass concentration of dust solids.

B. The collection of larger samples, 20–100 mgm., from 10 to 50 cubic metres of air for gravimetric analysis and approximate estimations of mass concentration.

C. The collection of very large samples, 10–200 grams, for extensive investigations on the general chemical and physical properties of dust-solids, and especially their solubility in water and body fluids. The ordinary range of dust concentrations (1–100 mgm./m.^{3}) corresponds with 1 gram in 1000 – 10 m.^{3}, so that collection of samples of this magnitude may mean handling as much as 200,000 m.^{3} of air.

Occasionally method (i) may serve purpose B, or method (ii) purpose A, and sometimes even method (iii) may give a measure of concentration; but for the most part method (i) is used for purpose A, (ii) for B, and (iii) for C.

To provide the necessary suction for collecting samples of types B and C is in itself no easy matter, as may be appreciated when we consider that to get a 50-mgm. sample we must often cause the whole air content of a London bus to pass in about 2 hours through a filter-bed which cannot conveniently be more than 7 or 8 cm. in diameter. Mechanical pumps which can shift 10–20 cubic feet of free air per minute and produce the necessary suction of, say, 6 inches of mercury are big machines, rarely found in factories and practically unknown in mines. On the other hand most factories have steam at 100 lb. pressure or more and many mines have supplies of air at 60 lb. pressure, and either steam or air will work an ejector, which, if properly designed, will both give the suction required and shift the necessary volume of air. As the ejector, though not particularly efficient, is thoroughly effective and has the great merit of being simple, robust, cheap and easily portable, we have used it in most of our sampling campaigns.

Having thus outlined the methods and apparatus employed, it may be of interest to give in more detail some typical examples of them.

(i) A.—As typical of this method we may consider the naphthalene filter. The purest naphthalene is redistilled slowly through a scrubber column to secure a distillate free from all trace of dust or other non-volatile impurity, and the solidified distillate is finely powdered. The powder is compressed to form a filter-pad in the holder shown in Fig. 1. The pad, 5 mm. in diameter and 4–5 mm. thick, is formed on a perforated brass disc, A, soldered to the inner

tube, B (which, being slit and sprung, is a tight sliding fit in the outer tube, C), pressure being applied to it by a smooth-ended plunger, D, while B is supported by a second plunger, E. When the filter has thus been prepared, it is rolled up in a square of cellophane and the ends are twisted up: in this condition it will keep quite clean and ready for use for a week or two. On the job, the cellophane is removed by cutting through it with a sharp knife about the centre of the filter, so that each end will slide off as a short cellophane tube which can be slipped on again when the sample has been collected. Suction is applied by a hand-pump, rather like a motor-tyre pump, the capacity of which per stroke is accurately known, or by means of an aspirator.

In the laboratory, the inner tube is pushed out rightwards by the plunger E, and the naphthalene pellet is detached and put face downwards in a tiny shallow platinum dish which is then kept at 70° overnight on an electrically heated metal block. Next morning only the dust remains and its weight can be accurately

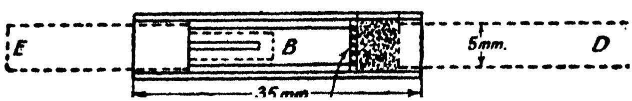

FIG. 1.—Naphthalene filter.

assessed by reweighing the dish on the micro-balance. The volume of air drawn by the pump is known within ± 2–3 per cent., and in most cases the whole error of the determination is only of this order. Moreover, small as these samples are—usually 0·5–0·2 mgm.—we have found it possible to go further and make partial analyses with them, determining, for example, the ash in a coal dust within ± 0·5 per cent. or the silica in a rock dust within ± 1 per cent. This information is frequently very useful, because these samples can be quickly taken, in perhaps 2–3 minutes, and so may be used to follow changes in the concentration and character of a dust cloud.

A recent development of this technique has made it useful for the microscopic examination of dusts, to determine the minerals present or to ascertain the size-distribution of the particles. In this case, still smaller samples are taken (100–500 c.c. of air), and the filter-pad is dissolved in benzene and the solution is centrifuged in a tube of special design so that the whole of the dust is collected, quite evenly distributed, on a half-inch cover slip at the base of the tube. The cover slip is specially prepared so that the particles are fixed where they fall, and it rests on a loose base-block of iron

which can be lifted from the brass tube by means of a magnet. Otherwise, as reflection will show, it is very difficult to extract the cover slip without touching it or disturbing the deposit. The tube and its base-block are rhodium plated and highly polished so that they may easily be wiped perfectly free from dust.

(ii) B.—In this case the typical apparatus is the salicylic acid filter. Some may be puzzled at the choice of this material for the bed, but it was a deliberate choice from more than a hundred possible materials, based largely upon the fact that commercial A.R. salicylic acid is very pure, reasonably cheap and, because of the fine acicular form of its crystals, is the best material we know to form an efficient filter-bed which will easily pass large volumes of air. This filter-bed is carried on an assembly of stainless steel

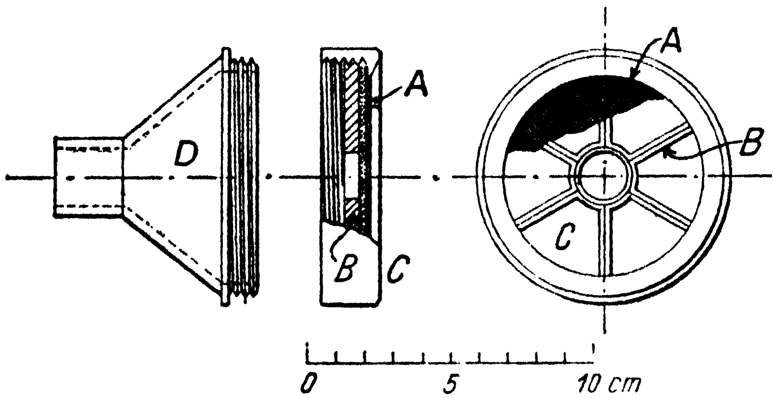

FIG. 2.—Salicylic acid filter.
(*Trans. Inst. Min. Met.*, 1938, XLVI, 270.)

gauzes, A (one of 140 mesh between two of 20 mesh), held in an ebonite holder of the form shown in Fig. 2 and supported by the ebonite grid B, which screws into the cell C and clamps the gauze circles against the front flange. The whole cell then screws on to the funnel D so that it may be connected to suction.

Unlike that formed from naphthalene, the salicylic acid filter-bed is an *uncompressed* mass of crystals. While under suction it is quite firm and resistant to shock (*e.g.* by blasting near by) but it cannot bear transport and so it is formed and removed on the job by the following simple device. The proper charge of crystals (10 gm. to form a 4-mm. bed on a 7-cm. diameter filter) is carried folded up in a square of cellophane. When the ejector and connections have been set up in place, the cellophane packet is unfolded in the hollow of the hand, the filter is inverted over the pile of acid

and then turned face up, so that the acid, covered by the cellophane, can by suitable massage be distributed evenly on the gauze and smoothed over. Then the filter is fitted to the ejector inlet by means of a soft rubber sleeve, the cellophane is lifted off and the suction is turned on. So made, the filter will pass 20–30 $m.^3$ per hour, with a suction of 4–6 inches of mercury, but if it is compressed, as has happened by accidentally applying suction before the cellophane was removed, its capacity is greatly reduced, perhaps to about 5 $m.^3$ per hour. At the end of the sampling period, the suction is cut off, a clean sheet of cellophane is laid across the filter, and by turning the filter over and tapping it the whole filter-bed with the collected dust is transferred to the cellophane and wrapped up in it for transport to the laboratory. There the filter-bed is dissolved up in about 50 c.c. of alcohol, the dust is centrifuged out of the solution and after washing once or twice with alcohol it is washed finally with ether and dried at 50°.

Evidently the question of the efficiency of these filters is very important and it has been pretty thoroughly investigated. We have found by numerous experiments that a properly made bed of a crystalline solid like naphthalene or salicylic acid which is 4 mm. thick will collect 98–99 per cent. of the total solids in air passed through it. The most important evidence for this conclusion is that a second similar bed placed beyond the first in the same air stream collects a quantity of dust which is only about 1 per cent. or less of that collected on the first bed, but several other independent lines of enquiry confirm our belief in the high efficiency of such filters.

(iii) C.—The labyrinth was devised to meet the need for much larger samples than could be secured with any filter of practicable size. A typical form, shown in Fig. 3, consists of 32 flat rectangular plates of polished copper, A, spaced apart by tubular copper distance pieces, B, and clamped up in a single assembly by brass bolts, C, with wing-nuts, D. This assembly fits closely in a long box of rectangular section made up from two strips of ¼-inch plate glass, E, exactly the same width as the baffle-plates, clamped between two rubber-lined wooden side-pieces, F, by means of four brass bolts and wing-nuts, G.

The cross-section of the box is 6 inches by 4 inches, while the baffle-plates are 6 inches by 3¼ inches and set alternately to touch the top and bottom of the box: therefore a current of air drawn through the box has to pass alternately up and down between the baffles and through the 6 inch by ¾ inch openings above and below them. Every time the air turns a corner, it does so on roller

bearings of its own making, on eddies in the corners, in which, by centrifugal action, the dust particles are thrown against the glass or the baffle and there stick.

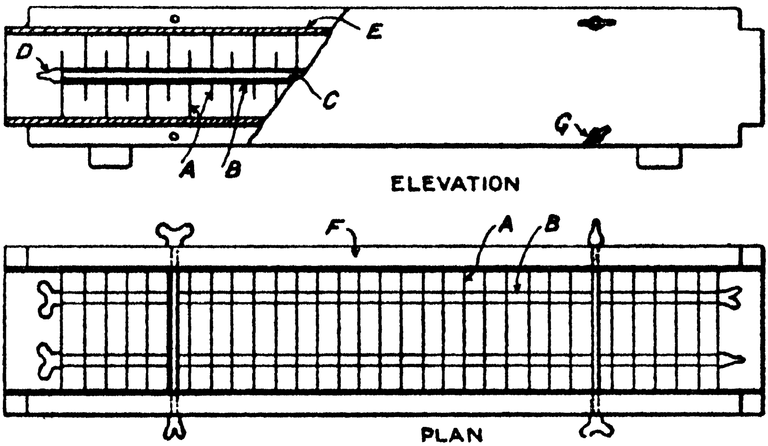

FIG. 3.—The rectangular labyrinth.
(*Trans. Inst. Min. Met.*, 1938, XLVI, 271.)

We have been so much occupied with getting dust samples and studying them that we have never had time to study the labyrinth properly, but we have incidentally learned some of its properties. As the description suggests, it is made so that it can easily be taken

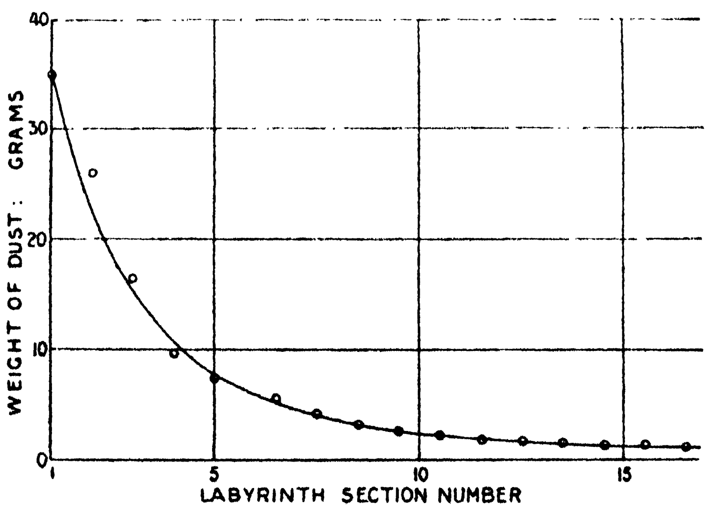

FIG. 4.—Flint dust, Sample A. Mass distribution in rectangular labyrinth. Calculated efficiency 93 per cent.
(*Trans. Inst. Min. Met.*, 1938, XLVI, 275.)

apart and in such a way that the dust deposited in each section can be collected separately for weighing or examination.

If we plot, as in Fig. 4, the weight of dust per section (here *two* plates and *two* spaces are counted as one section) we get a curve which is nearly logarithmic, and by comparing the area under the curve with that under its extrapolation to forty or fifty sections we can get a good idea of the efficiency of collection. The efficiency is surprisingly high : in the particular case illustrated it is about 90 per cent. and with some dusts it has been over 95 per cent., though with others, for example asbestos or coal, it has been as low as 35 or 40 per cent. In any case, however, as has been shown

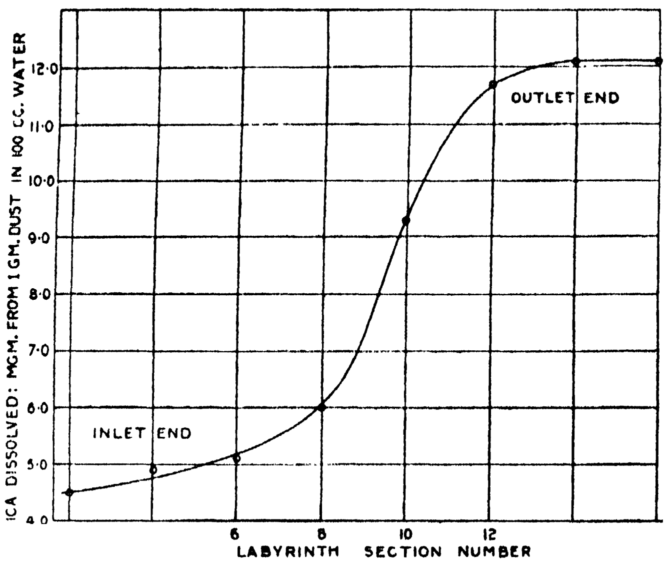

FIG. 5.—Solubility of calcined flint dust taken from different sections of the labyrinth. (Flint Sample A.)

(*Trans. Inst. Min. Met.*, 1938, XLVI, 296.)

repeatedly by comparison with complete samples collected on salicylic acid, the labyrinth samples are true samples and properly exhibit the typical composition and properties of the dust.

As might perhaps be expected, the dust collected in the labyrinth is to some extent sorted according to size, and with the variation in size goes a corresponding variation in properties. This is well shown in Fig. 5, where the silica solubility of a calcined flint dust is plotted against its position in the labyrinth. The composition of the dry dust is the same at both ends, about 80 per cent. SiO_2, but the size distribution (numerical percentages) varies between the limits shown below :

	Particle Size-Distribution.		
	8μ–2μ	2μ–1μ	$<1\mu$
Inlet end	15 per cent.	18 per cent.	65 per cent.
Outlet end	0·5 „	9 „	90 „

As the numerical percentage of particles under 1μ ($1\mu = 0{\cdot}001$ mm.) increases from 65 to 90, the amount of silica which can be dissolved from the dust by water under fixed conditions increases nearly three times in value.

It seems, on the other hand, rather surprising that the efficiency of the labyrinth *increases* rapidly with increasing velocity of air flow, though this is readily intelligible if the deposition occurs centrifugally in eddies as outlined above. Another curious property of the labyrinth, which has not really been explored at all, is its ability to discriminate between different types of suspended matter in air. When it was fed with mine air carrying granite dust and also a mist of oil and water produced by the drilling we found that oil and water collected with the dust on the first few plates, while the later sections of the labyrinth collected *dry* dust. This affords the first proof, so far as we know, that it is possible to have dry dust, and a good deal of it, floating independently in an atmosphere which is fully saturated with water vapour and contains also liquid water and oil.

The great practical merit of the labyrinth is that it may be left to run unattended for days or even weeks on end. Whenever suction comes on, for example on starting up the ventilating system to which it may be attached in a factory or the compressed air supply feeding its ejector in a mine, the labyrinth functions; and the sample, once deposited, is safe in its depths from any interference or contamination. To collect the sample shown in Fig. 4, about 130 grams in all, occupied about 500 working hours, but the time we had to attend on the plant to set the labyrinth up and remove it was only about 5 hours. To collect the same size of sample on salicylic acid filters would have required our whole time work, on the plant and in the laboratory, for about 50 days, so it is easy to see why we have a warm regard for the labyrinth.

Our insistence on securing the dust "as the worker breathes it" has been completely justified by the results we have secured by applying these methods to industrial dusts. The first samples taken by the new methods were of dusts produced in a Cornish mine by wet-drilling and blasting in granite. On beginning their analysis

we immediately found that the loss on ignition, determined on the sample dried at 105°, was about 10 per cent., whereas the corresponding figure for granite powdered in the ordinary way in the laboratory was only 0·5 per cent. Further work confirmed this incredible figure and indeed showed that, for the finer portions of the dust, the content of combined water might reach 15 or 20 per cent. Moreover, this hydration, which occurs instantaneously in the dry dust particle, renders a substantial fraction of the total silica and alkali in the dust immediately soluble in water. This remarkable reactivity of the freshly fractured mineral has been observed also in other cases. Newly calcined flint, when crushed, yields a dust having a loss on ignition which may reach 8 per cent. or 9 per cent., and the same is true of other fine dusts such as those of felspar, mica, or asbestos, even when these are produced as air-borne dusts in the relatively dry atmosphere of the laboratory.

Perhaps we ought not to have felt surprise, as we did, at these results. Every chemist is familiar with the fact that clean surfaces adsorb water vapour, and though with massive material this effect is negligibly small it may, evidently, become quantitatively important for minute particles having a surface area which is relatively enormous in relation to their mass. It was, perhaps, less easy to anticipate that such adsorption would produce correspondingly large chemical changes in the dust, yet this too is readily intelligible if we suppose, as seems reasonable, that the raw edges of the atomic lattice exposed by the fracture of the crystal are rather like free radicals in character and as avid for union with anything that happens to be available. Usually, of course, the most reactive material available is water vapour, and so we commonly find that industrial dusts are extensively hydrated. But much less reactive substances may be similarly held by the dust. If, for example, felspar be finely ground under benzene and then dried at 105°, the resulting dust is inert to water, but will yield soluble alkali in some quantity on extraction with 90 per cent. alcohol. Evidently, the mineral is protected against water by a firmly held film of adsorbed benzene and can only be attacked by water when this is present in a solvent which can remove the benzene.

The most important effect of the hydration of dusts is to confer upon them the remarkably enhanced solubility in water to which reference has already been made. When we began a detailed study of this property, using the large samples collected by means of the labyrinth, we soon found that the solubility of mineral dusts is an extraordinarily complex phenomenon. Here again, of course, a special technique is required which we must dismiss with a mere

mention : it involves vessels of nickel, platinum or wax-lined glass, special methods of agitation, centrifugal clarification of the solution, micro-titrations, and the determination of silica and other solutes by photoelectric colorimetric methods.

Whereas an ordinary crystalline solid has an easily attained maximum solubility in water which is dependent only on temperature, we find that in the crazy realm of mineral aerosols a definite solubility in this sense is unknown. The mineral is chemically changed by water and its several hydrated constituents are differentially leached out so that a felspar dust, for example, loses soluble silica and caustic alkali in amounts which are unrelated to each other or to the stoichiometric proportions present in the crystal and vary independently as leaching progresses with time. There is, of course, the other aspect of this matter, the changing composition of the solid, with which our friends the mineralogists are especially concerned. This change results, as it inevitably must, in the transformation of the original mineral into other species of lesser solubility. We glimpse here the reason why it is very difficult to derive much useful information as to the cause of silicosis from the study of the residual minerals in the lung.

The "solubility" for any one constituent, say, silica, from a given pure mineral dust is a function of the several factors enumerated below :

Temperature has a large effect : usually the extent of solution in 3 weeks at 20° C. is roughly equalled by that in 3 hours at 100°.

Time also has a marked effect especially at the lower temperatures and in the earlier stages of solution. With quartz at 20°, the solubility in 50 days is about three times that attained in 3 days.

Solid : Solvent Ratio is always important, and has an effect which is widely different for different minerals. With some, the silica solubility increases to a maximum (in unit time) at a solid : solvent ratio of 2 : 100 ; with others, notably quartz, the solubility continues to increase up to and beyond a ratio of 20 : 100.

Particle Size affects solubility in a manner which is sufficiently illustrated by the data already given for calcined flint dust.

The effect of each of these factors has been investigated in turn in the usual way by arbitrarily fixing the values for the other variables. In order to get comparable figures for a range of dusts it is necessary, of course, to standardise all the variables as far as possible. Particle size cannot be accurately controlled, but it can be determined in each case, and for the rest we use two sets of standard conditions :

(*a*) 3 hours at 100° with solid : solvent ratio 1 : 100.

(*b*) 3 weeks at 20° with solid : solvent ratio 1 : 100.

But there are other complications, too. In some cases where lime is a constituent of the mineral, the silica solubility is greatly affected by the presence of carbon dioxide in the solvent, even in the very small concentration represented by equilibrium with ordinary air. We have also found that admixtures which one would expect to be inert, such as certain metallic dusts, and some forms of carbon, can greatly reduce the silica solubility of a dust. Traces of potash or soda, whether caustic or carbonated, will largely increase silica solubility, and the same is true of small amounts of lime, but larger proportions of lime will reduce the silica solubility to nil.

Then again, the solubility may be localised in certain directions. An asbestos fibre is insoluble on its long sides but easily soluble at the broken ends, so that if it be allowed to fall on a neutral jelly containing phenolphthalein, red stains appear at each end of the crystal owing to the local high concentration of the alkali there liberated. That this is *caustic* alkali can be proved by laying a crystal, in the dark, on an exposed photographic plate which has been soaked in a developer minus alkali (hydroquinone and sodium sulphite) when black spots develop around the ends of the fibre or at any point of fracture in its length, where the liberated caustic alkali renders the developer active. When one considers the characteristic "dumb-bell" form of the "asbestosis body" formed by the fibre in the lung, it does seem extremely probable that the pathogenic reaction of asbestos is intimately connected with its localised solubility.

With ordinary industrial dusts matters are still further complicated by the fact that a number of minerals are usually present. In a sample of anthracite coal dust we recently collected, some twenty minerals could be identified with certainty under the petrological microscope.[1]

This being so, it is not, perhaps, a matter for surprise or disappointment that the connection between dust solubility and silicosis remains as yet obscure. The very knowledge, however, that the chemical properties of dusts are as complex and variable as we now see them to be affords a valuable correlation with the long known complexity and variability of the phenomena of silicosis. We know already that it is possible in various ways to minimise the silica solubility of an active dust. We, ourselves, found in our earlier experiments that caustic lime or cement dust added to an active dust might reduce its silica solubility almost to nil, and that a similar effect was produced by some forms of carbon though not

[1] We are indebted for this identification to our colleagues Dr. Brammall and Mr. Leech, of the Department of Geology.

by others. Recently Denny, a Canadian metallurgist, and his collaborators,[1] have shown that finely divided metallic aluminium will reduce the silica solubility of quartz, and have also secured evidence tending to show that admixture of aluminium powder with the dust of this quartz will prevent its producing silicosis in rabbits.

Here, for the moment, the story ends, but we feel that there is good reason to hope that if the chemical attack be pressed, in collaboration with the physiologist, it should be possible to ascertain precisely which of the numerous properties of siliceous dusts are really responsible for their damaging effects. It may yet be found that something other than silica is the active agent. Once we have this knowledge in our grasp it may well be but a short step to devise practicable means to inhibit the effects of dangerous dusts and so to save much human suffering.

[1] Denny, Robson and Irwin, *Canadian Medical Association Journal*, 1937, **37**, 1.

THE PRESENT LANDSCAPE OF NORTHERN LABRADOR IN RELATION TO ITS GEOLOGICAL HISTORY[1]

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In this article it is proposed to give a summarised account of the geological facts and later erosional processes to which the scenery and topographical features of the northern parts of Labrador are due. It was my good fortune to accompany as geologist the Grenfell-Forbes Northern Labrador Expedition of 1931, and I have already presented some of the observations made and the conclusions arrived at in a paper given to the Royal Geographical Society in the following year.[2]

During the course of the Brown-Harvard Expedition of 1900, Professor R. A. Daly was able to make the first observations on the geology of the north-east coast, and these he has admirably described in a chapter of Sir Wilfred Grenfell's book *Labrador: the Country and the People*, and elsewhere in greater detail.[3] In 1915 and 1916 on behalf of the Geological Survey of Canada, Professor A. P. Coleman extended some of Daly's observations.[4]

The greater part of the Peninsula of Labrador is a rolling plateau 1600 to 1800 feet in elevation which has a slight tilt towards the north-west, as was shown long ago by A. P. Low, of the Canadian Geological Survey.[5] As elsewhere on the great Canadian "Shield,"

[1] I am indebted to my friend, Dr. Alexander Forbes, of Harvard Medical School, and the American Geographical Society, for permission to reproduce in part my contribution to his book *Northernmost Labrador Mapped from the Air*, American Geographical Society, 1938.

[2] N. E. Odell, "The Mountains of Northern Labrador," *Geogr. Journ.*, **82**, 3 and 4, 1933.

[3] Reginald A. Daly, "The Geology of the North-East Coast of Labrador," *Bull. Mus. Comp. Zool., Harvard*, XXXVIII, Geol. Ser., **5**, No. 5, 1902.

[4] A. P. Coleman, "North-Eastern Part of Labrador and New Quebec," *Geol. Surv. Can.*, Memoir 124, 1921.

[5] A. P. Low, Report on Explorations in Labrador Peninsula, *Geol. Surv. Canada*, **8**, L, 1895.

of which Labrador forms part, there are innumerable lakes scattered over the surface, and many rivers make their way outwards by irregular channels from the low and almost indefinable "height of land," or watershed.[1] In contrast to the vast interior, the north-east coast of Labrador, north of latitude 56½°, is dominated by mountain ranges. These ranges fall into three main groups: the Kiglapait Mountains, signifying in Eskimo "sierra" or "saw-tooth" mountains, the most southerly range; the Kaumajet Mountains, or "shining-top" range, situated 60 miles farther north in the vicinity of Cape Mugford; and the Torngat Mountains, implying "home of the spirits," that stretch from Saglek Bay for 140 miles northward to the island of Killinek, which caps the peninsula at the entrance to Hudson Straits. Of the three groups the Torngats are the most important and extensive.

It is a matter of considerable geographical interest, as well as geological significance, that out of the many thousands of square miles of the ancient rocks of the Canadian "Shield," we find here on the far-flung extremity of north-east Labrador, and only here, a mountain-country of such pronounced relief. Whatever in the way of mountain chains may have traversed parts of Ontario and Quebec in the remote past (and there is ample evidence to show that they did), it is only on this extreme north-eastern rim of the "Shield" facing the Atlantic that bold ranges worthy of the name are at present to be found.

General Geology of the Torngat Mountains

As just cited, the rocks of this region form a part of the great Pre-Cambrian Shield of Canada. That is to say, they are composed of ancient crystalline types, gneisses, granulites, and schists mostly of various kinds. Resting on these between Saglek and Nachvak Fjords is a younger sedimentary series of slates and quartzites, etc., named by Daly the "Ramah Series," which may correspond with Huronian, or Keweenawan, the uppermost series in the Canadian Succession of pre-Cambrian rocks. Precisely how much, and what, of the Basement Series can be said to be co-eval with the Archean rocks of Canada is a debatable point, but Coleman considered it probable that Laurentian, Grenville, and Keewatin were represented. It must be realised that all these formations are entirely without fossil-content, which thereby renders questions of absolute age very difficult of determination. Not only granite-

[1] Since 1926 the northward trending watershed has formed the political boundary between the Province of Quebec and Labrador proper, which is under Newfoundland jurisdiction.

gneiss, but gneisses and granulites rich in red garnet and black pyroxene, are widespread and these are often injected by light-coloured aplite and pegmatite dykes and sills, and in places contain lenses of black amphibolite. Here and there all formations are cut by great dykes of pyroxenite and dolerite, and "swarms" of these in places give a remarkable appearance to the coast-line. Also occurring is a rock so rich in hypersthene as to form the quite uncommon type hypersthenite. Moreover, its rather rare related type charnockite as well as anorthosite, were found in the almost unknown dyke-form, and by their superior resistance to the effects of atmospheric weathering forming upstanding peaks or pinnacles on the summit-ridge of the range. These same rocks respectively occur in their more usual massive form in Mount Razorback and in the Kiglapait Group to the south. Further, kyanite-bearing rocks as well as the uncommon pseudotachylyte are to be found, indicating the especially high crustal stresses to which this region has been subjected; and sillimanite schist occurs in the far north.

The Kaumajet Mountains

Geologically distinct from the other ranges of Northern Labrador, this group is composed of a great mass of volcanic rocks, basic lavas and ashes, etc., which were laid down on the older land-surface of gneisses and schists. The attitude of the group is nearly horizontal, and their age is in doubt, but may be related to the Ramah Series.

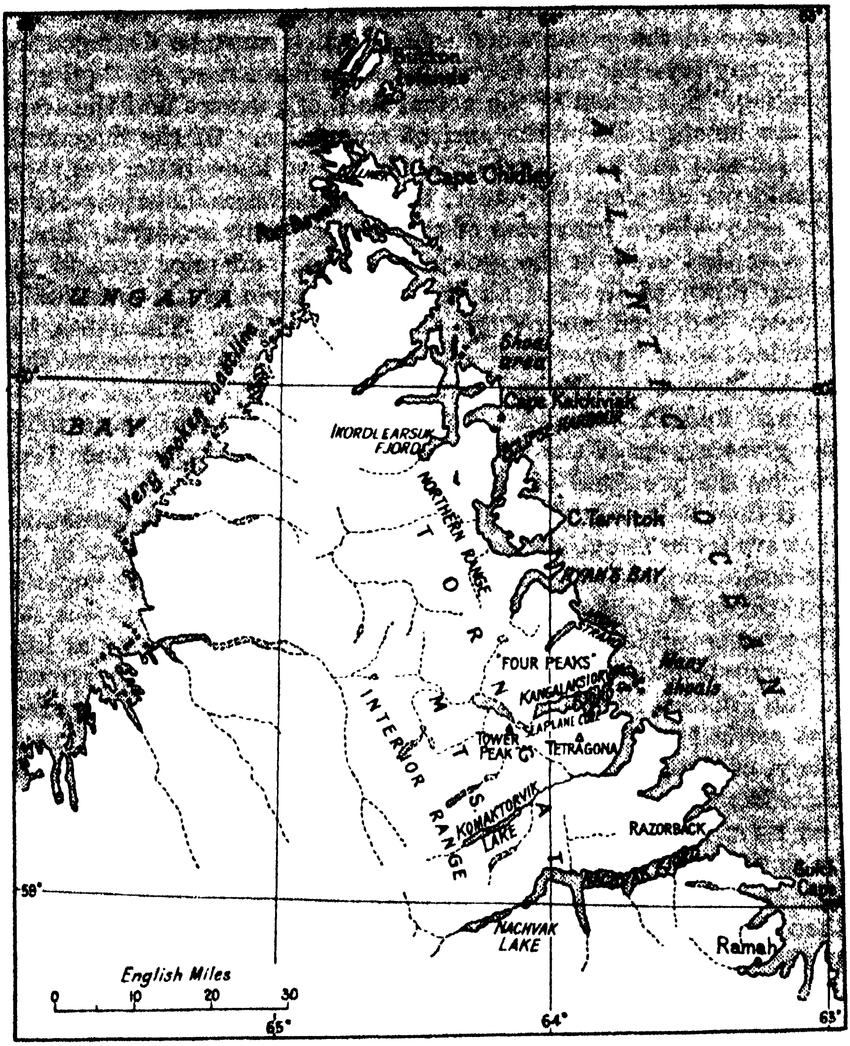
The Kiglapait Mountains

Visitors to the coast have long been told of the beautiful iridescent rock, labradorite, to be found in the district round Nain. This and its parent rock anorthosite, with gabbro and norite, form the striking massif of the Kiglapaits, the fine serrated range rising 100 miles southward of the Torngat Mountains. In its massive form the origin and history of anorthosite are one of the outstanding problems of geology. Sufficient in general, however, is known for it to be regarded as an intrusive rock of unusual type.

Structure and Morphology of the Torngat Region

As Daly himself has rightly declared, "the essence of a mountain mass is not form or height, but structure; its component rocks have been deformed, thrown out of their original levels, by tilting or folding. Initially the strata lay flat." [1] Such is undoubtedly the case in northern Labrador. The gneisses, schists, granulites, and other metamorphic rocks of the country, were of course origin-

[1] *Our Mobile Earth*, p. 251.



Northernmost Labrador
(rivers, etc., only approximate)

ally stratified rocks, intruded at a later period by various igneous types. The whole mass was afterwards subjected to great crustal stress and horizontal compression along lines oriented between north and south and north-west and south-east in the coastal belt. The rocks were thrown into folds, were upended, fractured, and faulted, and with continued compression and concomitant constitutional change they acquired the "banded" or layered condition

that is a mark of nearly all metamorphic rocks. This banded character of the gneisses and schists which must be distinguished from any layering due to original stratification or to "primary banding," is a token of the actual lines of pressure and the trend of the innate folds of the ancient mountains. Of the magnitude (amplitude) and full extent of the latter we know little, but there is evidence all along the coast, that not northern Labrador alone, but the southern parts also of the country were involved. But it is clear that early in the geological record, and prior even to the laying down of the Ramah Series, the whole of this mountain-country had been worn down almost to a plain. The south has remained so, a "peneplain," and it is important to appreciate that in the present mountains of northern Labrador we find not the original folds of formation, but merely the roots of those folds, the greater part of the latter having in the long ages since been lost by the processes of denudation.

If then it is only the truncated roots of the ancient mountains that are to be found all along the Labrador Coast, whether in the far north or in the south, to what are we to ascribe the superior elevation and boldness of the Torngat region as we now see it? Geomorphological evidence suggests that in geological times long subsequent to that of the original crumpling and crushing, namely in the Pliocene Period or somewhat earlier in all probability, there was vertical uplift of the region to an elevation far in excess, most likely, of the present mountains.[1] But during and since the uplift there have been atmospheric agencies operative to dissect by stream and river this elevated tract of country, apart from the action of ice during the glacial epoch, shortly to be referred to. It was, then, this elevation of the country at a later stage in its history, rather than the original folding of the rocks, that has given such eminence to the Torngat region. The strike and general trend of the uplifted remnants of these folds gives us an indication of the ancient mountain chain, which must have extended throughout northern and southern Labrador and to have coincided approximately with the present coast-line; but it is only in the northern country now under consideration that we have evidence of one epoch of mountain-building by re-elevation having been superimposed, as it were, on another, and tending thereby to reinforce the earlier. It would appear that we have in this region of north-east Labrador a very special area of instability manifesting itself in later geological time. Although the vast areas of the Canadian Shield covering the interior, in Quebec,

[1] H. C. Cooke, "Studies of the Physiography of the Canadian Shield," *Trans. Roy. Soc. Canada*, XXIII–XXV, Sec. 4, 1929–31.

Ontario, and Manitoba, especially, have been subjected to some movement, mostly that of depression following earlier elevation, they seem to have remained relatively stable during a long period of pre-Glacial time. The north-east coast, on the other hand, has been *relatively* up-warped, so that the Torngat region facing the Atlantic has performed a special function in relation to the rest of the Canadian Shield, and in consequence has established for itself an individual physiographic history.[1]

The Evolution of the Present Relief of the Torngat Region

Let us now briefly consider the probable course of events in the sculpturing of this northern area, and the establishment of its river-courses and fjords.

One important fact that the work of the aerial survey and the ground-observations ascertained is that several of the rivers of the Torngat region have their sources in the interior to the westward of the mountains, so that the water-divide, or "height of land," is not along the crest of the chief chain. These rivers rise on the western flanks of the Interior Range, and flowing eastward rather surprisingly cut right through the line of highest peaks of that range to reach the heads of the larger fjords. Part of their courses, deeply inset in the mountains, form veritable trenches which are occupied by lakes. Such are Nachvak Lake and Komaktorvik Lake and others to the northward. It should also be noted that, the fjords themselves occupy a like relationship to the coastal mountains, running athwart them to the open coast-line or to small archipelagos of islands. It can also be seen from the map that many of the rivers have a dominant and parallel direction, and a tendency in part of their courses to take more or less right-angular bends, forming in general an almost rectangular valley-pattern. This would suggest that their directions have been largely governed or controlled, by the ruling structure of the country, one direction, nearly N.N.W.–S.S.E., conforming to the prevalent foliation and cleavage of the rocks already referred to, and the other at right angles being determined by cross-joints or faults.

Now the phenomenon of rivers flowing "inconsequently," or "non-consequently,"[2] across mountain ranges has been observed

[1] It is here stated that the Torngat region has been *relatively upwarped* with respect to the interior of Labrador. Actually it may be that the latter has been *relatively downwarped*, leaving the eastern rim of the ancient plateau upstanding or emphasised in the Torngat area.

[2] Owing to the existence of W. M. Davis's term "inconsequent" for streams of a specific type, it may be preferable to introduce the word "non-consequent" for drainage that is not "consequent."

in other parts of the world, and it is usually explained, in a case like that of Northern Labrador, as being due to the river-courses having been established on the older land surface prior to the existence of the hills through which they now cut their way: that when the hills began to be formed these "antecedent" rivers were able by their own powers of erosion, and on account of the slowness of elevation, to maintain their courses against the uplift of the region. Such a thesis might be applicable to the area under review, and antecedent rivers most probably in fact once occurred there, since, as mentioned above, there is evidence of a geologically late (probably Pliocene) upwarping of the region: however, another and important consideration must be taken into account. From the scattered observations so far made throughout the whole area of Labrador and elsewhere, H. C. Cooke has shown that the land surface of the peninsula probably stood from 300 to 700 feet higher before the Pleistocene Epoch than the present surface does. If this were the case with the interior of the country we can well understand that the direction of drainage there may have been very different from the present, and indeed such outlying districts as that of the Torngats may have been considerably affected. But the uplift of the hinterland postulated can scarcely have been sufficient to produce the channels of the "non-consequent" rivers that penetrate the elevated tract now occupied by the Torngat Mountains. We have to take into consideration another factor in its history, namely that of the Pleistocene Ice Age, and the notable if not profound morphological effects which it and the subsequent Late-Glacial and Post-Glacial Sub-Epochs have left upon the surface, a matter that will now be discussed.

The Onset of the Great Ice Age, and its Effects

Whatever may have been the cause of this great event in later geological history, and there are reasons for supposing that it was brought about by a world-wide lowering of temperature, the evidences of the Glacial Epoch in Labrador are ubiquitous up and down the coast, as well as in the interior. As is the case in eastern Canada and in New England, great areas of bare polished rock-surface, scratched and furrowed, give eloquent testimony to the former presence and passage of a great regional ice-sheet. Particularly in southern Labrador and in the whole region south of Cape Mugford is this scoured and apparently planed-down character of the surface in evidence. In the Kaumajet Mountains, however, and northwards within the Torngats there is much superficially to suggest that the continental ice-sheet never entirely covered the

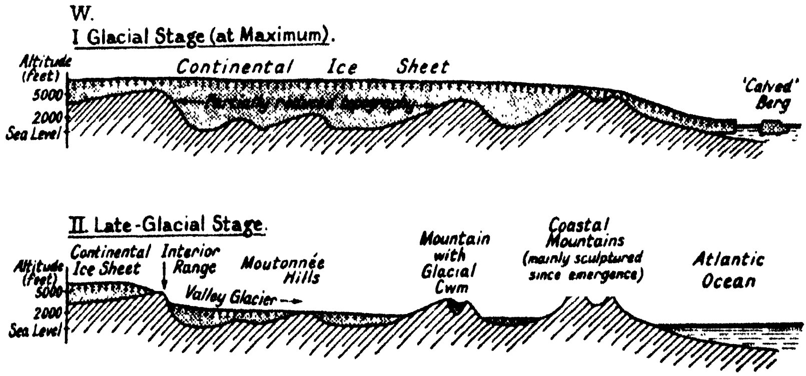
country ; it has been claimed that those mountains always rose as "nunataks" above the surrounding ice-sheets, and that contemporaneous mountain-glaciation gave rise to valley-glaciers that wound their way from their high local gathering-grounds to the sea. The evidence that previous observers [1] have adduced in support of this thesis is the jagged and broken character of mountain ridges, which in many cases rise above the smoothly moulded lower slopes, demonstrating, it is claimed, that the serrated crests surmounted the level of the former ice-flood and thereby escaped planation or polish. Moreover, since the higher rounded summits are in many cases found to be covered with a considerable thickness of rock débris, it has been thought, by these observers, that they too are indicative of having been subjected to a longer period of atmospheric weathering than could be accounted for since the lapse of the Glacial Epoch alone.

From experience in the Arctic elsewhere, however, the writer has found that the latter consideration depends very much on local circumstances. High terrain in proximity to the sea with ample moisture-laden winds in a region of intense frost-action has been observed in the mountains of Spitsbergen and East Greenland to produce thick coverings of frost-riven stony soil, entirely since the recent disappearance of the continental ice-sheets that have been withdrawn into the interior. Moreover, the sharp jagged peaks often to be found in the coastal belts of these regions can be most plausibly explained, in the writer's opinion, by this same intense frost-action that has operated upon them for a longer period since their emergence from ice-covering than upon the higher, still partly névé-mantled, tableland of the interior. In Northern Labrador the writer found actual evidence of ice-polished rocks, at about 4,700 feet on one summit of the Interior Range. Other similar polished surfaces, also, at 3,000 feet and over were encountered, which could not be considered as due to the agency of merely local mountain glaciers, but only to glaciation of a larger kind.

It is known that during the Pleistocene Glacial Epoch, of which at least four and possibly five principal ice-advances can by their "drifts" be demonstrated in central North America, the radiation area or centre of dispersion of the ice, as far as extreme North America is concerned, was in the central interior parts of Labrador, having apparently shifted progressively northward in the course of its existence from about latitude 51° to 57°. While present investigation has revealed no definite evidence of more than one glaciation in Northern Labrador, yet it seems to be quite clear that during

[1] Coleman (*op. cit.*, p. 26) in particular.

the greatest advance of the ice from the Labradorean centre the Torngat and the Kaumajet mountain ranges were both entirely submerged (see Section I). As for the Kiglapaits, the work of E. P. Wheeler, of Cornell University, has gone far to show that they also were completely inundated by the continental ice-sheet.[1]



Diagrammatic section through Northern Labrador during the Glacial and Late Glacial Stages.

(Assumed corresponding section through country: *ca.* 25 miles. Vertical scale greatly exaggerated.)

Now it is a debatable point whether this ice-sheet, in moving seaward from central Labrador over the Torngats and other ranges, deeply eroded or greatly modified the whole of the old pre-Glacial elevated land-surface to which reference has earlier been made. At any rate the major modification and sculpturing of the landscape would appear to have come afterwards, in "Late-Glacial" as well as "Post-Glacial" times. In the opinion of the writer, however, it is probable that these ranges were not very deeply submerged by the ice-sheet from the interior.

The Late-Glacial and Post-Glacial Stages. (See Section II.)

Although the actual depth of ice covering the region may be in doubt, it would seem that on its gradual retreat into the interior it left great valley-glaciers that wound their way eastwards from the ice-cap through devious channels to the sea. At first only the higher coastal mountains would emerge above the mer-de-glace, and then as more and more became uncovered, so ever increasing restriction and confinement of the ice-flood amongst the mountains would result. The great "through" valleys, with all the usual characters of glacial erosion, which open from the interior athwart

[1] Personal communication. See also E. P. Wheeler, "The Nain-Okak Section of Labrador," *Geogr. Rev.*, **25**, 1935.

the Interior Range, must have derived their formation from the time when the continental ice-margin stood just westward of that Range. This may be called the Late-Glacial Stage or Sub-Epoch, when so many of the immense transverse trenches were moulded and which now contain long deep lakes. There is little doubt, too, that at this stage much of the sculpturing of the fjords themselves was carried out by the strong and well-armed streams of ice passing seaward through them. And while these trench-like features were suffering active erosion by moving glaciers from the immense reservoir of the interior, we must picture the vast upland and flatter areas to have been meanwhile relatively protected by the immobile névé sheets resting upon them. Such a stage in the cycle of "glacierisation"[1] is to be found in significant degree at the present day in the mountains of north-east Greenland, where the writer has described a similar process for the carving out of the great trunk valleys and fjords while the uplands are relatively protected.[2]

As that portion of the ice which was engulfed in the mountains progressively diminished, more and more summits would begin to raise their heads above it to become islands, or "nunataks," in the mer-de-glace. And then would commence the process of frost-action and fretting of the exposed summits from the more rounded forms to the sharper crests and peaks. The so-called Cycle of Mountain Glaciation has commenced. This frost-splitting process, which indeed has continued into the Post-Glacial Sub-Epoch of the present day, has been responsible for some of the most important morphological effects that are to be found throughout these northern mountains. Let us briefly consider the succession of events inherent in this process, and endeavour shortly to trace the effects of it upon the area as the degree of glacierisation waned.

It was found that most of the summits of the Interior Range of the Torngats in particular were rounded, or even of contour. Innumerable examples are nearly flat-topped or but slightly inclined at their crests. Their flanks are, however, hollowed and scalloped. Now it may be said in general that hollows or cwms that were formed by stream-action, must have been left upon the flanks of hills prior to glacierisation of the region. These then would become occupied by snow and later glaciers, as glacial conditions increased, and the cwms and cirques become correspondingly emphasised.

[1] "Glacierisation" implies the inundation of land by ice; while "glaciation" refers to the erosive effect of ice upon the land over which it flows (Wright and Priestley, *Glaciology*, p. 134).

[2] N. E. Odell, "The Glaciers and Morphology of the Franz Josef Fjord Region of North-East Greenland," *Geogr. Journ.*, **90**, 2 and 3, 1937.

Moreover, it has been found both in Greenland [1] and Antarctica and elsewhere that such cwms and cirques [2] may survive through conditions of complete inundation by a regional ice-sheet, the latter having had insufficient erosive powers to destroy them. An interesting and significant feature, also, which is evident in many of the aerial photographs, is the almost ruling situation of cwms on the eastern flanks of the mountains of the interior, especially of the Interior Range. Many of these hills have smooth rounded contours on all other sides, but on the east are abruptly broken by often deeply scalloped cwms, which sometimes contain small glaciers. As opposed to the view that these hills represent the extreme "moutonnée" form of erosion beneath a great ice-sheet, it would appear that many if not all of them have acquired their broken or scalloped eastern sides by the erosive action of small glaciers and névé masses that tend to linger longest on this colder and more sheltered aspect, after all snow and ice has vanished from the exposed quarters. Such a view, then, dates these widespread features from a time when the great majority of the summits had already emerged and reared their heads well above the lingering ice-flood. The sculpturing of the cwms has been in progress right down to the present day, for we still see small relict glaciers, armed with rock-material from the surrounding cliffs, continuing the process in them, though evidently with much-reduced vigour owing to the glaciers being in a waning condition.

Now in the Coastal Mountains are situated the most jagged and serrated crests, of which Mount Razorback (Plate 1), "The Four Peaks," etc., are excellent examples. In contrast to the summits nearer to, and still partly encumbered with, the continental ice-sheet, these coastal peaks would for a longer period have raised their heads above the ice, as well as any local protective snow covering, and thus become exposed to the intense action of frost and atmosphere, which operates with such powerful effects in these frigid yet amply humid regions.

With the recession of the continental ice-sheet away from the western flanks of the Interior Range into the interior, the Post-Glacial Stage can be said to have been inaugurated. Then would

[1] Examples were found by the writer along the border of the Inland Ice in the mountains of North-East Greenland (*v.* N. E. Odell, *op. cit.*, p. 250).

[2] "To avoid confusion and misunderstanding it would seem advisable to the writer to restrict the word cwm (Welsh), or corrie (Scottish), to the individual high mountain valley . . . and apply the term cirque to the larger, often composite, feature which is so characteristic of the great Alpine ranges of the world. Such a distinction is in common usage amongst mountaineers" (N. E. Odell, *idem*, p. 246).

cease the supply of ice to the great trunk glaciers flowing eastwards, and they would speedily lose their power of further moulding and carving out the great valleys that contained them. Previously, in their descent across the range from points westward of it they had given in the course of time and by prolonged erosion an eastward grade to their channels. These channels have since been occupied by the "non-consequent" rivers, to which earlier reference has been made, and which have their sources anomalously on the "wrong" side of the mountains through which they cut. The deep basins to be found at intervals along these river-valleys, in which now lie beautiful lakes, are to be explained as having been largely carved by the former vigorous valley-glaciers at such places where restriction gave increased speed and therefore erosive power to the ice-streams. And the latter were in many cases confluent streams, and therefore provided with additional ice in these localities. Moraines and outwash gravels have also assisted in damming up some of these lakes. Not that there is as much evidence as might be expected of débris ultimately derived from the action of the former glaciers, and such erosion-products, if they ever existed, must have been carried to the coast and dropped in the deep off-shore waters of the Atlantic.

And so we come to the last stage in the Post-Glacial march of events, which brings us right down to the final touches in the sculpturing of the landscape as we now see it. The higher groups of hills, often still rounded or flat-topped—portions of the old and perhaps little modified pre-Glacial surface, were being gradually scoured and abraded by the mountain glaciers lying on their flanks. These glaciers, where favourably situated and well-conditioned for the work of erosion, have progressively deepened the cwms in which they lie. Where the lapse of time since the recession of the formerly overriding continental ice has been short, as in the case of the Interior Range itself and adjacent groups, the majority of summits are merely scalloped, and a few scattered cwms only are in evidence even yet upon the almost unmodified surface. But where, as in the Coastal Mountains, and other coast-wise groups, summits have long been subjected to the work of mountain glaciers alone, the latter converging upon the higher areas have almost, or entirely, carved away the older surface of smoother contour. From the considerable summit-area of Mount Blow-me-down, to the more restricted table-top of Tower Peak, north of Komaktorvik Lake (Plate II), we find varying relicts of the old erosion surface, and significant evidence of the physiographic history of the region: a cycle of mountain glaciation has slowly and effectively impressed

itself upon the features derived from an earlier continental one. The agent which last of all is giving its finishing touches to these hills is that of frost-action, which, as earlier mentioned, is such a potent cause of destruction in the Arctic provided ample moisture is available. At the present time more wearing down of the peaks and summits is to be attributed to this process than to the action of the small waning "glacierettes" that still lie in their recesses.

As far as the Kaumajet Mountains are concerned, our ascents in the range, and more especially the aerial photographs, clearly show them to be less dissected than the Torngats, but evidence was found that they were completely overridden by the continental ice-sheet.

In the Kiglapaits Wheeler has found unquestionable evidence of glaciation to at least 2,600 feet and more: in fact, no upper limit of glaciation has so far come to light. From the air a striking view of glacial scour of even the highest summits is obtained, with the result that the massive anorthosite rock is etched out along the weaker zones of the foliation and cleavage to give, with snow infillings, a most remarkable banded appearance to the terrain.

Space does not permit of more than passing reference to the interesting old shore-lines and raised beaches to be seen at intervals along the whole length of the Labrador coast. They mark the general uplift of the country, which, as Daly has pointed out, would seem to be attributable to the gradual relief of the region from its former load of continental ice. It is well known that evidence of such up-warping has been ascertained in Europe and the British Isles, and for the same reason: that the earth's crust is not too rigid to bend under sufficient loading, and to recover when relieved of that load. Along the Labrador coast, by means of raised beaches as well as by noting the limit of glacial boulders not shifted by the once higher sea-level, Daly showed that there has been differential uplift such that, in general, localities to the south have been raised more than those farther north. Coleman concurred with this view, and my own observations indicate briefly that in the extreme north of the peninsula at Ikordlearsuk there is no positive evidence at all of rise of the land. The existence of this tilt of the land northwards suggests that the southern area has been relieved of its ice-load sooner than the extreme northern districts, which are still relatively depressed though to an unequal extent. On the other hand, the latter may never have been so encumbered with ice, and therefore never depressed to the same extent as the more southern area over which the Pleistocene ice-cap had its centre and presumably its greatest thickness.

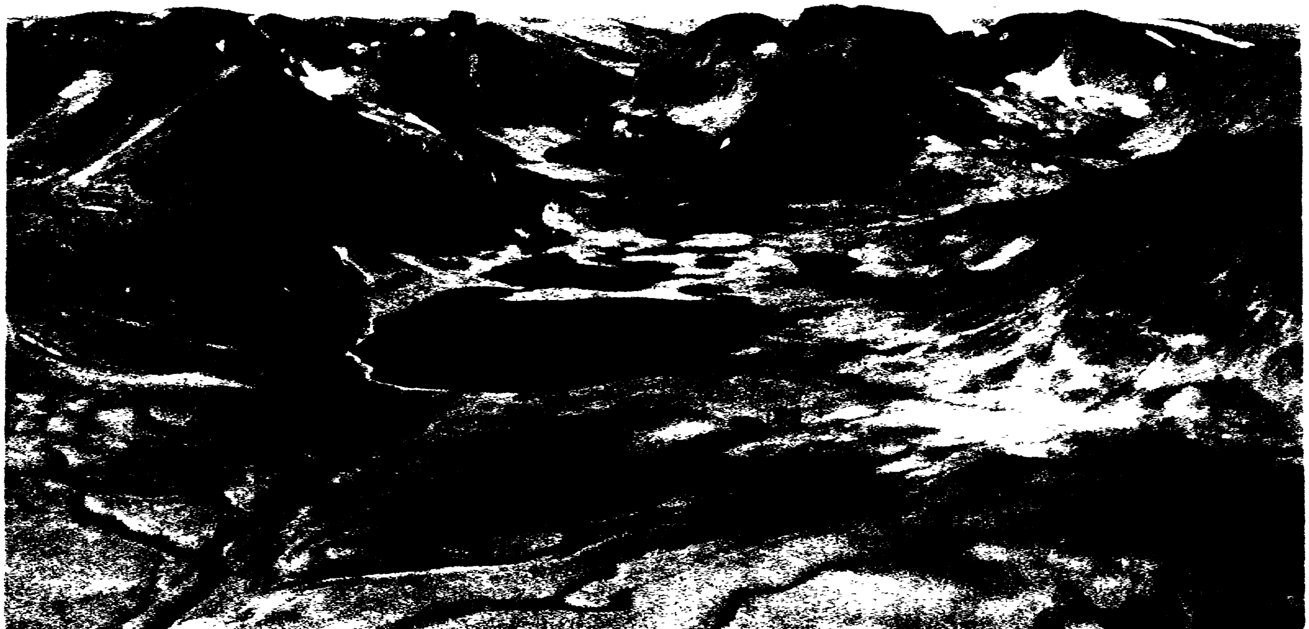
But apart from the evidence and theories of the "pandits,"

PLATE I

Mt. Razorback (resistant mass of intrusive charnockite): a characteristic coastal frost-riven feature.

N.B.—Cwm-development and relict "glacierettes", also moutonnée forms in foreground

PLATE II



Looking westward across Interior Range, north of Komaktorvik Lake.

N.B.—A landscape largely due to Late-Glacial sculpture. Note relics ("Tower Peak," etc.) of old land surface ("Glacial peneplain"?): truncated spurs, due to earlier "through" glacier from interior; and cwms and cirques.

there are the reports of the fishermen-settlers along the coast, that rock-ledges and reefs have during the last 70 or 80 years become notably less covered by water, and that passages, or " tickles " as they are locally called, which were once navigable by their fishing-boats can now no longer be used. Such reports are many and wide-spread, and although only qualitative evidence, cannot be wholly dismissed. It is sincerely to be hoped that quantitative observations, by the fixing of bench-marks along the coast, may yet throw positive light upon this and the many other fascinating problems with which Labrador abounds.

MAMMARY ACTIVITY

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Introductory

The control of quantity and quality of mammary secretion is obviously a matter of great practical as well as theoretical interest. The very existence of the whole class of animals to which man belongs depends on the efficient functioning of the mammary gland, and the fount and origin of our immense dairy industry (consumer figures for Great Britain alone show an annual turnover of more than £160 millions) with its supremely important place in national nutrition and fitness, is the bovine udder.

In the course of centuries of selection by shrewd animal husbandrymen, the cow has become, in modern times, an animal in which mammary function is greatly hypertrophied. Not only does she produce far more milk per day than her ancestors, but she continues to lactate for greater periods of time. The undomesticated cow may secrete 150–200 gallons of milk over a period of four months or so; a good modern cow will produce six to ten times as much milk, over a period of ten or more months, during a large part of which time she will be in calf and therefore providing also for the not inconsiderable demands of the growing fœtus. Quite apart from the material of the fœtus, the solids which she will secrete in the milk of a single lactation is in the neighbourhood of four times the total weight of solid material in her own body.

There is a certain amount of uneasiness amongst some of the more intelligent breeders of dairy cattle with regard to the immense strain that is being undergone by the modern cow. This uneasiness is ascribed in the first place to their belief that reproduction difficulties in high-yielding dairy stock are increasing, in the second place to the apparent liability of high-producing cows to contract udder infection and other diseases, and in the third place—though this fact is not taken, as yet, so seriously as it ought to be—to the realization that the nutritional quality of the milk usually begins to suffer where quantity is the sole criterion of selection. Their

qualms are not lessened when breeders of dairy cattle consider the serious condition in which the poultry industry finds itself as the result, it would seem, of acute functional overstrain brought about by uncontrolled breeding for egg production.

How far this anxiety as regards the future of the modern dairy cow is justified is still a moot point. Whatever view is taken, there can be little doubt that what is urgently needed in dairy science is far greater knowledge of the physiology and biochemistry of milk secretion, particularly of milk secretion in the specialised modern ruminant, so that the maximal physiological capacity may be safely and regularly attained, and that the danger signals of approaching overstrain may be recognised in good time and guarded against. Our knowledge of lactation at present is rudimentary. To mention only a few of our chief ignorances, we need a real insight into the functional histology of the mammary gland, into the functions of its smooth muscle, into its lymph drainage, into the enzyme mechanism in the secretory cells and the sequence of chemical changes taking place in these cells, into the energy requirements for secretion, into the nature of the precursors in blood of the milk constituents, and into the control of mammary activity by the endocrine glands.

Of these fields there are two, namely that of the influence of hormones on mammary development and lactation, and that of the nature of those materials in the blood from which the specific milk constituents are made, that have received a good deal of attention during the last few years. The experimental results obtained are of no small interest and importance. It is proposed in this article to deal briefly with some of the recent findings in these two fields.

Hormonal Control of Mammary Secretion

It has been suspected for many years that the hormones of the pituitary gland are concerned with the initiation and control of mammary function; lately this relationship has been examined in more detail. The secretions of certain other endocrine glands—the thyroid, the gonads and the suprarenal, which are controlled, at least in part, by the pituitary gland—have also been found to be concerned with efficient lactation. A large amount of experimental work has been done in this complex but fascinating field since 1928. Much of the information obtained is provisional and tentative, and is insufficient, as yet, to form a satisfactory picture; nevertheless, during the past five or six years some of the main outlines have begun to take shape.

Development of the Mammary Gland.—Lactation normally follows

parturition, but the young mammary gland, both before and during pregnancy, undergoes a lengthy process of preparation before milk production is possible.

In the female calf there is a slow increase in the size of the udder and teats with the general growth of the body, but these organs remain relatively small until puberty, when growth becomes more rapid. This increase in growth rate is coincident with the onset of ovarian function, and is, in fact controlled (but not necessarily directly controlled, see below) by a hormone secreted by the ovary. In the virgin heifer each ovarian cycle leads to a small increase in the size of the udder. The latter diminishes in size again between periods of œstrus, but the regression is less than the access of size at œstrus, and the net effect of repeated ovarian cycles is cumulative in largely increasing the udder size. Internally, the udder undergoes at this stage changes which are mainly an increase in the size and complexity of the duct system (in which milk will be stored after secretion and through which it will eventually be drained towards the teats), with relatively little development of the actual milk secretory cells.

Different species of mammals vary a good deal in the extent to which their mammary glands respond to the œstrogenic hormones, but there is good experimental evidence to associate the progressive increase in the complexity of the duct system in the bovine, at least during puberty, with the secretion of œstrone by the ovary.

Recent work by Gomez and Turner with small animals (1937) appears to show that œstrone is inactive in this direction in animals from which the pituitary gland has been removed. If an appreciable portion of the pituitary gland is left, œstrone retains its activity. They also show that whereas normal rat pituitaries implanted daily into guinea pigs produce no proliferation of the duct system, yet if pituitaries *from rats which have previously received* 10 *to* 20 *daily* injections of œstrone are implanted daily, then development of the duct system, similar to that observed in an intact animal which has received 10 daily injections of œstrone, is obtained. If this work is confirmed, it must be accepted that the effect of œstrone on mammary development takes place as a result not of the direct stimulation of the mammary gland, but indirectly by the production, under the influence of œstrone, of a specific mammary-gland-growth-stimulating hormone by the pituitary.

There is a further increase in the size of the udder during the early months of pregnancy. This has been shown to be due largely to the joint effect of the œstrogenic hormone (secreted by the ovary and possibly also by the placenta), and the hormone secreted by the

corpus luteum (found for a short time in the ovary after an ovum is liberated, and which persists if a fertilised ovum becomes implanted in the uterus) to which the name progesterone is now usually given. This substance, like œstrone, is a sterol derivative. This further enlargement of the udder is associated internally not only with an increasing complexity of the duct system, but also with a large increase in the amount of lobule alveolar tissue containing the big columnar cells that are ultimately to secrete milk.

In some species of animals œstrone alone (possibly acting indirectly through the pituitary gland) appears to be able to cause the complete development of the mammary gland to the stage at which it is ready to function, but in the bovine, with which we are mainly concerned here, the successive action of the two hormones seems necessary.

The Initiation of Milk Secretion.—In the later stages of pregnancy the mammary glands are so well developed that milk secretion could readily take place. But lactation is nearly always absent till after parturition, when, after a rapid onset, it becomes firmly established. It was first suggested by Gaines and Davidson in 1926 and by Corner, and has since been supported by Nelson on the basis of a considerable volume of experimental work, that a factor is at work in the later stages of pregnancy actively *inhibiting* lactation. Nelson considers that the factor is either the ovarian hormone, œstrone, which continues to be secreted by the ovaries during pregnancy, or some similar hormone, possibly secreted by the placenta, though here the evidence "though positive, is disappointing." The restraining influence is removed at parturition.

Certainly at parturition a powerful positive factor comes into play. It was shown in 1928 by Grueter that administration of crude anterior pituitary extracts to animals whose mammary glands were fully developed, but from which the ovaries had been removed, resulted in milk secretion. It is also known that the complete removal of the anterior-pituitary gland during pregnancy prevents lactation, but that in certain species and individuals which survive this rather drastic operation lactation may sometimes be induced by the administration of fresh pituitary extracts. It may be taken as reasonably well established that the main factors which initiate lactation are (1) the removal of some inhibitory influence associated with the œstrogenic hormones; (2) the liberation of, or increased availability to the mammary gland of, an internal secretion (or secretions) of the anterior lobe of the pituitary gland. Work of Selye, Collip and Thomson in 1934 and 1935 has suggested that mechanical distension of the uterus may also be a contributory

factor, at least in the rat, to the prevention of lactation before parturition.

On the positive side, these workers have also fairly recently shown that non-pregnant rats and mice, suitably prepared beforehand by use of the ovarian hormones, can be induced into active lactation by placing them in contact with an actively suckling litter. The stimulus to secretion here would appear to be, in the first place, mechanical, via the nipples. The view of these investigators is that milk secretion is largely consequent upon the liberation of the lactogenic hormone of the pituitary as a nervous reflex induced by the mechanical stimulation of suckling. Common experience indicates clearly, however, that the initiation of milk formation in the normal mammary gland after parturition is independent of suckling.

The Lactation Hormone.—The phrase "lactogenic hormone" has just been used. This idea of a single lactation hormone is derived from work of Riddle and others who have shown that relatively purified anterior-pituitary extracts retain their lactogenic powers when administered to animals suitably prepared beforehand. The name "prolactin" has in fact been given to this hormone.

Riddle found that his lactogenically active material also caused a marked increase in the weight of the crop glands of the pigeon, and on this basis of assessment, highly purified preparations have been obtained which are active in increasing the size of pigeon crop glands in doses as low as the fiftieth of a milligram. There has been for some time, however, an accumulation of evidence which would indicate that activity in stimulating the pigeon crop gland is not always associated with true *lactogenic* activity. Thus Gomez and Turner found in 1934 that the "purified lactogenic hormone" would not re-establish lactation in animals from which the pituitary gland had been removed during full lactation, whereas the mammary glands of two similar animals which were given a daily dose of ground, aqueous suspension of fresh pituitary gland remained functional.

Still more recently (1937) it has been shown by Nelson and Gaunt and also by Gomez and Turner that for the initiation of lactation in animals from which the pituitary gland has been removed, either the cortical hormone from the adrenal gland, or the hormone of the pituitary which stimulates the adrenal cortex to secrete, is necessary in addition to purified "prolactin," purified, that is, as regards its crop-gland-stimulating power.

Very recent work of Folley and Young (1938) has strongly accentuated this difference between such "prolactin" and the

hormone or hormones of the anterior pituitary gland which increases milk flow in the lactating cow. It had been previously found at Shinfield that the administration to cows past the peak of lactation of either a crude saline extract, or an alkaline extract, of fresh anterior lobe tissue caused a substantial but temporary increase in milk volume. At the same time the composition of the milk was somewhat altered, there was a slight fall in the percentage of non-fatty solids, together with a slight rise in the percentage of fat in the milk. Crude extracts of the anterior pituitary gland possess many hormonal activities, some of which by suitable methods may be separated by fractionating the crude extracts. There is, for example, the crop-gland-stimulating fraction called "prolactin," the portion that stimulates the growth of the thyroid in immature animals ("thyrotropic hormone"), the "glycotropic" or "anti-insulin" fraction which prevents the hypoglycæmic effect of insulin, the "diabetogenic" fraction which induces the symptoms of diabetes mellitus in normal dogs. In the very recent work just referred to, various fractions were assayed for their specific activity in other directions and were then tested out on lactating cows for their ability to increase milk yield during declining lactation.

A single injection of crude saline extract of the anterior pituitary gland, and of the thyrotropic and prolactin preparation *from the fresh gland*, resulted in each case in a substantial increase in the milk yield for 5 or 6 days. Only slight changes were found in the compositional quality of the milk. With preparations from the *dried gland*, a single injection of prolactin administered in a dose equivalent, as regards crop-gland stimulation, to the dose of prolactin from the fresh gland had *no effect* on the milk volume or composition. The same lack of effect was shown on injection of the thyrotropic fraction prepared from the dried gland. When much larger doses of "dried gland" prolactin were used there followed a substantial and prolonged increase in milk volume, with a rise in lactose content. The lactogenic properties, as regards the lactating cow, of the thyrotropic preparation from the fresh gland were unaccompanied by any detectable pigeon-crop-stimulating activity.

Thus it has been found both that purified "prolactin" with crop-gland-stimulating powers has no true lactogenic activity and that a fraction from the pituitary with no "crop-gland" powers is strongly lactogenic. The Shinfield workers conclude (i) that for true lactogenic assay, the pigeon crop-gland method is misleading, (ii) that there is probably no single specific lactation hormone.

The Thyroid Hormone and Milk Secretion.—Normal milk secretion

depends not only on the presence of an intact pituitary gland, but, also as was first shown in 1918, on the adequate functioning of the thyroid gland.

As has been stated already, it has been fairly recently demonstrated that the pituitary gland secretes an agent (the thyrotropic hormone just mentioned) which increases the size of the thyroid gland in the immature animals. This, or some closely allied hormone also derived from the pituitary, increases thyroid activity, thus controlling the total metabolic rate of the body, the rate at which individual tissues use up oxygen and excrete CO_2, and probably also the arterial blood sugar level. Reece and Turner showed in 1937 that the pituitary glands from actively lactating dairy cows contained rather more of the thyrotropic hormone than the corresponding glands of dry cows, or of beef cows.

Six years ago Graham found that when a cow in declining lactation was given either desiccated thyroid tissue to eat, or was injected with synthetic thyroxine, there was a rise in the volume of milk secreted and also in the percentage of fat in that milk. Later, more carefully controlled experiments of Folley and White indicated that the volume of milk could be increased by 25 per cent. or more, and the *percentage of* fat and non-fatty solids in the milk increased very markedly, by administration to lactating cows of 10 mgm. of synthetic thyroxine per day. Lactation was raised to a higher plane of efficiency, and the increase in volume and quality of the milk was maintained so long as the thyroxine treatment was continued. The phosphatase content of the milk, which, in general, is an inverse indicator of the efficiency of the secretory cells, *decreased* during the treatment period, but rose again as soon as treatment was discontinued.

Thus the administration either of certain of the hormones of the pituitary gland or of thyroxine will stimulate milk secretion. Is it legitimate to connect the two? Does the pituitary act indirectly through its power to stimulate activity of the thyroid gland? Are the results of thyroid gland or thyroxine treatment due to a rise in the metabolic rate of the mammary tissue, or due to the increased blood flow through it that follows such treatment and which will bring increased quantities of the milk precursors to the secretory cells, or to an increased mobilisation of sugar which is also a concomitant of increased thyroid activity? Herman, Graham and Turner (1938) in some very recent experiments have found that whereas the administration of thyroxine to cows in declining lactation produced the effects on milk yield and quality described above, its administration to cows at the peak of milk production (3–7 weeks

after parturition) resulted in a diminution of both milk volume and fat yield. They suggest that the rise and decline of the lactation curve represents a rise and decline in the secretion of the complex of pituitary hormones which influence, directly or indirectly, the process of lactation. At the height of lactation, the production of the thyrotropic hormone by the pituitary has reached a maximum, and the animal is virtually on the verge of a hyperthyroid condition. Injection of additional amounts of thyroxine at this stage may depress milk secretion as a result of the increased competition of the hyperactive tissues of the various organs of the body for the not illimitable supply of those materials transported by the blood which are needed to meet the tremendous demands of the secretory cells of the udder at the peak of the lactation curve. Whether this is indeed the case, only further experiment will show. One observation which may eventually lead to a convincing explanation is that the arterial blood sugar level, which appears to be correlated, even in the normal animal, with milk yield, is raised by thyroxine treatment.

Other things being equal, the most valuable cow is the persistent one, whose lactation curve, after the peak has been reached, falls very slowly. Persistency of milk secretion is due very probably, at least in large part, to the maintenance of the secretion of the thyrotropic, or thyrotropic + lactogenic, hormones by the pituitary. Where natural persistency of secretion is poor, the response to administered pituitary hormone or to thyroxine treatment would be expected to be greater. Apart from such administration, the dairy cow is the creature of her pituitary and cannot rise above the functional efficiency of this gland.

Œstrogenic Hormones and Established Lactation.—On the basis of the suggestion mentioned earlier, that œstrogenic hormones inhibit the secretion of milk by the mammary gland when it is fully developed during the later stages of pregnancy (and when, in fact, it has already secreted, but not emitted from the gland, a certain quantity of colostrum), it might be anticipated that the administration of such hormones to animals in full lactation would either seriously interfere with milk secretion or would completely inhibit it.

This has recently been put to the test at Shinfield, where it has been found that in the rat, œstrone markedly inhibits lactation, as judged by the effect of its administration to the mother on the weight and survival rate of the young sucklings. An inhibitory effect is also claimed clinically, in the human. With the actively lactating cow the effect is much smaller and is somewhat unexpected. Œstrone administration diminishes somewhat the volume of milk

secreted in a given time, but at the same time considerably increases the compositional quality of the milk as regards non-fatty solids, an increase in the percentage of which may persist for weeks after a single injection (or a short course of injections) of œstrone has been given, and after the immediately depressing effect on milk volume has worn off. Another curious effect of œstrone administration to the cow is the large increase in the amount of phosphatase in unit volume of milk.

A few months ago, it was found by Dodds and his colleagues that certain synthetic substances made in their laboratory had œstrogenic effects as great or greater than those of the naturally occurring œstrogens. Amongst these synthetic substances was one "diethylstilbœstrol", a stilbene derivative, which had an effect very similar to œstrone on the uteri of immature rats and rabbits and ovariectomised rats, and on the vagina and mating reactions of the immature rat, etc. It has very recently been found that this material, like œstrone, inhibits lactation in the rat. In the cow in declining lactation, the implantation of a small tablet of diethylstilbœstrol subcutaneously does not, it would appear, either inhibit or increase the volume of milk secreted, but improves its chemical composition, both as regards fat percentage and also as regards the percentage of non-fatty solids. The lactose content is increased, and there is a sudden rise in milk phosphatase followed by a rapid drop to about the normal level. Either directly or indirectly, this synthetic œstrogen has, therefore, an action comparable with that of natural œstrogens. Œstrogenic activity and the ability to stimulate the active udder to improve the compositional quality of milk seem to be linked together.

The practical question arises as to whether in certain cases in which cows secrete milk low in non-fatty solids, which might lead to legal proceedings if such milk with a non-fatty solids content of less than 8·5 per cent. were sold to the public, a synthetic œstrogen might not be administered clinically. So far the experimental basis for such a procedure is not sufficiently secure to permit its recommendation, but there is little doubt that the recent experimental work just described is bringing closer the solution of this practically important problem of "non-pathological low solids-not-fat".

The Supply of Materials to the Mammary Gland

Remarkable chemical transformations and syntheses take place in the milk-secreting cells. Such are the production of lactose, milk fat and caseinogen, substances quite specific for the secretion of the mammary gland, and produced nowhere in nature outside it.

The mammary gland, nevertheless, is like all other organs of the body in being entirely dependent for its functional activity on its blood supply. All materials out of which the specific milk constituents are to be made must be transported to it by the blood stream, and though efficient secretion, as we have seen, depends in part on the direct control of the secretory cells by hormones, it depends no less on the maintenance of a constant supply to those cells of the right raw materials from without, both for milk production and for tissue maintenance. Gomez and Turner state pertinently that "the lactogenic hormone exerts a direct influence upon the lobule epithelium (of the mammary gland) thereby initiating the secretory function, and by continued stimulation maintains the activity of the secretory cells *provided the precursors of milk are ample.*"

These two aspects of mammary secretion, namely, (i) hormonal control of the gland and (ii) supply of raw materials to it, are clearly very closely interlinked, progress in our knowledge of the one depends on, and is assisted by well-established findings as regards the other. In the last few years, advance has been slower in the technically more difficult field of determining the chemical identity of the materials from the blood stream that are used by the gland, and how these materials are synthesised into specific milk constituents in the secretory cells.

Definite progress has been made, however, and in the next few paragraphs some account will be given of recent developments.

Technique of Investigation.—As regards the identity of the milk precursors, the direct method of comparing the composition of blood entering and of blood leaving the mammary gland was adopted some years ago. At first, owing to uncertainty of technique, surprising conclusions were drawn as to the nature of these precursors. These conclusions need not now be taken seriously.

Blood leaving the udder of the cow—with which animal we are here mainly concerned—can be obtained with relative ease from the abdominal subcutaneous vein (or milk vein). This vein is connected by anastomoses with two smaller veins which assist in the drainage of the udder, and it is generally believed that the blood in the abdominal subcutaneous vein represents a mean sample of the blood issuing from the mammary gland at any given moment. The blood entering the udder comes mainly from an artery which is deep and inaccessible. But arterial blood, wherever taken, is at any given moment almost exactly uniform in composition throughout the whole arterial system, so that the blood from any artery may be taken as representing the blood entering the mammary gland.

Nevertheless, methods of obtaining arterial blood from the cow compatible with minimal disturbance of the animal were slow to develop and it had therefore been assumed that if blood were taken from a vein which did not drain the mammary gland, *e.g.* the jugular vein, it could be regarded, for purposes of assessing the extent of mammary demand on the blood constituents, as equivalent to arterial blood.

Our modern knowledge of the precursors in blood of the milk constituents may be regarded as dating from the demonstration by Blackwood and Stirling in 1932 that jugular venous blood *cannot* be taken as representing the blood reaching the mammary gland. They showed very clearly that it was necessary to take true arterial blood, and in their subsequent work arterial blood was obtained from lactating cows by puncture of the radial artery (in the leg) and venous blood from the mammary vein. Lintzel, working about the same time with goats, obtained his arterial samples by heart puncture.

Though valuable results have been obtained by these workers, the technique of radial or heart puncture is somewhat hazardous, and virtually impossible if successive samples of arterial blood at fairly short and definite intervals are needed. The next advance was made by Graham, Kay and McIntosh (1936) who obtained arterial blood samples from the lactating cow by puncture of the iliac artery through the rectal wall. By this method large samples of blood from both artery and mammary vein could be obtained with great rapidity and with only the most minor disturbance to the cow. It was found essential to take the venous sample first, followed at once by the arterial sample. Maynard and his collaborators (1937) used a somewhat similar method : they obtained arterial blood from the cow by entering the internal pudic artery through the vaginal wall.

A method which requires a preliminary operation on the experimental animal, but which avoids certain difficulties of the rectal or vaginal techniques just mentioned, has recently been devised by Graham, Turner and Gomez (1937). The position of the carotid artery in the goat is surgically altered to bring it into a loop of skin on the surface of the neck. After healing, which takes a few weeks, samples of arterial blood may readily be obtained from the exteriorised carotid at short or long intervals with a minimum of disturbance.

This digression on technique is justified in that it has been clearly demonstrated that any disturbances of the lactating animal that are more than minor, temporarily check, or at least interfere with, milk secretion. Since the differences, even at the height of secretion,

between the chemical composition of arterial and of mammary venous blood are small, and in some instances are not much greater than the analytical errors of the method of determination, an experiment may be almost without value unless the best possible blood-sampling conditions are strictly observed.

Uptake of Sugar by the Mammary Gland.—The changes in blood glucose as between arterial and mammary venous blood in the lactating cow are large, averaging, in some experiments at Shinfield, about 20 per cent. of the arterial blood sugar, *i.e.* quite large quantities of sugar are taken up by the active gland. It may safely be assumed that this sugar is used for one or both of two purposes, (*a*) for the energy requirements of the gland, (*b*) for conversion into lactose or other milk constituent. In this connection many experiments have shown that the diminution of the blood sugar level by insulin, by phloridzin, or by starvation is accompanied by a fall both in the total quantity of milk secreted and the quantity and percentage of lactose in the milk. What then is the effect of a rise in blood sugar level ?

The ruminant is different from most mammals hitherto investigated in that its blood sugar level is normally extremely stable. Several attempts have been made without much success by the Shinfield workers to cause hyperglycæmia in the lactating cow by feeding, and by glucose injections. The recent attempts of Brown and his collaborators (1936) to raise the cow's blood sugar level and observe the effect of such hyperglycæmia on lactose secretion can only be described as gallant, but although these investigators confirmed that diminished blood sugar was followed by diminished lactose production, for the reverse conclusion the evidence they obtained was unconvincing, owing to the extreme experimental difficulties involved.

The probability of the dependence of lactose formation on the glucose brought to the gland by the blood stream was heightened by the work of Grant, who found, using slices of surviving mammary gland, that glucose was the only material out of four hexoses and of several hexosephosphoric esters used, which could be converted into lactose by this tissue.

Further, a certain degree of correlation was observed by the Shinfield workers between the arterial-venous difference in blood sugar level and the amount of milk secreted, and also between the level of sugar in arterial blood in different cows and the amount of milk secreted. Thyroxine, too, which increased the arterial blood sugar level, also increased both the total amount of milk secreted in a given time and the concentration of lactose in that milk.

From the foregoing work, and other pertinent findings which are not cited here, two tentative conclusions were drawn at this stage, (i) that the level of glucose in the blood has directly or indirectly a controlling influence on milk secretion, (ii) that the lactose of the milk is probably derived entirely from the glucose of the circulating blood. An important corollary is that since, as will be described shortly, a large proportion of the total blood volume passes through the mammary gland every minute, the maintenance in the actively lactating animal of a steady level of arterial blood sugar must, particularly in the bovine (where owing to the tremendous micro-biological fermentation in the stomachs, carbohydrate is probably not taken up from the gut in any more complicated a form than that of lactic acid), make large demands on the liver or other organs responsible for this maintenance.

Some of the Shinfield work in 1935 seemed to indicate that the amount of glucose taken up by the gland of an actively lactating cow would be barely sufficient to cover both the metabolic glucose and the glucose required for lactose synthesis. If indeed glucose was the sole material used to meet both these requirements, then a very high figure for the circulation rate of the blood through the udder would be called for, a rate much higher than that calculated from other findings. It was therefore not entirely surprising when Graham in 1937 was able to show that in fact nearly twice as much lactose was secreted in the milk as could be accounted for by the removal of glucose from the blood. He used goats with exteriorised carotids, with only one mammary gland, and only one venous channel —the mammary vein—for drainage of blood from the udder, the external pudic and perineal veins having been tied and cut. The rate of blood flow was determined by a modification of the thermo-stromuhr technique. He further found that not only glucose, but also lactic acid, which as is well known can be readily converted into carbohydrate in the body, was freely removed from the blood by the lactating (though not by the non-lactating) udder, the weight of lactic acid so removed from unit volume of blood being over four-fifths of that of the glucose removed. Some 85 per cent. of the lactose secreted could be accounted for from the glucose plus the lactic acid which disappeared from the blood. The remaining 15 per cent. might well be either accounted for by experimental error in the blood flow measurements owing to peripheral venous drainage which could not be entirely prevented, and which, since it was not measured by the stromuhr, gave a slightly too small rate of blood flow through the udder, or it might be provided by the conversion into glucose of a portion of certain of the amino acids broken

up in the gland cells. Graham, Houchin and Turner have, indeed, shown that urea is readily formed in the lactating udder of the goat, the mammary venous blood containing on the average 3·2 per cent., or under favourable conditions as much as 4·5 per cent., more urea than the arterial blood. The remote contingency that this increase in urea arises merely by the abstraction of water for milk formation from the circulating blood is ruled out since it has been shown by hæmoglobin determinations that any blood concentration in the udder is less than 1 per cent. In view of the rapid circulation of blood through the udder, this extent of urea formation indicates that deamination is proceeding actively in this organ. Presumably, therefore, the other fraction of the deaminised amino acid molecule would also be available in not inconsiderable quantities in the mammary tissues, to be transformed wholly or in part into carbohydrate (or some intermediate substance such as lactic acid) and thus into lactose. The ability of the mammary gland to form urea from amino acids, if confirmed, will fit in well with the observations that high protein diets have a stimulating effect on milk yield.

What may prove to be a complicating factor has very recently (1938) been described by Petersen and Shaw. They accept the finding that glucose and lactic acid are normally used by the mammary gland for the synthesising of lactose, but they also find that as much as 0·2 per cent. of the wet weight of the mammary gland may be glycogen. The glycogen is probably built up from blood carbohydrate when opportunity offers, and used as a temporary source of sugar for lactose formation when for any reason the blood glucose or glucose + lactic acid is low.

Blood Precursor of Milk Fat.—Of the other milk constituents, milk fat or butter fat is one of the most characteristic. It is unique amongst naturally occurring fats in its high content of the lower fatty acids, and any complete account of the methods of its formation in the mammary gland from the raw materials provided in the circulating blood must explain this peculiarity of composition.

Possible origins of the milk fat may be arranged in the following *a priori* order of probability. Milk fat may be formed (i) by transformation of blood fat, *i.e.* the circulating triglycerides; (ii) by re-synthesis from blood phospholipin, *e.g.* from lecithin; (iii) from the blood sugar (providing the glycerol), and cholesterol esters (providing the fatty acids), with the liberation of free cholesterol in the venous blood; (iv) from some other source, *e.g.* entirely from the blood sugar, by extensive chemical transformation.

The consensus of recent findings, with careful sampling methods

and improved analytical technique, is in favour of the first, and simplest, of these alternatives.

It was shown by Graham, Jones and Kay (1936), at Shinfield, that blood going through the udder lost none of its phospholipin, so that phospholipin cannot be the blood precursor of milk fat. They also found a definite, though small, fall in the "total fatty acid" concentration of blood in going through the gland, *i.e.* there was a fall either in the true triglyceride, or in the fatty acid combined with cholesterol, or in both. The average fall in blood fatty acids was only about 2 per cent. (or in the best series of experiments 3·5 per cent.) of the total fatty acids.

Lintzel, in Germany, found that in the goat the cholesterol esters do not provide the fatty acids for the secretion of milk fat, but that these acids come entirely from blood triglycerides. In more recent work, Maynard and collaborators (1937) at Cornell agree that there is a drop in total fatty acids as the blood traverses the udder of the lactating cow, and show also that this drop is neither in the phospholipins nor in the cholesterol esters, *i.e.* it must be due to a fall in the neutral fat, which is the only other fatty-acid-containing constituent of blood that occurs in any quantity. The *direct* determination of blood neutral fat is a matter of considerable analytical difficulty and, as yet, has not been made. Neutral fat is present in blood, however, in quantities which are smaller than the amount of fatty acid combined as phospholipin or as cholesterol ester. It is believed that the neutral fat of blood does not contain appreciable quantities of the lower fatty acids, although about one-third of the fatty acids in cows' milk fat are lower members of the series. Does the mammary gland form these by breaking down the higher fatty acids, or by synthesis from glucose? Judging by the difficulties of meeting the demands, mentioned in a previous paragraph, which are already made on the blood glucose by the mammary gland for lactose synthesis and for metabolic purposes, it seems unlikely that the blood glucose also provides the lower fatty acids of the milk fat.

Caseinogen and Other Phosphorus Compounds in Milk.—The synthesis of caseinogen, the characteristic protein of milk, has received some attention lately, but like the other two, the mechanism of its production is still somewhat uncertain. Linderstrøm-Lang some years ago was able to separate caseinogen into two or more fractions with different chemical and physical properties. This aspect will not be discussed here; for present purposes the name caseinogen will be taken to cover the protein complex. Caseinogen may be produced from one of the blood proteins, say the serum

globulin, by a relatively small transformation, or it may be built up from blood amino acids which Blackwood and Stirling (1932) have shown to be significantly lower in concentration in mammary venous than in arterial blood. Whichever is found to be the case, the fact that caseinogen contains a large proportion of phosphorus in the molecule means that this, at least, of its constituent parts, must come from non-protein sources.

The blood phosphorus compounds comprise phospholipin, phosphoric esters of several types and inorganic phosphate. (The proteins of blood have relatively low phosphorus content.) Milk, in addition to caseinogen, contains phosphoric esters and inorganic phosphate: the phosphorus being distributed in the proportion, approximately, of 25 per cent. casein P, 10 per cent. phosphoric ester P, 65 per cent. inorganic P.

The most careful analysis of arterial and mammary venous blood in the cow has revealed that the only phosphorus fraction that diminishes whilst the blood traverses the mammary gland is the inorganic phosphate fraction. Three laboratories, one in England, one in Scotland and one in Germany, are agreed that the phosphorus compounds of milk, organic and inorganic, are derived in the main from the inorganic phosphate of the blood plasma. It has also been observed that the rate of uptake of inorganic phosphate from the circulating blood by the udder is not constant during secretion, but depends on the time that has elapsed since the last milking, diminishing as the udder fills up with milk.

Very recently (1938) these findings have been confirmed in a most elegant manner by Aten and Hevesy, using radioactive phosphorus. When "labelled" sodium phosphate is administered subcutaneously to goats, the radioactive phosphate is slowly but steadily taken up from the blood plasma by the gland, and the caseinogen synthesised during the first few hours contains a comparatively high proportion of radioactive P. The milk esters containing radioactive P are not quite so rapidly synthesised, but here again, judging by their content of "labelled" P, their phosphoric acid moiety appears to come largely, if not exclusively, from the *inorganic* phosphate of the blood. The phospholipin phosphorus of the milk seems, by this method of assessment, to be clearly derived not from the lipin compounds in the blood, but again from the free inorganic phosphate.

An interesting sidelight is thrown by this experimental work on the rate at which blood must circulate through the mammary gland in the lactating dairy cow, an animal, it must be remembered, very highly specialised for milk production. Assuming that all the pre-

cursor disappearing is synthesised into the milk constituent without loss, if a mgm. of the blood precursors of a milk constituent disappear from unit volume of blood in passing through the mammary gland, and b mgm. of the product are present in unit volume of the milk produced, then the amount of blood required to circulate through the gland to produce unit volume of milk must be approximately $\frac{b}{a}$. It can be calculated in this way that some 300–400 litres of blood circulate through the udder for each litre of milk secreted. This indicates, with a little further calculation, that the udder of an average cow in full lactation takes about one quarter of the heart's output of arterial blood. It is not to be wondered at that the " milk veins " (abdominal subcutaneous veins along which the main venous drainage of the udder is effected) in a good cow are characteristically large, and in fact the size of the " milk well " (the aperture in the body wall through which this vein goes) is not infrequently used in the cattle market as a guide to milk-producing ability or potentiality.

From the findings described in this short article, it will be apparent that our ideas as to the method of control of milk secretion by the endocrine glands, and as to the relationship between blood constituents and the characteristic milk components, are beginning to take shape. These two inter-related features have been selected somewhat arbitrarily by the writer from a number of fields in which dairy science has been making advances of late years because they seem to him to be of fundamental importance for the whole of dairy science and because he himself has been directly interested in the experimental work concerned.

In Great Britain, lack of funds is handicapping progress at present. If men and money can be found to continue these investigations, and keep dairy science a step or two ahead of dairy practice, many valuable lines of guidance will be forthcoming, and serious pitfalls, almost inevitable when animal or plant physiology is being strained to the limit by artificial selection, may be avoided.

THE CHEMICAL PHYSICS OF PAINTINGS

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A characteristic feature of our time is the specialisation of knowledge; the tendency to know more and more about less and less. Probably, such a course is unavoidable, for man's ability to become aware of new facts is enormous, yet at the same time his powers of assessment and appraisement—in short, his critical faculties—are more limited. And thus it comes about that revolt sets in, revolt that is against being told what is or what is not a fit subject for study. One has only to recollect Maurice Hewlett's "Youth at the Summit" in his "Pan and the Young Shepherd," with its scene at the mountain edge and the "world stretcht out," the wind at gale force and all the rest, to exclaim with him "World, world, I am coming." Frontiers may be disastrous things; confines of knowledge are little less dangerous in the long run. Thus, our urge to create simply ignores such barbed-wire entanglements as divide "Art studies" from "Science studies," or hinder us from wandering at will in the no-man's land that lies between subjective and objective enquiries.

America has performed a fine service for the readers of scientific literature by producing *The Journal of Chemical Physics*. The title is most significant: it means, if I am not mistaken, that the aim is to discuss physics in their chemical aspect rather than chemistry in its physical aspect, which is what many of us have long —perhaps over-long—been accustomed to do. At this stage then is perhaps the moment to explain that I propose to deal with the physics of paintings from a slightly chemical point of view, closely, that is, as the basis of natural science is partitioned in that publication.

The sequel will show that the so-called "easel paintings" come in for the major share of attention. The reasons for this are partly because it so happens that my work is amongst them, and partly because their mobility (a relative term I know, to my sorrow) makes them a little more amenable than—say—mural decorations, to

scientific investigation. We shall have occasion to notice the limitations of such methods more than once ; indeed, nobody would wish to deny the paramount importance of sensibility for those in and around a collection of pictures. At the same time, however, "knowledge goes ahead as technique develops," as the late Lord Rutherford never tired of reminding us. And technique connotes a habit of mind, a "methodik" as well as a mastery over materials.

Our first task then is to take stock of the objects with which we have to deal. This will prevent our "methodik" from becoming so abstract as to have no direct application. Our second duty is to set up such intellectual machinery as will enable us to cope with the physical problems they present, with a view (and this is the crux) not only to conserving and preserving them, but to knowing more about their *raison d'être* than could be achieved by subjective contemplation alone. Finally, in our quest for a due recognition of materials, we shall need to sort out such laboratory processes as help in this work, from those which apply perhaps to exactly the same substances when they do not form part of a picture. Otherwise we shall never see the wood for the trees.

The advantage of looking at our subject from such a dynamic aspect is, I believe, that it will prevent this article from becoming a mere description of how certain things, probably most desirable in themselves, can be done. The lead which others have given in the technics of Art make it presumptuous for me to enlarge upon this theme. It may be better therefore to confine this paper to aims and ambitions, with a brief review of such experimentation as this country has produced.

Pictures as Physical Objects

A picture, on a wall, on wood, or on canvas, is an object, the chief feature of which, physically, is its stratified nature. For the moment, its chemical properties are subordinate, and, clearly, they are exceedingly complex. Faced with the proposition of thinking out ways and means of dealing with such an object, the scientist will instinctively turn to his experience of other stratified systems, such as rocks, certain classes of crystals, film-structures, and so forth. He will then ask himself whether the well-tried methods available for examining strata in general are applicable to the present case. This question prompts him to caution. The successive layers of a painting are not separate and simple entities : infinite difficulty would be removed if they were. The sequence, support, ground, paint-film, surface-film are functionally related, and failure to allow for this is to court disaster at the start. Our investigator,

however, without turning the blind eye to all this, will be well advised to persist in his mental image of "layer upon layer," and begin at the beginning. We all remember the sound advice to youthful examinees, that it is of the greatest importance to write down something, even if wrong, in order to overcome the mental inertia of gazing at a vacant sheet of paper for three hours. Much the same applies to our case at the start. The need then is for fundamental experiments, experiments, that is, that put first things first. By no other means shall we prevent our subject from becoming a mass of data having no basis of generalisation, but only a collection of special cases. But to return to our object, the stratified thing called a picture. Can anything be done to classify the layers more precisely? Probably not, but there is one step which we can take now, or better, there are two alternatives between which we can choose at once. This concerns the paint-film, in a sense the most important, for, after all, it is the "picture." Either we can distinguish between different kinds of paint-films by their structure, or by the vehicle or medium. The first way has lately been brought forward most convincingly by Dr. Stout, and the second is really the gist of Professor Ostwald's "Iconoscopic studies." Otherwise expressed, the former is physical (with a due regard to chemistry), the latter is chemical, with no regard to physics.

The orientation of this contribution being what it is, it is scarcely necessary to state that the former provides a degree of satisfaction totally lacking in the latter. The strength of the structural concept of paint lies in its ability to deal with observables (literally to "face the facts") whereas Ostwald's microchemical criteria, refined as they are, destroy in the testing just that which we wish to preserve. Given then a set of structures, the chemical-physicist can no longer resist the appeal to investigate them optically. And indeed this is what a number of workers have been constrained to do for several years past, without realising the power which the structural concept imparts to their efforts. The microscope of varying degrees of refinement and magnification is probably the most effective instrument in a picture laboratory. To estimate the extent of dispersion of pigment, to learn the nature of the mechanical "lock" between paint-film and ground, to detect discontinuities and to infer from them something about a picture's condition, all these are within the scope of what may broadly be called microscopy.

A host of other possibilities obtrude themselves, once one begins to think of a paint-film functionally; they will come in for their share of attention later on; our business at the moment is with the picture itself, interesting always, and perhaps sublime.

We recollect, for example, that paintings need all the help we can give them in their struggle for continued existence in the best state obtainable. Further, that we are responsible for solving, as far as we are able, the problem of exhibiting them to the best advantage, at the same time paying heed to their health. These matters concern gallery directors in the main, but it is but natural that scientists should be called upon to assist, and their part must be played with due understanding of a curator's viewpoint.

The passage of time leaves its mark upon the bulk of a painting. This means that heat and cold, dry conditions and damp conditions, radiation effects like sunlight, artificial illumination and so on contribute to the condition of a picture at any moment of its life and make its instantaneous state largely a function of its previous states.

In addition, there is the menace of a polluted and gas-laden atmosphere, characteristic of many great cities, and it is said that altarpieces in Italian churches have suffered from the chlorine liberated by the burning of bleached candles. If a picture is where it is for profit or for pleasure, or for both, all these drawbacks must most likely be endured, but their evil effects can be controlled to a large extent. A close study of radiation between wall and picture (often at distinctly different temperatures) will go far to reduce the settlement of condensation upon surfaces ; the use of a "chemical balance " such as the appropriate hydrates of zinc sulphate will keep the fluctuations of relative humidity within reasonable bounds ; air-filtration (with proper refrigeration) is becoming more and more desirable in the dusty and fog-laden cities of Great Britain and elsewhere. And alas ! precautions against aerial warfare take a forefront place in the minds of those responsible for " documents " (for pictures *are* documents) which in reality belong not to any one nation, but to a world disquieted and distressed.

All these questions are capable of more or less of a scientific answer, but between asking them and making reply stand problems of administration and perhaps of finance. It is scarcely fair to evade these, and so a few short moments will be devoted to an outline of our experience in London. In the main, no gallery or museum can hope to tackle all the chemico-physical queries which confront it. And indeed, it is absurd to expect that it should. Co-operation with institutions staffed by experts in various branches of applied physics is the obvious solution, and this, we have found, is perfectly easy. Nevertheless, a fair amount of clear thinking is needed, and for these reasons. Firstly, it is probably impossible to allow works of art to leave the gallery where they are, and secondly

it is evident that no risks whatever can be taken with them in their own habitat or elsewhere. This means that research must be conducted upon "models" (and "models" for such purposes may be very quaint things, bearing next to no resemblance to their prototypes in appearance), and also that due allowance must be made for the fact that what it may be permissible to do to such a model is totally inadmissible for the "real thing." However, given a reasonable degree of imagination in designing experiments and, be it said, in the interpretation of results, much good may come.

Here perhaps is the place to remark that art is by no means the only human activity which relies for fundamental progress upon intuition; mathematics and pure science have leapt forward by flashes of intuition, and the logical defects of æsthetics are paralleled by the epistemological gaps which have, for a time, pervaded some of the greatest advances in natural philosophy. With the utmost diffidence, I would urge the art-critic to a study of pure mathematics if he would revel in sheer beauty, with the same conviction that I would invite my brother scientists to spend many hours in a disciplined tour of the great art collections of the world.

To return however to co-operation. The scientist whose duty it is to have charge of a gallery or museum laboratory should spend a considerable portion of his time in working out co-partnership experiments such as those already indicated. He it is who should be able to appreciate the needs of his institution in relation to tests which in the nature of things it cannot perform for itself.

The Significance of Art Technics

An attempt has been made in the last section to conceive the picture in bulk as an object for scientific investigation. We now approach the core of the whole matter in asking ourselves why such examination is worth while, and if we can give a satisfactory answer to that, our next occupation must be to devise exact means of laboratory experimentation.

It will have become clear already that physical methods are likely to help the gallery curator in the discharge of his normal duties. The ultimate aim, however, in my view, is to find out more—much more—about the technique of the great masters than we know already, and to assist in bringing pictures back as nearly as may be to their original state. It is true, no doubt, that the problem of restoration is largely æsthetic, and other factors which do not concern us here will probably influence a director's decision to restore, or not to restore, a certain picture. Nevertheless, if it

be granted that it may be a worthy aim to present a work of art once again in a state closely approximating to that which the artist originally intended, then all the resources of a refined scientific "methodik" will be necessary to achieve it, both in pre-examination of the work in question, and at all stages of the restoration.

It may be illuminating to work through an example in as much detail as mere words can provide.

Here, for instance, is a picture, say of an Italian School. It is, in its present guise, probably next to worthless as a work of art, or as a document for its supposed author. It may even be covered with a rich brown varnish, but in spite of that, it will be obvious to the eye of any reasonably observant person that the paint-film is very far from original. Probably it is Mid-Victorian. On the assumption that we hesitate to exhibit this object as a genuine old master, the first step would seem to be to ask whether in fact there is any of the original left, and if so, how much.

The scientist will immediately subject the picture to X-ray analysis, and with certain reservations, he will most likely be able to reveal what is underneath the confident super-structure of the nineteenth century. These reservations we shall discuss later on; the immediate point is that physical means will almost certainly provide a definite answer to the question as to whether or not there is anything there to restore, or at least to bring to light. Anything may happen; the picture may be found to be perfectly valid, or a considerable amount of "original" may appear, or, next to nothing whatever, a mere wreck of scrapings and patches.

Next, analyses by ultra-violet and infra-red radiations will be invoked. The former is often valuable for its merciless revelation of retouches, whereas the latter penetrates the discoloured varnish (and dirt) and thus gives the restorer a good deal of information of what the paint-film is like before he tries to remove the varnish layer. Naturally, it is not the affair of the scientist to pronounce the final verdict "to clean, or not to clean," but it is scarcely open to argument that the data in the form of prognosis which he will be able to provide will go far to influence the decision.

Once the word has been given to proceed, it behoves both curator and scientist to assemble a dossier of records throughout the process. The reason is partly to safeguard themselves against possible criticism and partly as data for future efforts. Detail photographs are the most obvious aid; they need experience and skill in the taking, to bring out the particular feature which it is desired to stress. As a rule, a judicious use of filters will lead to the required results. Another stage-by-stage process is colorimetry:

with the help of a tintometer or similar instrument readings can be collected at any desired place on the picture.

In this section I have purposely accorded prior consideration to a hypothetical case of cleaning; the more profound subject of the objective study of techniques will be taken up now. This work is obviously helped by experience in a scientific approach to restoration, which accounts for the order herein adopted.

For the purpose of building up a corpus of knowledge about technique, access is naturally required to the best authenticated examples of the great masters; cases in which there is not a shadow of doubt of the picture's origin. Then X-ray analysis, together with low-magnification microscopy, plus the use of the microsectioner will play their part in producing evidence of "how they did it." Investigations in this field are in their infancy as yet, and it is obvious that many difficulties both of a scientific and general nature will have to be surmounted before one can recognise such techniques objectively. But a beginning has been made, and the X-ray shadowgraphs obtained in Vienna, with perhaps those from one or two of the best Raphaels at the National Gallery, encourage one to believe that the quest is not hopeless.[1]

Special Laboratory Methods

In such a review as this, covering a general scheme of chemico-physical tests and researches, one endeavours to present the subject in tolerably logical order. In other words, an attempt is made to discuss what is to be done before explanations are offered of how one tries to do it. Historically, as in other branches of activity, this has not always been so; an apparatus has been assembled for some seemingly good purpose, and it has proved itself in the event the ideal way of doing something quite different.

The remainder of this article therefore is devoted to principles, principles that is which underlie all physical examinations of paintings. The whole subject is too encumbered with individual instances as yet to be able to give explicit instructions, and even if such was feasible, it would be altogether rash to assert that the ways and means which we have developed in London are in any respect sacrosanct. The most that can be claimed for them is that they are established upon rigid physical concepts, and that the apparatus has been designed or modified expressly for the end in view.

[1] The radiography of technique is largely directed towards a fuller knowledge of the "whites" than would otherwise be obtainable. By observation of relative density it is possible to infer whether the "whites" arise from the ground, the paint-film, or from a mixed method.

The optical equipment may be discussed first, in view of its fundamental importance. Direct observation with a good hand-lens always plays a great part in preliminaries; one detail at this point is that such a hand-lens can be designed with a specially long working-distance so that it can readily be focussed through the glass of a picture, without the trouble of deframing it before anything can be done. A further refinement still is a long-working-distance binocular. This is a little too bulky to carry everywhere and anywhere however. A magnification of some six times is convenient. For laboratory purposes a binocular fitting on a travelling rackwork or arm, with a low voltage lamp such as the Leitz "Monla," is excellent. There is next to no danger from overheating at any reasonably close range. The study of the structure of paint, such as Dr. Stout has recently encouraged, is readily carried out at a magnification of about ten times or less, though it will be necessary to be prepared for enlargements of some 100–200 diameters for the examination of cores from the microsectioner, or for microchemical reactions involving the precipitation of small characteristic crystals. Incidentally, the recent manual by Dr. Wilson on Microchemical Methods is most valuable. Such tests do not compare with structural determinations so far as the paint film itself is concerned; their worth is in the identification of pigments.

Reference to ultra-violet analysis may be brief, not because it is unimportant—far from it—but because its usefulness depends upon factors which may not always be present, or, in medical language, it has a number of "contra-indications." The fluorescence of most varnishes is a specific property, exceedingly fortunate when one wishes to know the state of the varnish layer, or whether it has been painted over. Ultra-violet photographs should always form part of the dossier of material collected during a restoration. There is no difficulty now with the technique; ordinary camera equipment, together with a special filter and a source of suitable radiation is all that is necessary.

Infra-red registration is in a somewhat different category. Its potential value is probably greater, but its troubles are too. The property of immediate importance is the penetration. Varnish, however much discoloured, is nearly transparent to these rays, and surface dirt offers remarkably little resistance. On these scores alone, the gallery scientist will wish to experiment with them. He will be forced to depend wholly upon photography; there is no method known of making their effects directly visible to the eye. Further, he will be well advised to ignore the sweet counsel of the optimists who aver that infra-red photography is a simple extension

of the usual art, requiring nothing beyond a tungsten bulb and a deep-red filter. Few sadder hours can be imagined than to trudge this pointless track.

To obtain success is not difficult, but one simply must "begin at the beginning."

Firstly, a camera of all-metal construction, for wood, leather and so forth are not "infra-red tight" and fogging of plates is sure to follow their use. Such a camera has recently been described in detail,[1] and to that article those specially interested may be referred. Secondly, a lens specially computed for this portion of the spectrum. An optical combination has been worked out by the firm of Ross (and I believe elsewhere as well) the special feature of which is that the dispersion curve is horizontal for long wave-lengths, which means that once an image is obtained and focussed for the visible spectrum, it is automatically in focus for the infra-red. Given all the optical data of a lens system it is only a laborious computation to arrive at equivalent focus for any set of components, but if infra-red photography is to be of immediate service in the picture laboratory, a parfocal lens such as that described is essential. It is also of consequence to notice that visual focussing upon a picture surface is a little precarious, for there are an infinite number of positions if the surface is appreciably rough, and the deep penetration of long wave-length radiation should be borne in mind. Thirdly, although there is no need whatever to take the heroic—and uncomfortable—measures sometimes advocated for hyper-sensitising infra-red plates, it is as well to maintain the speed reasonably constant by keeping them in an ordinary domestic refrigerator. They should be left at normal room temperature for about half an hour before use, otherwise disaster will ensue on account of condensation. To some extent, these precautions are local, and depend upon average temperature, degree of humidity and so on ; infra-red photographic material is inclined to be "temperamental" at times, but to respond to "coaxing." The Ilford deep infra-red filters are adequate, but of course a dummy (white) filter should be used as a compensator when focussing is being done in the visible. Almost any high-temperature electric bulb will give some result, but "Neron" lamps in suitable metallic reflectors are excellent. Exposures seldom amount to more than a matter of seconds. A special dark room filter enables operations there to go on with ease.

The last main subject for review is X-ray equipment. Before all else, the special character of the work to be done points to the use of soft radiation.

[1] F. I. G. Rawlins, *Museums Journal*, July, 1938, p. 186.

Our own experience in London has demonstrated that there is no need for the Lindemann window, nor for the "Grenze" tube, though both these appliances are useful. A tube has been found which gives a steady supply of X-rays at 10–12 kilovolts with a current of about 15 milliamps. With the aid of intensifying screens, exposures are conveniently short. But the successful progress of X-ray photography of paintings has been seriously hindered by lack of appreciation of the elementary fact that almost any result can be obtained by varying the physical factors, such as kilo-voltage and current. Therefore it is essential to state these conditions whenever such photographs are being considered. Much fruitless controversy has arisen through neglect of this matter. In general, due to the permeability for X-rays of most components of a painting, with the exception of lead and mercury pigments, all detail will be lost if radiation much above 12 kilovolts is used. The employment of carbon compounds for pigments in modern times is the reason why, in many cases, it is possible by means of X-rays to distinguish original paint from later additions.

Chemical physics in the service of art is a new venture; it has yet to learn its weakness and its inherent limitations, but it is too young, too near "the mountain edge with the world stretcht out" to know even the sound of the term "moribund." Again, those who should be in a position to judge speak of a resting time, or of a period of comparative leanness in the natural sciences. Is it therefore fantastic to hope that some of those who have undergone the discipline of a scientific training may feel inclined to join in these efforts, immature and elementary though they may be?

Perhaps the art historian will find much to interest him in following the chronological development of man's mastery over recalcitrant materials, and so discover some common measure of concern with technique. But in any event, the call is to those who are prepared to spend year after year, with very little to show for their labours, in observation, thought and experiments.

Note on Plates I and II

According to Stout's concept of Paint structures there are three main classes:

(*a*) granular;
(*b*) vehicular;
(*c*) pellicular.

In (*a*) there is little or no binding medium, whereas in (*b*) there is considerable binding content. (*c*) contains the "glazes" and the extreme case of a pigment-free film. In addition, there are various sub-classes.

Fig. 1 is "lean-vehicular": Fig. 2 is "locked-granular": Fig. 3 is "vehicular," and Fig. 4 "paste-vehicular."

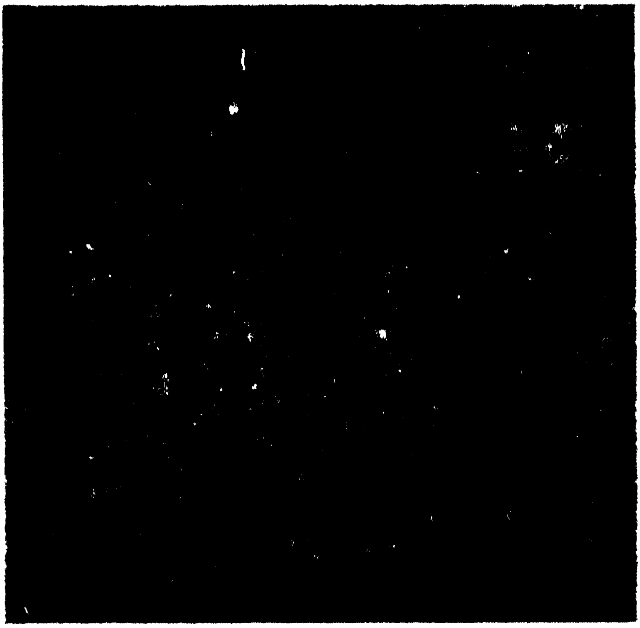


FIG. 1.—"Crackle" (or system of fine fissures), characteristic of fifteenth-century Italian tempera in perfect condition (× 9).

FIG. 2.—Structure of Italian fresco (c. 1450), indicating stone "support." Fragment from the Porta Romana, Siena (× 2).
(*By kind permission of the Trustees of the National Gallery.*)

FIG. 3.—Example of Rubens' technique (1635), built up in strata with local impasto (× 2).

FIG. 4.—Example of modern (late nineteenth-century) "direct technique" (× 2).
(By kind permission of the Trustees of the National Gallery.)

RECENT ADVANCES IN SCIENCE

MATHEMATICS. By J. H. C. WHITEHEAD, M.A., Balliol College, Oxford.

THE FOUNDATIONS OF DIFFERENTIAL GEOMETRY : I.—Towards the end of the last century mathematicians had agreed that there are many systems of geometry, such as the Euclidean, affine, projective, elliptic and hyperbolic geometries. I have explained in an earlier article how Klein, in his *Erlanger Programm*, threw both the similarities and the differences between these geometries into relief by describing a geometry as the study of properties which are invariant under the transformations of a given group. In its philosophical background this differs little from what we may suppose to have been Euclid's conception of geometry. Euclid's geometry is based on the notion of congruence, or "equality" between figures in space, and the test of equality, at least in plane geometry, is whether or no either of two given figures can be superimposed upon the other. This process of "bringing one figure up to another" is now described as a transformation consisting of a translation followed by a rotation, in other words by a transformation of the Euclidean group. In Klein's definition of geometry Euclidean space is replaced by an abstract set of points and the Euclidean group by an arbitrary group of transformations in a given space.

However, before Klein's definition appeared, Riemann had put forward a concept of geometry which differs essentially from Euclid's or Klein's. Riemann started with the differential geometry of surfaces developed by Gauss but, instead of taking a surface to be a locus in ordinary space, he thought of it as a 2-dimensional world, confining himself to the intrinsic geometry of the surface, which is defined as follows. Let a surface be given parametrically by equations of the form

$$x = x(u_1, u_2),\ y = y(u_1, u_2),\ z = z(u_1, u_2).$$

The parameters u_1, u_2, which we take to be real numbers, are generally subject to an inequality of the form $|u_i - a_i| < \delta$, where a_1, a_2 and δ are constants. This is a characteristic feature of

differential, or "infinitesimal" geometry, whose theorems generally deal with an arbitrarily small region containing a given point (a_1, a_2) (the geometry at the point (a_1, a_2)). The element of arc of a curve on the surface is given by

$$\begin{aligned} ds^2 &= dx^2 + dy^2 + dz^2 \\ &= g_{11}du_1^2 + 2g_{12}du_1du_2 + g_{22}du_2^2, \end{aligned}$$

where $$g_{ij} = \frac{\partial x}{\partial u_i}\frac{\partial x}{\partial u_j} + \frac{\partial y}{\partial u_i}\frac{\partial y}{\partial u_j} + \frac{\partial z}{\partial u_i}\frac{\partial z}{\partial u_j} \quad (i, j = 1, 2).$$

This expression for ds^2 as a quadratic differential form in (du_1, du_2) is called the first fundamental form of the surface. Its geometrical significance can best be seen by considering a curve on the surface, given by $u_i = u_i(t)$, the arc length of which, measured from the point t_0 to the point t_1, is seen to be

$$\int_{t_0}^{t_1} \sqrt{g_{11}\dot{u}_1^2 + 2g_{12}\dot{u}_1\dot{u}_2 + g_{22}\dot{u}_2^2}\, dt,$$

where $\dot{u}_i = du_i/dt$.

In the classical theory the differential geometry of a surface is based upon the first fundamental form and a second quadratic differential form which, with an error in the third order, gives twice the (second order) perpendicular distance from the point $(u_1 + du_1, u_2 + du_2)$ on the surface to the tangent plane at (u_1, u_2).

Now the parameters u_1, u_2 may be arbitrary, subject to certain conditions of differentiability on the transformations by which one passes to other parameters. Let v_1, v_2 be related to u_1, u_2 by equations of the form $v_i = v_i(u_1, u_2)$. Since

$$(1)\ ds^2 = g_{11}du_1^2 + 2g_{12}du_1du_2 + g_{22}du_2^2 = h_{11}dv_1^2 + 2h_{12}dv_1dv_2 + h_{22}dv_2^2,$$

h_{ij} being the coefficients of the first fundamental form in dv_1, dv_2, it follows that

$$(2)\ h_{pq} = \sum_{i,j=1}^{2} g_{ij}\frac{\partial u_i}{\partial v_p}\frac{\partial u_j}{\partial v_q}.$$

Conversely, the relations (2) imply (1). Similar relations to (2) hold between the coefficients of the second fundamental form expressed in terms of u_1, u_2 and of v_1, v_2. In consequence of the relations (2) the two quadratic forms which appear in (1) are said to be equivalent.

Transformations of the form $v_i = v_i(u_1, u_2)$ can arise in two ways. First, as in the last paragraph, u_1, u_2 and v_1, v_2 may be two sets of parameters for the same point. Secondly, u_1, u_2 may

be parameters on a surface S and v_1, v_2 on a surface S*. In this case the equations $v_i = v_i(u_1, u_2)$ define a transformation in which the point (u_1, u_2) corresponds to the point (v_1, v_2). If the two fundamental forms of S are equivalent to the fundamental forms of S* the two surfaces have the same differential geometry. In fact, it can be proved that S is transformable into S* by a Euclidean transformation. That is to say, they differ only in their position in space.

It would not be unreasonable to describe the abstract theory of these two fundamental forms as the intrinsic geometry of the surface. However, the term is generally understood to mean the theory of the first fundamental form only. The geometrical intuition behind this is that the " internal " metric structure of a surface S is not altered by a deformation in which S is not stretched, and if a surface S* is obtainable from S by such a deformation then the fundamental forms of S and S* are equivalent (actually the simplest mathematical definition of a deformation without " stretching " is one which leaves ds^2 unaltered. This is justified by the fact that the length of a given curve remains constant throughout the deformation). Thus the intrinsic differential geometry of a surface is the study of those properties of the quadratic form ds^2 which are unaltered by a transformation of the parameters u_1, u_2, in other words those properties which are the same for all equivalent forms.

I have written at some length on familiar matters in order to emphasise two concepts ; the relation of equivalence between two quadratic forms and the concept of a geometry as the study of those properties which are not altered by a transformation of variables. Examples of such properties are the length of a curve, the angle at which two curves intersect and the total curvature at any point. The latter, though usually defined in the first place as an invariant of the two fundamental forms, actually depends only on the metric. This is apparent from the following theorem which, if for no other reason, deserves a place of honour in the theory of surfaces. Let ABC be a geodesic triangle containing in its interior a point P. It should perhaps be explained that a geodesic is a curve, any " short " segment of which is the shortest curve joining its end points. The term " short " simply means shorter than some positive δ, and excludes, for example, the segment described by a voyage round the equator to within a mile of one's starting-point. A geodesic triangle is one whose sides are geodesics. The theorem is that, as A, B and C tend to P, then

$$(A + B + C - \pi)/\Delta$$

tends to the total curvature at P, where A, B and C are the angles at the vertices and Δ is the area of the triangle.

Having grasped the notion of intrinsic geometry Riemann did not confine himself to a 2-dimensional space, but introduced what is now known as n-dimensional Riemannian geometry. He started with an n-dimensional manifold, whose points can be represented by n real co-ordinates $x^1, \ldots, x^n$ (the indices are written above rather than below for reasons of formal elegance. There will be no confusion with exponents) which, as when $n = 2$, are generally subjected to an inequality of the form $|x^i - a^i| < \delta$. A $(1 - 1)$ correspondence between a sub-set of the points in the manifold and ordered sets of real numbers is called a co-ordinate system, and the co-ordinates may be changed by any transformation of the form

$$y^i = y^i(x^1, \ldots, x^n),$$

provided the functions $y^i(x^1, \ldots, x^n)$ satisfy certain differentiability conditions in the region under consideration. So far I have been describing not a "Riemannian space," but what may be called a "smooth manifold," the smoothness coming from the differentiability of the functions defining the transformations of co-ordinates. It is analogous to a description, in general terms, of a surface with a continuously turning tangent plane but without any particular metric structure. We now introduce a metric in the form of a quadratic differential invariant

$$ds^2 = \sum_{i=1}^{n} \sum_{j=1}^{n} g_{ij} dx^i dx^j \quad (g_{ij} = g_{ji}),$$

whose coefficients, g_{ij}, are functions of $x^1, \ldots, x^n$. We may omit the summation signs, on the understanding that a repeated index implies summation for values of that index running from 1 to n. Thus

$$\begin{aligned} ds^2 &= g_{ij} dx_i dx_j \\ &= g_{11}(dx^1)^2 + \ldots + g_{12} dx^1 dx^2 + g_{21} dx^2 dx^1 + \ldots \\ &= g_{11}(dx^1)^2 + \ldots + 2g_{12} dx^1 dx^2 + \ldots \end{aligned}$$

More precisely, we associate such a quadratic form with each co-ordinate system in such a way that

$$(3) \quad h_{pq} dy^p dy^q = g_{ij} dx^i dx^j,$$

where $h_{pq} dy^p dy^q$ is the form in co-ordinates $(y^1, \ldots, y^n)$, and the equality (3) is between values of $dx^1, \ldots, dx^n$ and $dy^1, \ldots, dy^n$ which are related by the equations

$$dy^p = \frac{\partial y^p}{\partial x^i} dx^i, \; dx^i = \frac{\partial x^i}{\partial y^p} dy^p.$$

From these relations and (3) we have, as when $n = 2$,

(4) $h_{pq} = g_{ij}\dfrac{\partial x^i}{\partial y^p}\dfrac{\partial x^j}{\partial y^q}$.

The relations (4) are a special case of what is called the transformation law of a tensor. Equivalence and invariance under transformations of co-ordinates are defined as when $n = 2$, and n-dimensional Riemannian geometry is the study of the properties of quadratic differential forms which are unaltered by transformations of co-ordinates.

As before, the word " property " may mean, for example, a function of the functions g_{ij} and their partial derivatives (*e.g.* the total curvature), a numerical function of two or more points (*e.g.* the distance between two points) or a set of loci. Among the latter the geodesics are the most important. If the quadratic form $g_{ij}dx^i dx^j$ is positive for all values of $dx^1, \ldots, dx^n$ other than $0, \ldots, 0$ the geodesics may be defined as above, the length of the segment of a curve $x^i = x^i(t)$ joining t_0 to t_1 being given by the integral

$$\int_{t_0}^{t_1} \sqrt{g_{ij}\dot{x}^i\dot{x}^j}\, dt.$$

In any case they are the curves which satisfy the differential equations

(5) $\dfrac{d^2x^i}{dt^2} + \Gamma^i_{jk}\dfrac{dx^j}{dt}\dfrac{dx^k}{dt} = 0$,

where the functions Γ^i_{jk} are given by

(6) $\Gamma^i_{jk} = \frac{1}{2}g^{ip}\left(\dfrac{\partial g_{jp}}{\partial x^k} + \dfrac{\partial g_{pk}}{\partial x^j} - \dfrac{\partial g_{jk}}{\partial x^p}\right)$,

in which $g^{ip} = \dfrac{1}{g}\partial g/\partial g_{pi}$, g being the determinant $|g_{ij}|$. Thus

$$g^{ip}g_{pj} = \delta^i_j\begin{cases} = 0 \text{ if } i \neq j \\ = 1 \text{ if } i = j. \end{cases}$$

If the functions h_{pq} are related to g_{ij} by the equations (4), and if Λ^p_{qr} are given by (6) with g replaced by h, it may be verified that

$$\Lambda^p_{qr} = \left(\Gamma^i_{jk}\frac{\partial x^j}{\partial y^q}\frac{\partial x^k}{\partial y^r} + \frac{\partial^2 x^i}{\partial y^q \partial y^r}\right)\frac{\partial y^p}{\partial x^i},$$

and hence that

$$\frac{d^2y^p}{dt^2} + \Lambda^p_{qr}\frac{dy^q}{dt}\frac{dy^r}{dt} = \frac{\partial y^p}{\partial x^i}\left(\frac{d^2x^i}{dt^2} + \Gamma^i_{jk}\frac{dx^j}{dt}\frac{dx^k}{dt}\right).$$

If, therefore, a curve $x^i = x^i(t)$ satisfies the equations (5), the same

curve, given in co-ordinates $y^1, \ldots, y^n$ by $y^i = y^i\{x^1(t), \ldots, x^n(t)\}$, satisfies the equations

$$\frac{d^2y^p}{dt^2} + A^p_{qr}\frac{dy^q}{dt}\frac{dy^r}{dt} = 0.$$

That is to say, the property of being a geodesic is invariant under transformations of co-ordinates. Notice that if there is a co-ordinate system $y^1, \ldots, y^n$ in which the functions h_{pq} are constants, which in general there is not, then the geodesics are given by the differential equations

$$\frac{d^2y^p}{dt^2} = 0,$$

or by the finite equations

$$y^p = a^p + l^p t,$$

where a^p and l^p are constants. In this case the space has the same infinitesimal geometry as Euclidean space and is described as "flat."

Riemann's formal apparatus was the basis of a chapter in mathematics which came to be known as the "absolute differential calculus," being so described by the Italian mathematicians Ricci and Levi-Civita who contributed more, perhaps, than anyone else to its development. But, with one remarkable exception,[1] no one seems to have taken Riemann's work very seriously as a contribution to geometry until Einstein's discovery of general relativity. According to the latter, physical space-time is to be regarded as a 4-dimensional Riemannian space, in which matter and gravitation appear as purely geometrical phenomena. This was followed by great activity in the mathematical development of Riemannian geometry and of other analogous kinds of geometry, some of which have been applied to relativity in various attempts to give a geometrical description, not only of the gravitational, but also of the electro-magnetic field. I hope in a second article to describe some of the post-relativity developments in differential geometry. I will conclude this first article with a few comments on a well-worn topic, the curvature of space.

Riemannian geometry naturally inherits much of its terminology from the theory of surfaces, which is largely concerned with the study of curvature as Newton understood the word. In particular

[1] W. K. Clifford (*Collected Papers*). Clifford saw the full importance of Riemann's contribution to geometry. He translated Riemann's memoir, "Über die Hypothesen, welche der Geometrie zu Grunde liegen" and, in what should perhaps be called a brilliant guess, anticipated the characteristic feature of general relativity by over half a century.

much in Riemannian geometry centres round what is called the "curvature tensor," the vanishing of which is necessary and sufficient for the space to be flat. From the mathematical point of view this terminology is good. It is vivid and points to useful analogies. Moreover, a curved surface is a useful model of a Riemannian space. But here the danger of confusion begins, since the curvature of a sphere, for example, is visualised as a relation between the sphere and a 3-dimensional Euclidean space containing it. Also a great circle which is a geodesic on the sphere is curved in the Euclidean space. Therefore, in order to understand the nature of Riemannian geometry in general, and of relativity in particular, one must thoroughly master the concept of a geometry as the intrinsic theory of a given tensor g_{ij}, rather than the study of a locus in Euclidean space. Again it is essential to realise that the geodesics are the "straight lines" of Riemannian geometry. The difference between Riemannian geometry in general and the special case of Euclidean geometry is not that the former deals with "curved geodesics," rather than with "straight lines," but that the relations of Euclidean geometry do not, in general, hold between the geodesics. As we have seen, the angles of a triangle do not necessarily add up to two right angles, and in this context the word "curvature" should not primarily be thought of as a reference to some kind of "bending," but as a measure of such discrepancies between Euclidean geometry and Riemannian geometry in general.

Good books on the subject are L. P. Eisenhart's *Riemannian Geometry*, T. Levi-Civita's *The Absolute Differential Calculus*, and E. Cartan's *La Géométrie des Espaces de Riemann*.

ASTRONOMY. By A. Hunter, Ph.D., F.R.A.S., Royal Observatory, Greenwich.

In the last issue of Science Progress the current state of the problem of time measurement by astronomical observations was reviewed. The present report deals with the astronomical side of time-keeping. The two are, of course, complementary aspects of the same subject—that of providing a public time-service.

The astronomical clock may be considered a means of interpolation between astronomical observations, just as the domestic clock is nowadays a means of interpolation between radio time-signals. Even in the early days of water-clocks, calibrated candles, sand-glasses, and the like, the time-interval measured by the particular means employed was no doubt always known approximately in terms of the solar day. It is when the other function of

the clock is invoked—that of extrapolation, *i.e.* time-forecasting—that trouble arises. As will appear presently, the efforts of the modern astronomical horologist are now directed towards improving the clock's performance in this second respect. Improvement will automatically follow in the first.

The perfect astronomical clock, once adjusted to give no zero-point error, will indicate correctly the time given by all subsequent observations, if these are themselves perfect. Such a clock will represent the period of rotation of the earth as the sum of the equal intervals between a number of regularly recurrent phenomena. For convenience of interpolation, this number should be large. In the case of the pendulum clock, it is traditionally 86,401. Modern methods, however, are tending to employ periodicities thousands of times greater than this, but the principle is the same. The rotation period of the earth may of course be referred to any astronomical body or set of bodies, but, as was seen in the last report, the only satisfactory bodies are the stars. Astronomical clocks therefore show sidereal time. The mean solar time in practical everyday use is obtained from this by suitable adjustment, this adjustment depending on the fact that the number of sidereal days in the year exceeds the corresponding number of solar days by one. Clocks showing true solar time—that shown by sundials—would be needlessly complicated in construction, and moreover useless when constructed.

The perfect sidereal clock remains, of course, only an ideal. The astronomer would be quite satisfied with one whose divergence from perfection can be stated with confidence at any moment. For this purpose the clock should have a predictable rate of change of error. In practice this condition means that the clock should have a constant rate. which, if the error is to remain small (though this is purely a matter of convenience), should itself be very small. The extrapolation process is then as easy as that of interpolation, and the time can be forecast with an accuracy depending only on the precision with which the rate can be determined.

In order to appreciate how far the astronomer's search for a clock of constant rate has progressed, we must enquire a little more deeply into the nature of the "regularly recurrent phenomena" referred to above. The accurate subdivision of the day may be said to date from the discovery by Galileo in 1581—traditionally from observations, timed by his pulse-beat, of the swing of a chandelier in Pisa Cathedral—of the isochronism of the simple pendulum. The clock constructed in 1657 by Huygens using this principle at once ousted the timepiece of the day, which employed the Foliot

inertia balance, and which could only be relied on to the nearest hour. A principle, however, is one thing; its practical realisation is another. There were two points on which progress was held up: the pendulum had to be kept swinging against friction, and it had to provide impulses for recording the number of swings. It is no exaggeration to say that despite the enormous strides in clock-making since those days, these two points are still the stumbling-blocks to future progress in pendulum clock design. The crux of the matter is that both requirements involve interference with the pendulum during its swing, with consequent deleterious results on its timekeeping.

Hooke's anchor escapement, which had displaced the old crown wheel and verge escapement by the end of the seventeenth century, performed both these offices simultaneously. It was unlocked by the pendulum at each swing, thus permitting the wheelwork to advance one step, and at the same time it transmitted to the pendulum sufficient energy from the driving weight to compensate for all sources of loss during the last swing, including the previous unlocking. The accuracy of which this escapement was capable exceeded the astronomical requirements of the day. In a modified form due to Graham, the dead-beat escapement, it is still first-class horological practice. Bolstered up by secondary improvements in the pendulum itself, such as arc reduction and control, temperature compensation, barometric compensation, etc., it kept pace with astronomical technique for nearly two centuries. But at the best, its rate cannot be guaranteed to much more than 0·2 sec. per day. By the beginning of the present century, the advances in observational technique described in these pages last October had made an accuracy of 2 parts in 10^6 insufficient. The astronomer was uneasily conscious that a week's bad weather might throw his predictions out by a second or more.

The first great advance was the general recognition of the principle that the maintaining impulse must be given at zero, *i.e.* at the rest position of the pendulum, if interference with the swing is to be minimised.[1] The Riefler clock realises this principle by impulsing its pendulum every second through the suspension spring at the moment when it is vertical. The pendulum rod is of invar, and the clock is mounted in an airtight case at a pressure of some 700 mm. mercury. The drive is by a falling weight of 10 gm., wound electrically every 40 seconds. With a stable mounting at

[1] This principle has, however, not remained unchallenged. Experimental and theoretical support for impulsing after zero has been advanced by Féry and Stoyko, *Comptes Rendus*, **194**, 689, 1932.

constant temperature, a precision Riefler clock of this type can maintain a rate constant to $\pm\cdot015$ sec. per day for 3 months. The major part of this tenfold increase of accuracy over previous astronomical timekeepers must be attributed to the virtues of the Riefler escapement. The major part of the residual instability of rate must be attributed to the system's vices. Prominent amongst these latter is the frequency with which energy is imparted to the pendulum. By sufficiently reducing the pressure in the clock-case, atmospheric friction can be removed; energy will then be lost mainly in overcoming the viscosity of the suspension and in turning the wheel-work which counts the swings. If only the pendulum could be relieved of this latter function, a few foot-pounds per annum would suffice to keep it swinging, and impulses and their attendant disadvantages could be reduced to a minimum.

The "free pendulum" resulted from this idea. A real pendulum cannot of course be truly free: it must be kept swinging, and to that extent the term is a misnomer. But the Shortt clock of 1921 realises the ideal in so far as it can be realised. This clock consists of two similar synchronised pendulums, the master pendulum which "keeps" the time, and a slave pendulum which records and distributes it. Attempts had been made before at putting all the work of the escapement on to an auxiliary clock; notably those of Rudd in 1898, of Gill at the Cape Observatory in 1904, and of Bartrum in 1913. The Shortt clock was, however, the first really successful design. The master pendulum, swinging in an evacuated case, is impulsed at zero every half-minute by the fall of a gravity arm, released by the slave, on top of a wheel it carries, 7 in. below the suspension. As the pendulum swings out, the arm falls clear at a definite phase, and sends a synchronising current to the slave. The slave is then running in phase and is ready to release the gravity lever for the master at the next half-minute. Meanwhile, it counts the vibrations, transmits time-signals, "rewinds" itself and the master, and in general relieves the master pendulum of all restraint.

The performance of the first Shortt clock installed at the Royal Observatory, Edinburgh, in 1921, caused something of a stir. Fluctuations in its daily rate were only of the order of $\cdot003$ sec., representing a five-fold gain over the Riefler clocks which had been astronomical standards for a quarter of a century. It will be remembered that by this time use of the impersonal micrometer on transit circles was standard observatory practice; a good night's observations could give a time determination whose probable error was $\pm\cdot01$ sec. Now the astronomer was confronted with a clock whose daily rate was constant to about one-third this amount. He

responded by reversing the normal procedure and plotting transit observations on either side of a line representing the extrapolated clock rate, instead of plotting clock errors on a graph representing the transit observations. This procedure is still followed at Greenwich, where the rate is now, however, that of a mean clock which represents the average of seven Shortt clocks. The Shortt clock, then, is so good that once it has been rated, a single observation of time from it is more likely to be accurate than that from a single night's work at the telescope. The initial rating, of course, has to be done with the aid of transit observations, which are by definition accurate in bulk ; and subsequently any systematic deviation of the observations from the rate line must be attributed to clock errors ; but the fact remains that the Shortt clock is good enough to show how the short-term periodicity due to nutation affects the transit observations. For the first time since the discovery, two centuries ago, of the non-uniformity of the precessional motion of the equinoxes, mean sidereal time has become more than a scientific abstraction ; apparent sidereal time no longer suffices for checking the performance of the modern precision clock.

We now have fifteen years' experience with Shortt clocks. In spite of their high performance, it must be admitted that they are by no means perfect. Residual irregularities exist, so small as to be masked in any other type of clock by escapement errors, but nevertheless big enough to become of importance when the pendulum is freed from the necessity of operating the escapement. Certain of these errors have been traced, and either eliminated or studied in detail with a view to applying corrections. For instance, slight temperature variations in the clock-room can be allowed for by invoking the known expansion coefficient of invar. So, too, an attempt can be made at predicting the amount of the secular growth to which invar is unfortunately subject.[1] Many of the variations of rate are associated with variations of amplitude. Each half-minute impulse alters the semi-amplitude by 6 seconds of arc, an amount which would, if maintained, alter the rate by several thousandths of a second of time per day. But over and above this, unknown causes may alter the semi-arc on occasion by 20 seconds. The performance can be improved by correcting for this type of variation so as to reduce to a constant amplitude. Photoelectric control of the arc (see Tomlinson, *Proc. Phys. Soc.*, **45**, 41, 1933) may in future render this tedious procedure unnecessary. It has already

[1] Such growth has been known to reach 7 microns per metre per annum. Since a micron's increase in length causes a simple seconds pendulum to lose over ·04 sec. per day, the effect is serious.

been shown that with a standard pendulum swinging in air at 2 cm. mercury, the acceleration caused by a small reduction of pressure is counterbalanced by the circular-error retardation resulting from the consequent increase of arc.

After all known causes of irregularity are removed or corrected for, however, there still remain occasional sudden, unpredictable changes of rate against which even a combination of seven clocks is powerless. The average change is not very much (·01 sec. per day is considered large), but such a change occurring at the beginning of a spell of cloudy weather may remain undetected for a week or more, by which time the extrapolation is seriously in error. In the belief that we are now approaching the limit of accuracy of which pendulum clocks are ultimately capable, horologists have, during the past few years, been exploring the possibilities of other clocks, quite different in principle. The immediate aim is to produce a clock, if possible independent of gravity, which can be used as a check on the variable rates of present observatory clocks. We shall see how the ultimate result may well be to dispense with the pendulum clock altogether.

In most of the new clocks a natural high-frequency oscillation (high, that is, compared with the 1 cycle/sec. of the ordinary pendulum) of some stable mechanical vibrator is maintained electrically. Subsequent stages step down the frequency to a convenient value for time-keeping purposes. The rapid growth in radio technique during recent years has been responsible for much of the advance along these lines, for obviously a frequency standard used for controlling say the 50 kilocycle/sec. carrier wave of a broadcasting station can in principle be regarded as a time standard with a unit of 0·00002 sec.

Oscillators in experimental use at present consist of tuning-forks driven electromagnetically, bars maintained in longitudinal vibration by magnetic, electrostatic, or magnetostrictive forces, and quartz crystals kept oscillating by means of the inverse piezo-electric effect. Space forbids the discussion of each in detail, but the precision attained in the various types will be of interest.

An elinvar tuning-fork designed by Dye at the National Physical Laboratory (*Proc. Roy. Soc.*, A, **143**, 285, 1934) has a natural period of 1000 cycles/sec. constant to 1 part in 10^7 if its temperature is controlled to $\pm$·006° C. With complete elimination of pressure and voltage changes, it is believed that this accuracy could be maintained over a period of months, whilst over short periods 1 part in 10^8 would be easily reached. The best pendulum clocks are some three times less accurate than this.

Sears and Tomlinson (*Observatory*, **57**, 191, 1934) are developing a clock which uses the electrostatically-maintained longitudinal vibrations in a 1 m. elinvar rod kept at a temperature controlled to $\pm\cdot002°$ C. The natural frequency of about 2366 cycles/sec. is applied direct to a phonic motor which is geared to provide a final speed of almost exactly 1 rev./sec. A Shortt clock employed to time this form of oscillator is itself such a relatively poor timekeeper that it can detect no variation in rate. Intercomparison with a quartz crystal oscillator suggests that the daily rate varies by ·001 sec. at most.

Clocks whose working depends on the supersonic oscillations of quartz crystals deserve more than the passing mention with which we have had to dismiss the other types, for they appear at present to offer most advantages over pendulum clocks, and because more time has been devoted to developing them than any of the other new oscillators. Their chief advantage lies in the stable mechanical properties of quartz. An alternating electric field applied across the faces of a quartz crystal, suitably cut, sets up a forced mechanical vibration whose amplitude is increased to a value of the order of the wave-length of light when resonance occurs at a natural period of the crystal. Natural periods of realisable crystals correspond to alternations of the order of 10^4 or 10^5 cycles/sec., and a circuit tuned to the appropriate frequency will maintain the oscillations indefinitely. For timekeeping purposes, multivibrators, such as are used for the measurement and control of frequency, are employed, singly or in cascade, to produce subharmonics of the master frequency. By this means a frequency of 1 cycle/sec. could be obtained, but in practice, the last downward step in frequency is usually performed by a synchronous motor which drives a clock dial directly.

The earliest quartz clocks were made ten years ago at the U.S. Bell Telephone Laboratories. They proved their mettle (and incidentally that of the Shortt clock) at once by detecting, in the comparisons with Shortt clocks, a semi-diurnal term which is due to the lunar variation in gravity, a variation which, of course, affects the pendulums but not the crystals. Perhaps the most successful run yet obtained is that of a clock at the Reichsanstalt, which has a daily rate constant to $\pm\cdot002$ sec. over six months. Over periods of days, the frequency can be kept constant to within 1 part in 10^8. The element is a rectangular quartz bar 9 cm. long vibrating longitudinally, the second harmonic at 60,000 cycles/sec. being used. Unfortunately this geometrical form of crystal has a relatively high temperature coefficient of frequency (-3 parts in

10^6 per ° C.), and a very closely controlled temperature enclosure is needed. The high frequency causes atmospheric damping to be an important factor, and in practice the crystal is operated *in vacuo* to avoid pressure variations.

A new form of quartz clock developed at the National Physical Laboratory has proved so satisfactory that a duplicate is shortly to be installed at the Royal Observatory, Greenwich (see *Proc. Phys. Soc.*, **50**, 413, 1938). The crystal is in the form of a cylindrical ring of mean radius 2·7 cm., whose axis is parallel to the optic axis of the quartz. Coaxial electrodes in a tuned circuit excite strong compressional waves of frequency 100,000 cycles/sec. round the circumference of the ring even in air at atmospheric pressure. The temperature coefficient can be reduced to zero at a convenient working temperature, and is only 1 part in 10^6 per ° C. at values $\pm 15°$ C. from that temperature, so that relatively coarse control is sufficient. A clock of this type gives the extraordinary stability of ± 4 parts in 10^{10} over hourly periods, and 1 part in 10^8 over a month or so.

Whether or not quartz clocks are destined to supplant pendulum clocks in the observatories of the world is a question for the future. In their favour are their frequency stability, any change of rate being apparently slow and uniform ; and their ease of intercomparison—this can be carried out between clocks in any part of the world in a few moments by obtaining beats at the radio frequency. On the other hand, they are more elaborate than pendulum clocks ; the average astronomer will be reluctant to pin his faith on a mechanism which involves as auxiliaries some 10 valves, a high-speed motor, and a number of batteries, and which must be run in a strictly controlled environment. They have yet to be proved markedly better than Shortt clocks for long runs of the order of years. In this connection it should be remarked that the driving circuit must, of course, react to some extent on the oscillator itself, and compel it to vibrate, not strictly in its natural period, but in a slightly modified one. Any changes in the circuit, such as ageing of the valves, voltage variations, etc., must therefore react on the frequency. Renewal of the amplifying valve, for instance, is known to produce a change of rate, but this will be of little importance in a group of clocks used collectively as a standard, particularly as intercomparison is so accurate and quick.

The quartz clock must be regarded as in its infancy, and consequently subject to teething troubles. It has nevertheless already proved itself some five times as reliable as the best pendulum clocks over short runs of the order of weeks. In ten years' time

modifications may well have been introduced which will enable us finally to relegate Galileo's invention to the museum.

PHYSICS. By F. A. Vick, Ph.D., University College, London.

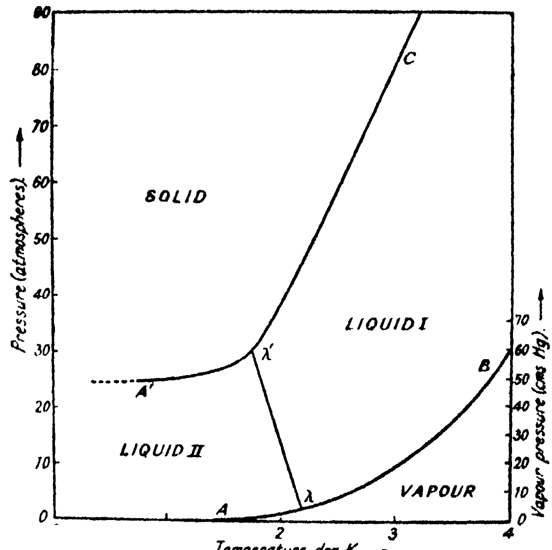
The Properties of Liquid Helium.—Water is sometimes referred to as "that liquid with amazing properties." During the last twenty years it has been becoming increasingly evident that the properties of liquid helium are even more amazing. Nearly every week we find in *Nature* a letter describing another exciting experiment with liquid helium or suggesting a further explanation of a phenomenon already known.

Helium was first obtained in liquid form by K. Onnes in 1908 (*Leiden Comm.*, No. 108, and Suppl. 21*a*). He found it to be a colourless, mobile liquid boiling at 4·22° K. under atmospheric pressure. His attempts to solidify helium by boiling under reduced pressure were unsuccessful, the liquid remaining perfectly mobile even at 0·82° K. (0·013 mm. Hg. pressure). This indicated a very different form of pressure-temperature diagram from that of other substances, with, perhaps, no triple point at which solid, liquid and vapour can exist together. In 1911 Onnes made the further observation (*Leiden Comm.*, No. 119) that liquid helium has a maximum density at about 2·2° K. (In this article all temperatures are reduced to the 1932 scale. The earlier scale was rather higher.) This was confirmed by more accurate measurements of Onnes and Boks (*Leiden Comm.*, No. 170*b*, 1924). The density rises by about 20 per cent. as the temperature falls from 4·2° K. to 2·19° K., reaching a maximum of 0·1462 gm./c.c., and just below this temperature there is a sudden drop (perhaps discontinuous), the density becoming approximately constant below 1·5° K. Just above 2·19° K. the coefficient of expansion is + 0·022 per degree, and just below it is − 0·0426 per degree.

The curve of dielectric constant against temperature has a very similar shape (W. H. Keesom and M. Wolfke, *Leiden Comm.*, No. 192*a*, 1928). The combined observations led to the suggestion that liquid helium can exist in two forms, I above 2·19° K, and II below this temperature. Following a suggestion of Ehrenfest, the transition temperature is called "the λ-point" (W. H. and A. P. Keesom, *Leiden Comm.*, No. 221*d*, 1932). The symbol "λ" is often used for latent heat, which implies a transformation. No surface of separation can be seen between the two forms of liquid helium, but, as described by MacLennan, Smith and Wilhelm (*Phil. Mag.*, **14**, 161, 1932), when the pressure above the liquid is lowered slowly, and the temperature with it, there is a sudden

change in appearance of the liquid as the λ-point is passed, rapid ebullition giving way to a perfectly clear and tranquil liquid.

Meanwhile, helium had been solidified by Keesom (*Nature*, **118**, 58 and 81, 1926; *Leiden Comm.*, No. 184*b*, 1926), who subjected the liquid to the combined effect of high pressure and low temperature. He was able to determine the pressures at which helium solidified at various temperatures, and so trace out the melting curve. This we can combine with the vapour pressure curves of liquid helium (Keesom, Weber, Nørgaard and Schmidt, *Leiden Comm.*, No. 202, 1929) and the variation of the λ-temperature with pressure (Keesom and Clusius, *Leiden Comm.*, No. 216*b*, 1931) to give the pressure-temperature diagram of helium. Thus in the figure, A′λ′C is the melting curve, and AλB the vapour pressure curve. The co-ordinates of λ′ are T = 1·753° K., p = 29·91 atmospheres, and those of λ are T = 2·19° K., p = 3·865 cm. Hg. The critical temperature and pressure of helium I are 5·1° K., and 2·3 atmospheres.



Temperatures below 4° K. are often measured via the vapour pressure curve of helium, so the equations of the curves Aλ and λB have been worked out carefully. They are

for Aλ $\log_{10} p = 2{\cdot}035 - 3{\cdot}859/T + 0{\cdot}922 \log_{10} T$

for λB $\log_{10} p = 1{\cdot}217 - 3{\cdot}024/T + 2{\cdot}208 \log_{10} T$

where p is in cm. of Hg. In order to make it visible, AλB has been drawn with a larger scale of pressure than A′λ′C. It will be seen that vapour and solid cannot exist together.

The density-temperature ($\rho - T$) curves have been plotted accurately by W. H. and A. P. Keesom (*Leiden Comm.*, No. 224, and Suppl. 76*b*; *Physica*, **1**, 128, 1933–4). Sudden changes in density occur along $\lambda\lambda'$, being more pronounced at the higher pressures. For helium II $d\rho/dT \rightarrow 0$ as $T \rightarrow 0$. Also, van Urk, Keesom and Onnes (*Leiden Comm.*, No. 179*a*, 1925) found that the surface tension γ rises linearly with fall in temperature down to the λ-point, changes suddenly, and becomes nearly constant below 1·5° K. Thus $d\gamma/dT \rightarrow 0$ as $T \rightarrow 0$. We can see from the $p - T$ diagram that the melting-point curve becomes nearly horizontal below 1·5° K. These three results are in agreement with the Nernst Heat Theorem, which states that changes taking place at absolute zero do so without change in entropy.

Calorimetry of solid and liquid helium has been carried out at Leiden, heat being generated electrically and temperature measured by resistance thermometers. Rate of change of temperature gives data for specific heats, and absorption of heat at constant temperature data for latent heats. Some latent heats of fusion are 0·835 cal./gm. at 2·5° K., 1·089 cal./gm. at 3·0° K. and 1·365 cal./gm. at 3·5° K. (W. H. and A. P. Keesom, *Physica*, **3**, 105, 1936). Inserting these values and the corresponding densities in the Clausius-Clapeyron equation, we find the change in melting-point with pressure to be 0·02° per atmosphere, which is in good agreement with the slope of $\lambda'C$. The latent heat of vaporisation between 1·5° K. and 3·0° K. is about 5·5 cal./gm. only, with a small anomaly at the λ-point (Dana and Onnes, *Leiden Comm.*, No. 179*c*, 1926). This very small value emphasises the need for good heat insulation if the liquid is to be kept for any length of time (compare 110 cal./gm. for hydrogen and 540 cal./gm. for water). The latent heat of transformation from helium II to I is probably zero, certainly less than 0·002 cal./gm. Thus we can hardly speak of a change of phase, in the thermodynamic sense; Ehrenfest calls it a "phase change of the second order" (*Leiden Comm.*, Suppl. 75*b*, 1933).

The specific heat of the liquid in equilibrium with its vapour shoots from 2 cal. per gm. per deg. to 6 cal. as the temperature rises to 2·19° K., and then suddenly drops to 1·5 cal. per gm. per deg., within perhaps 0·001° K. (W. H. and A. P. Keesom, *Leiden Comm.*, No. 221*d*, 1932). These two workers have also studied the changes in specific heats at higher pressures (*ibid.*, No. 235*d*; *Physica*, **2**, 557, 1935).

The specific resistance of liquid helium (Wolfke and Keesom, *Physica*, **3**, 823, 1936), its optical properties (Satterley, *Rev. Mod Phys.*, **8**, 357, 1936; Schubnikow and Kikoin, *Phys. Zeits. Sowjet*, **10**,

119, 1936) and the velocity of sound in the liquid (Burton, *Nature*, **141**, 970, May 1938; Findlay, Pitt, Smith and Wilhelm, *Phys. Rev.*, **54**, 506, Oct. 1938) do not call for much comment. The thermal conductivity, viscosity, and associated properties of the liquid are, however, of the greatest interest. During their work on the specific heats of liquid helium (*Physica*, **2**, 557, 1935) W. H. and A. P. Keesom inferred that the thermal conductivity changes suddenly at the λ-point. Preliminary figures, obtained by immersing in the liquid two parallel flat coils of wire, one as heater and the other as thermometer, were published in 1936 (*Physica*, **3**, 359, May 1936), assigning to helium I a thermal conductivity of 6×10^{-5} cal. deg.$^{-1}$ cm.$^{-1}$ sec.$^{-1}$, which is comparable with that of gases at room temperatures, and to helium II the astonishing figure of 190. This is 200 times the conductivity of copper at room temperatures, and 3×10^{6} times that of helium I. These figures provoked Allen, Peierls and Uddin (*Nature*, **140**, 62, July 1937) to repeat the measurements by, in effect, combining a vapour pressure thermometer with a liquid helium manometer. A cylindrical glass bulb was sealed on the top of a capillary tube and contained an electric heating coil. It was evacuated, immersed in a liquid helium bath, and then filled with the liquid. The temperature of the heating coil was raised until some of the helium in the bulb evaporated. The difference of level in bulb and bath was measured by a cathetometer. At equilibrium the vapour pressure of the helium in the bulb could be calculated from the difference in hydrostatic pressure, and hence the temperature difference between bulb and bath (i.e. between the ends of the capillary). Heat flow through the glass walls was a small proportion only of that through the liquid. They reported that the calculated "conductivity" increased as the temperature gradient increased, reaching $1{\cdot}1 \times 10^{4}$ cals. deg.$^{-1}$ cm.$^{-1}$ sec.$^{-1}$ for $\Delta T = 5 \times 10^{-5\circ}$ K., and $T = 2{\cdot}06^\circ$ K. These results were not explained.

Measurements of the viscosity of liquid helium had been made at Toronto (Burton, *Nature*, **135**, 265, 1935; Wilhelm, Misener and Clark, *Proc. Roy. Soc.*, A. **151**, 342, 1935) from observations of the logarithmic decrement of a cylinder oscillating in the liquid. For helium I at $2{\cdot}3^\circ$ K., $\eta = 270 \times 10^{-6}$ poise, and for helium II at just below $2{\cdot}2^\circ$ K., $\eta = 33 \times 10^{-6}$ poise. Thus the viscosity of liquid helium is extremely low (compare $\eta = 10{,}000 \times 10^{-6}$ poise for water) and there is a change by a factor of 8 at the λ-point. Kapitza pointed out (*Nature*, **141**, 74, Jan. 1938) that since the kinematic viscosity η/ρ is very small the Reynolds number, **R**, is much too large for the flow of liquid round the oscillating cylinder

to be laminar, and therefore the Toronto value for η is probably too high. Kapitza measured the viscosity by observing the flow of liquid between two optically-worked glass discs. When the discs were nearly in contact (separation $0 \cdot 5\mu$) the flow of helium I could just be detected, whereas that for helium II was very rapid, and he calculated that η for helium II is smaller by a factor of at least 1,500 than η for helium I. He also concluded that the flow was still turbulent. If, however, the flow is assumed to be laminar for a rough calculation, $\eta = 10^{-9}$ poise is an upper limit for helium II, and this is 10^{-4} of η for hydrogen gas. Using the upper limit, R is about 50,000, so turbulence is indeed to be expected. (R should not be greater than 890 for laminar flow—Davies and White, *Proc. Roy. Soc.*, A, **119**, 92, 1928.) Kapitza suggested that this small viscosity may make possible an explanation of the high thermal conductivity, in terms of convection currents, turbulent motion being set up during ordinary manipulation of the liquid, and also may account for the observations of Allen, Peierls and Uddin.

In the same number of *Nature* (**141**, 75, Jan. 1938) Allan and Misener describe measurements of viscosity by flow through capillary tubes. They found that the velocity of flow varied only slightly for large changes of pressure head and cross-sectional area. An upper limit of 4×10^{-9} poise for η is given, but they disagree with Kapitza's suggestion that the reason for the observed thermal conductivity can be assigned to undamped turbulent motion during the experiment.

Allen and Jones continued the experiments on thermal conductivity (*Nature*, **141**, 243, Feb. 1938) with the same apparatus as before, but with a narrower capillary and a temperature of $1 \cdot 08°$ K., small heat flows produced a *rise* in level of liquid in the closed bulb instead of the expected fall due to increased vapour pressure. The bulb was then changed to one open at the top; the level rose to a greater extent, determined by the heat flow. This effect was, of course, opposing the effect of increased vapour pressure in the original experiments of Allen, Peierls and Uddin, and may account for the non-linear variation of heat flow with temperature. The rise of level was shown in a very striking way when a capillary tube, connected at the bottom to a wider tube filled with fine emery powder and open to the liquid bath, was allowed to project above the surface of the liquid. When radiation from an electric lamp fell on the powder, a jet of liquid helium was observed to flow out of the top of the capillary, forming a "fountain" which rose as high as 16 cm. above the top of the tube.

Interesting experiments were described by Daunt and

Mendelssohn (*Nature*, **141**, 911, May 1938) who suspended a small beaker by a thin glass fibre so that it could be lowered into a bath of liquid helium II. Heat entry was reduced to a minimum, observations being made by the light of a neon lamp. When the empty beaker was lowered into the liquid, it filled up to the level of the bath, though the rim of the beaker was above the liquid level. If then the beaker was raised, the level inside it fell at the same rate as the original rise, until the levels inside and out were again the same. By increasing the surface artificially with "wicks" of copper wire, it was shown that the effect was due to flow over the surface and not to distillation. Rate of flow increased as the temperature *decreased*. The phenomenon does not occur appreciably above the λ-point. The experiments appear to show that the surface of a solid in contact with liquid helium II is covered with a thin film through which the liquid is transferred to the lowest available level. The transfer may depend on viscosity, which decreases as the temperature goes down (this is unusual for a liquid).

Experiments carried out by Kikoin and Lasarew (*Nature*, **141**, 912, May 1938; **142**, 289, Aug. 1938) on the thermal conduction along a glass rod heated at the top and with its lower end in liquid helium II enabled them to estimate the thickness of the film as between 10^{-5} and 10^{-6} cm., and showed that the thermal transfer is very high. This was confirmed by Daunt and Mendelssohn in a further communication (*Nature*, **142**, 475, Sept. 1938), in which they suggest that the whole heat transfer can be accounted for by actual flow of liquid. It is obvious that such flow of liquid along surfaces could vitiate measurements of viscosity by flow through capillary tubes if precautions are not taken. Bearing this in mind, measurements have been made by Giaugue, Stout and Barieau (*Phys. Rev.*, **54**, 147, July 1938) who give $\eta = 1{\cdot}1 \times 10^{-7}$ poise at $1{\cdot}468^\circ$ K. and $3{\cdot}8 \times 10^{-7}$ at $1{\cdot}823^\circ$ K; by workers at Toronto (Burton, *Nature*, **142**, 72, July 1938) giving $\eta = 7 \times 10^{-5}$ poise at $2{\cdot}16^\circ$ K; and by Keesom and Macwood in a series of experiments using an oscillating disc (*Physica*, **5**, 737, Aug. 1938). Their results range from $1{\cdot}24 \times 10^{-6}$ at $1{\cdot}304^\circ$ K. to $29{\cdot}8 \times 10^{-6}$ at $4{\cdot}021^\circ$ K., with some anomaly at the λ-point. It appears that the viscosity tends to zero as the absolute zero of temperature is approached. The amplitude of oscillations of the disc was so small that the linear velocity of the edge was never greater than $0{\cdot}3$ mm./sec., making fairly sure that the motion of the liquid was laminar.

Finally, to give some idea of the great experimental difficulties overcome at Leiden and other cryogenic laboratories, we will mention the X-ray study of the structure of solid and liquid helium

by Keesom and Taconis (*Physica*, **5**, 161 and 270, 1938). Solid helium can be obtained only at pressures greater than 25 atmospheres. It was compressed in a thin-walled aluminium tube mounted axially in the camera and capable of being rotated. The whole camera (including the film !) had to be kept below 1·7° K. ; this was accomplished by making use of the flow of liquid helium II up long cotton wicks dipping into a bath of the liquid. The thermal conductivity of the soaked wicks was then 200 times that of copper. The X-ray tube was on top of the Dewar vessel, Cu Kα radiation passing through nickel foil and an aluminium window. Keesom and Taconis concluded that solid helium probably has a hexagonal structure (close-packed). For liquid helium a horizontal jet was used, the liquid being raised from the bath by a centrifugal pump running at 2,000 revolutions per minute. Liquid helium I gave rings analogous to those of other liquids. Those for helium II fitted a structure based on a face-centred cubic lattice from which half the atoms have been removed, in such a way that each remaining atom is surrounded by six other atoms and by six holes.

The subject matter of this article has been confined to some of the more interesting experimental results. It is hoped in a subsequent article to deal with the significance of these results and with attempts to explain the striking differences of behaviour between liquid helium I and II. The properties of matter under the special conditions near the absolute zero are of great interest and fundamental importance.

Summaries of earlier work on liquid helium are given by L. C. Jackson (*Low Temperature Physics*, Methuen, 1934), J. Satterley (*Rev. Mod. Phys.*, **8**, 347, 1936) and W. H. Keesom (*Sur les Phénomènes Lambda de l'Helium*, *Leiden Comm.*, Suppl. 80, 1936).

GENERAL AND ORGANIC CHEMISTRY.

By O. L. Brady, D.Sc., F.I.C., University College, London.

In recent years a considerable amount of work has been done on the alkylation of aromatic hydrocarbons and it may be useful to summarise the present position as regards reactions of this type.

The classical method of introducing alkyl groups into aromatic hydrocarbons is that due to Friedel and Crafts (*Compt. rend.*, 1877, **84**, 1392) using alkyl halides, RX, with anhydrous aluminium chloride as catalyst.

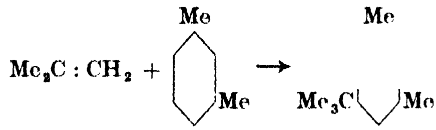
$$C_6H_6 + RX \rightleftharpoons C_6H_5R + HX.$$

The reaction is reversible, and when hexamethylbenzene and aluminium chloride are heated in a current of hydrogen chloride

the methyl groups may be detached successively with the formation of penta-, tetra-, etc., methylbenzenes (Jacobsen, *Ber.*, 1885, **18**, 339). In consequence, alkyl groups may be transferred from one nucleus to another, for example, when toluene is heated with aluminium chloride, benzene and *m*- and *p*-xylenes are produced (Copisarow, *J. Chem. Soc.*, 1921, **119**, 1806).

$$2C_6H_5Me \rightleftharpoons C_6H_6 + C_6H_4Me_2.$$

Unsaturated hydrocarbons may replace alkyl halides, for example, *iso*butylene and *m*-xylene yield 3 : 5-dimethyl-*tert.*-butylbenzene (D.R.P., 184,230).



Alkyl borates, prepared by heating an alcohol with boric acid, may also be used in place of alkyl halides and sometimes give better yields (F.P., 720,034, 1932). Secondary and tertiary but not primary alcohols may replace the halide (Huston and Hsieh, *J. Amer. Chem. Soc.*, 1936, **58**, 439).

$$C_6H_6 + Me.CHOH.Me \longrightarrow C_6H_5CHMe_2 + H_2O.$$

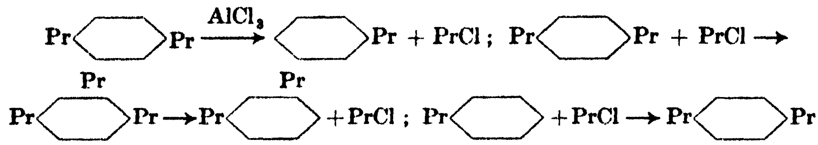
A second alkyl group enters more readily than the first and consequently, even with an excess of benzene, mixtures of mono-, di-, etc., alkylbenzenes are produced. When it is possible isomerisation of the alkyl group occurs giving an increased branching of the chain, for example, *n*-butyl chloride gives *sec.*-butylbenzene and *iso*-butyl chloride gives *tert.*-butylbenzene (Shoesmith and McGechan, *J. Chem. Soc.*, 1930, 2231).

$$C_6H_6 + CH_3(CH_2)_2CH_2Cl \longrightarrow C_6H_5.CH\begin{cases}CH_2CH_3\\CH_3\end{cases} + HCl$$

$$C_6H_6 + (CH_3)_2CH.CH_2Cl \longrightarrow C_6H_5C(CH_3)_3 + HCl.$$

When an alkyl group is already present in the nucleus, the dialkyl compound formed consists mainly of the *meta*- with a small amount of the *para*-derivative (Shoesmith and McGechan, *loc. cit.*). The formation of a large amount of the *meta*-substituted compound is contrary to the substitution rule and it is suggested that it is due to a greater stability of *m*-dialkylbenzines. *Para*-substitution takes place first and is followed by a migration of the group. It has been found that *p*-dipropylbenzene on heating with aluminium chloride gave a considerable amount of *m*-dipropylbenzene. Objection have been made to the suggestion that this change is due to the reversibility of the Friedel-Crafts reaction and the splitting

off of propyl chloride and re-alkylation on some such scheme as the following:



since no isomerisation of the alkyl group occurs during the reaction, whereas propyl benzene with propyl chloride would give propyl-*iso*-propyl benzene (Baddeley and Kenner, *J. Chem. Soc.*, 1935, 303). It has been reported, however, that isomerisation does occur during the migration of butyl groups in 1 : 3-dimethyl-4-butyl-benzene (Nightingale and Smith, 96th Meeting of the American Chemical Society, Milwaukee, Sept. 5, 1938, quoted by Price and Ciskowski, *loc. cit.*).

Alkylation of benzene with certain alcohols in the presence of concentrated sulphuric acid has long been known (Verley, *Bull. Soc. Chim.*, 1898, **19**, 68), but the reaction has been more fully investigated by Meyer and Bernhauer (*Monatsh.*, 1929, **53**, 721). Here again isomerisation of the alkyl group occurs when this is possible, propyl alcohol giving *iso*-propyl-, *n*-butyl alcohol *sec*-butyl-, and *sec*-butyl alcohol *tert*-butyl-benzenes. Benzyl alcohol reacts, but methyl alcohol does not, and ethyl alcohol only at 170° under pressure. In addition to mono-substituted derivatives, di- and tri-substituted compounds are formed, the second alkyl group entering into the *para*-position to the first.

More recently McKenna and Sowa (*J. Amer. Chem. Soc.*, 1937, **59**, 470) have used boron trifluoride as a condensing agent. In this case methyl and ethyl alcohols do not react but *n*- and *iso*-propyl alcohols give mono-, di- and poly-*iso*-propyl-benzenes, *n*- and *sec*-butyl alcohols yield *sec*-butylbenzenes and *iso*- and *tert*-butyl alcohols *tert*-butylbenzenes; *cyclo*hexyl alcohol and benzyl alcohol also give mixtures of mono-, di- and poly-*cyclo*hexyl and benzyl benzenes. In all cases the second alkyl group enters *para* to the first.

The reaction has been extended by Price and Ciskowski (*J. Amer. Chem. Soc.*, 1938, **60**, 2499) to the alkylation of naphthalene, but contrary to these authors' hopes the alkyl group enters in the β-position as is the case in the Friedel-Crafts reaction; *cyclo*hexyl, *tert*-butyl, *iso*-propyl and benzyl alcohols all react satisfactorily with naphthalene in the presence of boron trifluoride to give β-mono-alkyl derivatives with varying quantities of di- and tri-substitution compounds according to the amount of alcohol employed.

These authors discuss in some detail the mechanism of the reaction. McKenna and Sowa (*loc. cit.*) held that alkylene formation preceded the alkylation which then consisted in the addition of the alkylene to benzene, it having been found that alkylenes, with boron trifluoride in the presence of another catalyst, usually sulphuric acid, would alkylate benzene (Slanina, Sowa and Nieuwland, *J. Amer. Chem. Soc.*, 1935, **57**, 1547). The sulphuric acid could, of course, convert the alkylene to the alcohol or to its sulphate. It was now found, however, that naphthalene reacted with *cyclo*hexene in the presence of boron trifluoride alone to give a fairly good yield of β-*cyclo*hexylnaphthalene. There are, however, two objections to the view that the alkylene is an intermediate product, firstly *cyclo*hexanol is not converted to *cyclo*hexene by boron trifluoride under conditions even more drastic than exist in the alkylation process, and secondly benzyl alcohol, which reacts as readily as any secondary or tertiary alcohol cannot form an alkylene.

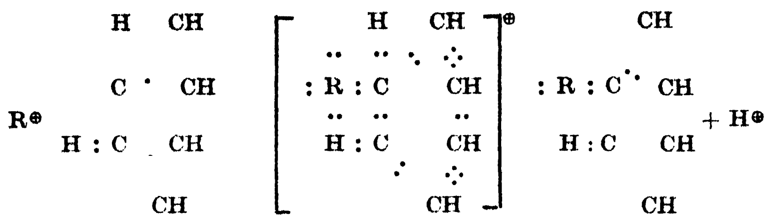
Price and Ciskowski suggest an explanation of the mechanism of the reaction based on the electron affinity of boron trifluoride. They suggest that its catalytic effect may be due to a weakening of the carbon-oxygen bond in the complex formed between the catalyst and the oxygen containing organic compound

$$\mathrm{R}:\underset{\cdot\cdot}{\overset{\mathrm{H}}{\overset{\cdot\cdot}{\mathrm{O}}}}:\;+\;\underset{\underset{\mathrm{F}}{\cdot\cdot}}{\overset{\mathrm{F}}{\overset{\cdot\cdot}{\mathrm{B}}}}:\mathrm{F}\rightleftharpoons\underset{[\mathrm{I}]}{\mathrm{R}:\underset{\cdot\cdot}{\overset{\mathrm{H}}{\overset{\cdot\cdot}{\mathrm{O}}}}:\underset{\underset{\mathrm{F}}{\cdot\cdot}}{\overset{\mathrm{F}}{\overset{\cdot\cdot}{\mathrm{B}}}}:\mathrm{F}}\rightleftharpoons\underset{[\mathrm{II}]}{\left[\mathrm{R}:\underset{\cdot\cdot}{\overset{\cdot\cdot}{\mathrm{O}}}:\underset{\underset{\mathrm{F}}{\cdot\cdot}}{\overset{\mathrm{F}}{\overset{\cdot\cdot}{\mathrm{B}}}}:\mathrm{F}\right]^{\ominus}}\;+\;\mathrm{H}^{\oplus}$$

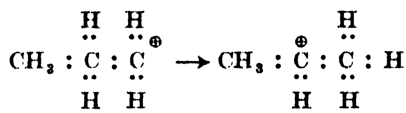
The boron trifluoride-alcohol complexes [I] are strong acids indicating a weakening of the oxygen-hydrogen bond and they ionise as in [II]. They consider it not unreasonable to suppose that there is also a weakening of the oxygen-carbon bond and that [I] is at least capable of ionising as in [III].

$$\underset{[\mathrm{I}]}{\mathrm{R}:\underset{\cdot\cdot}{\overset{\mathrm{H}}{\overset{\cdot\cdot}{\mathrm{O}}}}:\underset{\underset{\mathrm{F}}{\cdot\cdot}}{\overset{\mathrm{F}}{\overset{\cdot\cdot}{\mathrm{B}}}}:\mathrm{F}}\rightleftharpoons\underset{[\mathrm{III}]}{\left[:\underset{\cdot\cdot}{\overset{\mathrm{H}}{\overset{\cdot\cdot}{\mathrm{O}}}}:\underset{\underset{\mathrm{F}}{\cdot\cdot}}{\overset{\mathrm{F}}{\overset{\cdot\cdot}{\mathrm{B}}}}:\mathrm{F}\right]^{\ominus}}\;+\;\mathrm{R}^{\oplus}$$

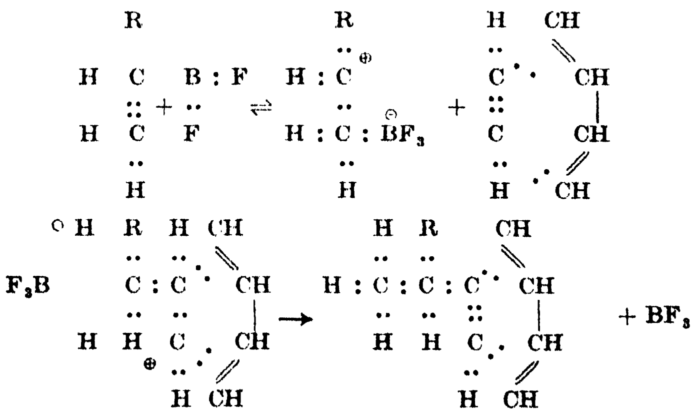
and that the carbonium ion $\mathrm{R}^{\oplus}$ is the active alkylating agent. It is not necessary to assume that such ionisation takes place except in the presence of a compound capable of donating electrons to the carbonium ion, for example, benzene, and even then the ion may have only a transitory existence, but for convenience the reaction may be represented thus using the free ion.



The isomerisation that takes place in the alkyl radical during these reactions offers no difficulty, as the products are always such as one would expect if the electron deficiency in the alkyl ion migrated from a primary to a secondary and from a secondary to a tertiary carbon atom (Whitmore, *J. Amer. Chem. Soc.*, 132, **54**, 3274).



The action of the alkylenes is explained also on the basis of the electron affinity of boron trifluoride with the formation of a polar compound (IV) which attracts electrons from the benzene.



In the Friedel-Crafts reaction the action of aluminium chloride, which also contains an incomplete octet, can be interpreted in a similar manner.

PHYSICAL CHEMISTRY. By H. W. Melville, D.Sc., Ph.D., Colloid Science Laboratory, Cambridge.

The luminescence emitted by solids upon exposure to visible and ultra-violet light, to X-rays and to electrons has long been known and much studied.[1] The accumulation of data has, moreover,

[1] A note dealing with the recent conference of the Faraday Society on Luminescence will be found on p. 563.

become so voluminous and the variety of effects so diverse that the invention of working hypotheses to explain the phenomena was apparently a hopeless task. Within the last year or two, however, the modern theory of solids has been applied to the problems with results which promise to bring some degree of order into this field of research. As yet the applications are rather of a qualitative nature and will be briefly described in the following pages, attention being devoted almost exclusively to the luminescence brought about by visible and ultra-violet light.

Absorption Spectra of Solids.—In the visible and ultra-violet, absorption is due essentially to excitation of electrons as happens with gaseous molecules or atoms. Consequently it would be anticipated that the electrons would be raised from the ground state to a discrete series of energy levels succeeded by a continuum in which the absorbing centre becomes ionised. In this latter case, the electron is raised into the so-called conducting band of the solid leaving a positive hole in the lattice. The solid thereby becomes photoconducting. The appearance of a continuum cannot be entertained as a criterion of ionisation for the discrete levels exhibited by an atom in the gaseous state are usually drastically modified in the solid. There are at least two reasons for this behaviour. Just as there are bands in molecular absorption spectra due to the vibrational energy of a molecule, so in solids the complex lattice vibrations are superimposed on the electronic transition with the result that the originally sharp level becomes broad. This broadening can, however, be considerably diminished by working at low temperatures. For example, Randall (*Nature*, **142**, 113, 1938) found that the broad band emitted by zinc orthosilicate, activated by manganese, with a peak at 3250 A. is reduced to half its width by cooling from 300 to 20° K. Other sharp bands make their appearance at the lower temperature. Similarly the strong electric and magnetic fields existing in solids may either lead to broadening or to splitting of energy levels.

Exceptions to the above-mentioned behaviour occur when the absorbing centre is not influenced by these two factors. For example, the absorption spectra of the rare earth elements and the transition elements are usually sharp for the simple reason that the incomplete shell of electrons within the valency shell is so well screened that external factors have little influence on the electronic transition, though there may be some modification of the position of the levels. Similar behaviour is obtained with compounds which contain conjugated double bonds, or benzene rings. Although such absorption spectra in the solid are sharp at low temperatures,

their structure, when compared with that in the gas phase, clearly shows the influence of the forces present in the solid lattice. Also certain co-ordination groups, *e.g.* UO_2^{++} are little subject to external fields—a fact which makes the uranyl salts and solutions so convenient for fluorescent experiments.

At the other extreme, absorption of light may induce photochemical action as in the silver halides. The other two possibilities are :

(1) The electron, if excited to a discrete level, falls back to the ground state with emission of radiation. Usually the electron has lost some of its energy before this transition occurs. This process is normally termed fluorescence, the lifetime of the excited state being of the order of 10^{-8} sec., that is, comparable with that of a free excited atom. Similarly the decay of the fluorescence, since it is concerned with only *one* absorbing centre, follows the usual exponential law, namely, $I = I_0 e^{-\alpha t}$, I_0 and I being the intensity of the fluorescence at $t = 0$ and $t = t$ and α a characteristic constant.

(2) The electron, when in a discrete level, receives sufficient energy from the lattice at high enough temperatures to enter the conduction band when it moves away from the absorbing centre to be trapped perhaps by a bound energy level at a crystal boundary or at a lattice defect (Gurney and Mott, *Trans. Faraday Soc.*, Jan. 1939). Finally, however, the electron will find a positive hole with which it combines, radiation being simultaneously emitted. Radiation emitted in this manner within a time interval greater than 10^{-6} sec. is usually termed phosphorescence. The important point is that according to this theory of phosphorescence the decay law should be quite different from that for fluorescence. Since the electron and positive hole must come together, the rate of emission of radiation will be proportional to the product of their concentrations (n), or $\frac{dn}{dt} = -\beta n^2$ or $n = (\beta t + \text{const.})^{-1}$, that is the decay follows a bimolecular law.

It has already been mentioned that the wavelength of fluorescence is generally longer than that of the exciting source and that the efficiency of the process is temperature dependent. In order to explain these facts conceptions familiar in dealing with gaseous molecules may be used. The energy of the absorbing centre may be represented qualitatively for the present purpose as a function of configurational co-ordinates in a two-dimensional diagram as shown in Fig. 1 (strictly, of course, the representation should be in 3N dimensional space, where N is the number of particles concerned). Curve *a* refers to the ground state of the system and *b* to one of the

excited states. According to the Franck Condon principle the atomic nuclei do not change their positions appreciably upon the absorption of a quantum with the result that the excited state is one possessing considerable vibrational energy (position A′). Now since the life of the excited state is *c.* 10^{-8} sec. and the time of interatomic vibration *c.* 10^{-13} sec., the excited state will lose its vibrational energy to the lattice as heat (position B′) before the electron jump to the lower level occurs, the system thereby arriving

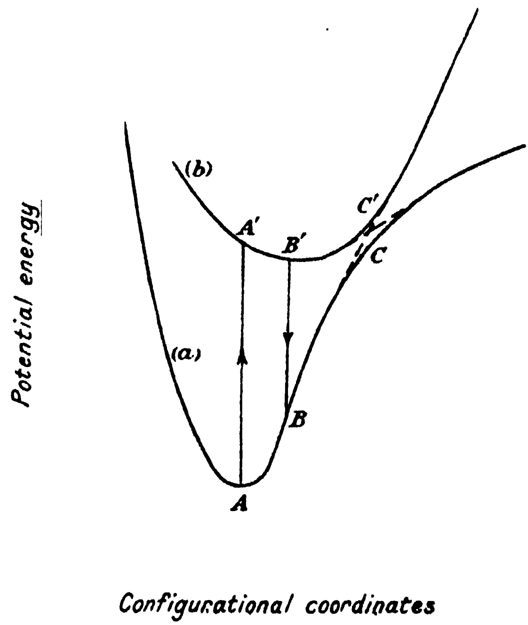


FIG. 1.

at position B with the emission of a quantum somewhat smaller than that absorbed.

While there has not been a great deal of work done on the variation of fluorescence intensity with temperature, there is now evidence (Randall, *Trans. Faraday Soc.*, Jan. 1939) that the intensity increases with decreasing temperature among those pure compounds not exhibiting photoconductivity. Examples are manganous salts, lead, cadmium and samarium salts which only fluoresce at 20° K. The list of substances given by Randall is so diverse that it is not impossible that fluorescence under suitable conditions may be a much more widespread phenomenon than was once supposed. Gurney and Mott have suggested how this temperature dependence may be

explained. The quantum efficiency (η) for fluorescence is defined by

$$\eta = \frac{A}{A + B},$$

where A and B are respectively the probabilities per unit time that a quantum of radiation is emitted and that the electronic energy is dissipated as heat. B may be made temperature dependent if the assumption is made that the potential energy curves for the ground state and the excited state cross (Fig. 1). There will then be a high probability that in this position C, the excited state will revert to the normal state. But if the excited state has already lost its vibrational quanta and is in position B′, additional energy W will have to be acquired from the lattice before point C′ is reached and the transition occurs. B may then be expressed by an equation giving the probability of this occurrence, namely,

$$B = be^{-W/kT}$$

where the constant b is of the order of the interatomic vibration frequency, *viz.*, 10^{13} sec.$^{-1}$, k = Boltzmann constant, T absolute temperature. Hence

$$\eta = (1 + \frac{b}{A}.e^{-W/kT})^{-1}.$$

Moreover since A = 10^8 sec.$^{-1}$, b/A is large compared with unity and hence (a) not only will fluorescence decrease with increasing temperature but (b) the transition from $\eta = 1$ to $\eta = 0$ will occur within a comparatively small temperature range.

Luminescence of Impure Solids.—The most striking examples of luminescence in solids are provided by impure substances, that is, solids consisting mainly of one type of molecule, to which have been added various activating substances, *e.g.*, manganese in zinc silicate; copper, silver and lead in zinc sulphide and in calcium sulphide. Many of these phosphors, as they are usually termed, are of ill-defined chemical constitution and cannot be obtained in large crystals suitable for quantitative investigation. Although they are of great importance technically a simpler type of solid, exhibiting both fluorescence and phosphorescence, is more likely to be amenable to theoretical study. Such substances have been found by Hilsch, Pohl and their collaborators (see, for example, Hilsch, *Proc. Phys. Soc.*, **49**, 40, 1937) in the alkali halides, activated by small concentrations—0·05 to 0·5 per cent.—of the corresponding thallous halides. Sufficiently large crystals may be grown from the molten salt.

When thallium is added, continuous absorption bands are produced at a wavelength somewhat longer than that of the fundamental

TABLE I

Absorption Peaks of Alkali-Halide Thallium Phosphors

Substance.	1st Fundamental.		A		B		C	
	e.v.*	A	e.v.	A	e.v.	A	e.v.	A
NaCl. . .	7·82	1592	4·87	2550	5·80	2145	6·20	2010
KCl . . .	7·60	1636	4·92	2530	5·90	2110	6·30	1976
RbCl . .	7·39	1684	4·98	2500	5·94	2095	6·40	1944
CsCl . . .	7·63	1632	4·90	2540	5·90	2110	6·30	1975
LiBr . .	6·68	1862	—	—	—	—	—	—
NaBr . .	6·49	1920	4·63	2690	—	—	5·72	2175
KBr . .	6·58	1892	4·73	2625	—	—	5·88	2120
RbBr . .	6·43	1936	4·77	2610	—	—	5·82	2135
CsBr. . .	6·61	1880	4·69	2650	—	—	5·76	2160
LiI . . .	5·59	2220	—	—	—	—	—	—
NaI . . .	5·38	2310	4·22	2950	—	—	5·28	2355
KI . . .	5·63	2210	4·30	2890	—	—	5·23	2375
RbI . . .	5·55	2240	4·32	2880	—	—	5·15	2415

* electron volts.

band of the alkali halide crystal itself. With the chlorides three distinct absorption bands make their appearance, the position of the maximum, as Table I shows, being practically independent of the nature of the cation. The bromides and iodides only exhibit two bands, but here the wavelength has been increased by the anion. On irradiation with wavelengths listed in Table I these crystals do not exhibit photoconductivity.

In so far as the emission of radiation is concerned, the potassium chloride phosphor has received most attention and the facts may be briefly summarised thus: When the concentration of the activator is small—0·0015 per cent.—fluorescence of a decay period of less than 10^{-5} sec. can be detected; phosphorescence is absent. The emission spectra consist of broad bands which seem to have their origin in two superimposed (three with KBr) all lying on the long wavelength side of the absorption bands. The entire emission spectrum is produced no matter in what excitation band the radiation is absorbed; the relative intensities of the bands is also independent of the exciting wavelength. The quantum efficiency of the fluorescence is of the order of unity.

The phosphorescence has rather different characteristics. For KCl the intensity of the phosphorescence is proportional to the square of the intensity of the exciting radiation for brief periods of illumination with weak enough light. Only light in absorption regions B and C is effective in inducing phosphorescence. That in

region A has no influence. The decay follows an exponential law $dN/dt = -\alpha N$ where N is equal to the total number of quanta to be emitted. α varies with temperature according to the simple equation $\alpha = se^{-\epsilon/kT}$ where $s = 2 \cdot 9 \times 10^9$ sec.$^{-1}$ for KCl and $\varepsilon = 0 \cdot 67$ electron volts. This means that the decay of the phosphorescence is arrested by lowering the temperature. As with the fluorescence, α is the same for all parts of the emission band. In addition, there is the important phenomenon that light of a suitable wavelength will induce emission of phosphorescent light ; the quantum, however, must not be less than 0·67 e.v. The efficiency of the phosphorescent process is not increased, and the effect is only observed provided the phosphor has already absorbed light in regions B and C.

In a recent paper[1] Seitz (*J. Chem. Physics*, **6**, 150, 1938) has made a successful effort to explain these significant phenomena. First it is necessary to account for the absorption spectra. From the fact that the position of the peak of absorption is determined predominantly by the anion, it is probable that one of the electrons of the halogen ion is excited. But since neither photoconductivity nor reaction ensues, the electron must be raised into a discrete level lying somewhat below the continuum or ionisation band associated with the alkali atom. With sufficiently short wavelength light it is possible that the electron might be raised to the ionisation band and thus confer a measure of electric conductivity on the crystal.

The absorption bands due to the presence of thallium ions probably have quite a different origin. Seitz suggests that the absorption is due to the thallium ion and not to excitation of the halide ions. A glance at Table I will show that the peaks in any one band are much less dependent on the nature of the alkali or halide ion than is the case with the pure halide. For example, by replacing Cl′ with I′, in the phosphor A band, there is only a change of 0·6 e.v., whereas a change of 2 volts is observed in the halides. If the A, B and C bands were due to halide ions they should exhibit doublet structure characteristic of such ions (Pohl, *Proc. Phys. Soc.*, **49**, 5, 1937), whereas, in fact, none is shown. Again the absorption should lie much further into the ultra-violet. The argument for the hypothesis is briefly this : Since the rubidium and thallium ions have approximately the same radius, replacement of rubidium by thallium should not cause appreciable distortion of the lattice. Hence the surrounding crystal field will not be greatly altered. The electron affinities of the two ions *in the crystal* can thus be com-

[1] References to previous papers are given here and also by Seitz, *Trans. Faraday Soc.*, January 1939.

pared by considering the ionisation potentials of the free atoms. The ionisation potential of thallium is 1·9 volts greater than that of rubidium, but in the crystal, in virtue of the fact that at low thallium concentrations the thallium ion is surrounded by rubidium ions, this difference will probably be diminished. Hence the halogen-thallium transition should lie at about the same wavelength as that for halogen-rubidium. For the rubidium chloride phosphor, it will be seen from Table I that the peak of the A band lies at a much longer wavelength corresponding to a difference of no less than 2·5 volts. Actually, pure thallous halides absorb in the same region as the A, B and C bands appear. There is, however, a complication in the photo dissociation of the halide.

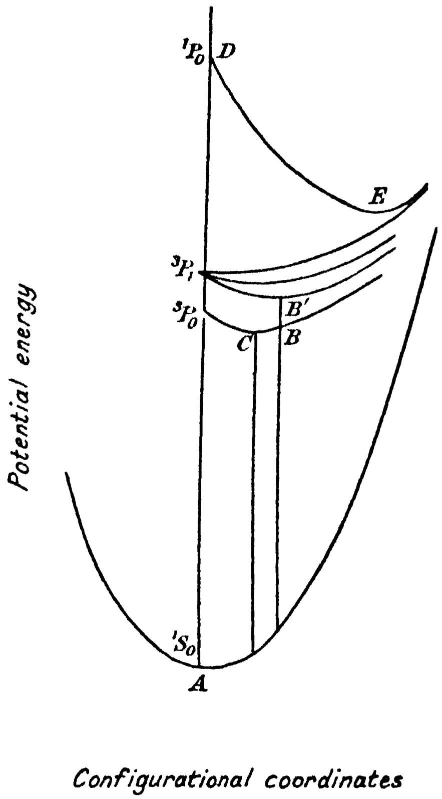


FIG. 2.—Energy level diagram for a thallium ion in an alkali-halide lattice (*after* Seitz).

Assuming that absorption is due to the thallium ion the next question is to account for three absorption bands. Here quantitative explanation is more difficult. In presence of crystalline fields the energy levels of the thallium ion ($6s\ 6p$ state) will be considerably changed. The three peaks may be correlated with the following transitions of the free atom $^1S_0 \rightarrow {}^3P_1$, $^1S_0 \rightarrow {}^3P_2$, $^1S_0 \rightarrow {}^1P_1$, but the absolute positions of the energy levels cannot be obtained, although of course they may be fixed by making use of the experimental data.

In order to explain the occurrence of not more than two bands in fluorescence the energy level diagram will have the form shown in Fig. 2. The lower curve corresponds to the normal state of the crystal while the upper curves show the energy of the excited states as a function of the configurational co-ordinates. In the A band absorption corresponds to excitation to the $^3P_1$ level, which is split

into three levels in the crystal. Consider the lowest of these levels. When a quantum is absorbed of a magnitude sufficient to excite the electron to this level, there will be a vertical transition from the ground state at A. (This would normally give rise to line absorption, but in virtue of the complex lattice vibrations, transitions may occur from an indefinite number of points in the neighbourhood of A, giving rise to a broad band.) As a result of this vertical transition, the excited state possesses considerable vibrational energy, which, as explained above, is soon lost, thus bringing the system to position B^1. Fluorescence then occurs when there is a vertical transition to the ground state. But if it should happen that there is a vibrational level in the crystal lying close to the difference in levels between B and B′ in the diagram, a radiationless transition may take place to the $^3P_0$ level. This latter level is a high vibrational state and therefore soon loses its energy, thereby arriving at point C. Subsequently an optical transition to the ground state gives rise to a second band in the fluorescent spectrum. Next it is necessary to suggest how it is that no matter whether absorption occurs in the A, B or C band, the complete fluorescent spectrum is obtained. Suppose absorption occurs in the C band bringing the system up to point D, then again vibrational energy is lost, the system taking up a position of minimum potential energy at E. Now if this state lies close to any of the lower excited levels, a transition may occur which eventually brings the system to B, thereby allowing the possibility of the emission of fluorescent radiation of the same wavelength as that brought about by absorption in band A.

The phosphorescent phenomena are less easily accounted for. Here luminescence is only obtained if the ion is excited to $^1P_1$ state provided also this is in the neighbourhood of another thallium ion. The presence of this second ion is supposed to give rise to a metastable minimum M as shown in Fig. 3. Phosphorescent light of the same wavelength as that of the fluorescence can then be emitted if sufficient vibrational energy is obtained from the lattice to raise it to point X, whence B may be reached. The higher the temperature of the crystal, the more rapidly will this process occur. On the other hand, absorption of infra-red radiation can also raise the system directly to the unstable level N, again allowing phosphorescence to be produced. In this manner the unimolecular decay law is explained, although *two* thallium ions really participate in the production of a suitable metastable state which virtually is a reservoir for the supply of phosphorescent radiation.

ZINC SULPHIDE PHOSPHORS.—In view of the fact that zinc sulphide phosphors are so important commercially the extensive

investigation of their luminescence is almost bound to supply sufficient data to construct working hypotheses as a basis for further experiment. Pure zinc sulphide, in the crystal forms of wurtzite or of zinc blonde, does not phosphoresce. An intimate mechanical mixture of the two crystalline forms produces a mutual disturbance of the lattice which induces phosphorescence. Likewise the disturbance produced by the sulphides of copper, silver, lead, etc., leads to intense luminescence. In fact, some idea of the rate of diffusion of such atoms or rather ions may be gained from observations on the fluorescence of the zinc sulphide phosphors. For

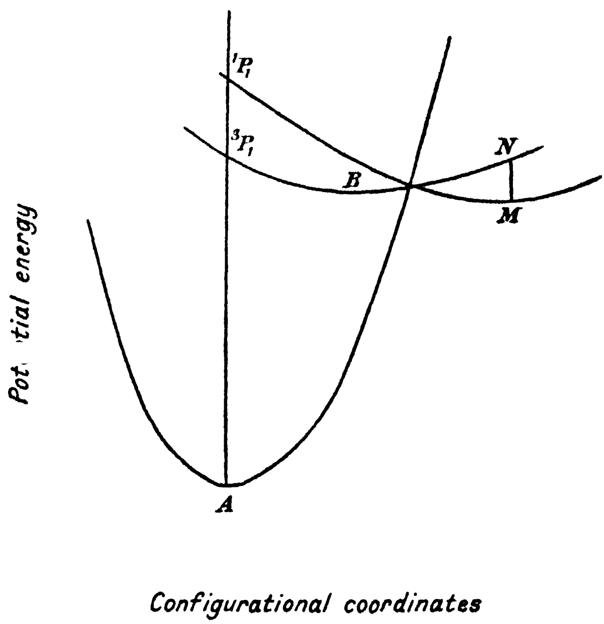


FIG. 3.—Energy level diagram to account for phosphorescence.

example, diffusion of CuS into small crystals of ZnS commences at 330° C. The CuS does not migrate along grain boundaries but into the lattice itself as is shown by removing successive layers of ZnS with hydrochloric acid and measuring the intensity of the luminescence (Tiede, *Ber.*, **65**, 364, 1932 ; Riehl, *Ann. der Physik*, **29**, 654, 1937). Not all metallic sulphides are activators for ZnS. The diameter of the metallic ion determines whether or not it will enter the crystal and therefore whether it will function as an activator for that crystal. For instance, the radioactive isotopes of lead (thorium B) and of bismuth (thorium C) are good activators for CaS and SrS but not for ZnS, for, by employing these indicators, it

can be shown that they do not enter the zinc sulphide lattice (Kading and Riehl, *Angew. Chem.*, **47**, 263, 1934). Thus one simple criterion for activators is at once established. The reason for the high mobility of activators in the ZnS lattice seems to be due to the openness of that lattice, which consists of interlinked tetrahedra with sulphur atoms at the corners, the centre of every other tetrahedron being occupied by a zinc atom. During diffusion therefore the activating atoms migrate from one empty tetrahedron to the next. If the activating atom just fits inside the tetrahedron, diffusion will be comparatively easy, but should the lattice require to be distended before the atom is accommodated the activation energy for diffusion will be so large that migration will not occur.

Unlike the alkali-halide phosphors, the presence of the activator in ZnS does not alter the absorption coefficient for the exciting radiation, whether this is X-rays or ultra-violet light. The absorbing centres are due to the zinc sulphide itself, and the activator atoms therefore provide a path whereby the radiation can be emitted in a number of discrete levels. Moreover the quantum efficiency of the luminescence is very nearly unity, and since the normal concentration of activators is of the order of one in 10^4 the energy absorbed by the ZnS must be capable of transport without appreciable loss to an activator atom which on the average is 20 atomic diameters distant. There are three other significant observations which provide a basis for the explanation of the behaviour of these phosphors. The first is the appearance of photoconductivity. The second is that there is no fluorescence, that is radiation of a decay period of 10^{-8} sec. From the experiments of Riehl (*Ann. der Physik* (5), **29**, 640, 1937), the luminescence appears to be composite, the normal temperature dependent true phosphorescence being accompanied by a temperature independent afterglow. This afterglow can, however, be quenched without affecting the phosphorescence by the addition of small traces of iron, nickel (Levy and West, B.P. 424195, *Trans. Faraday Soc.*, Jan. 1939). Third, in some ZnS phosphors (Reimann, *Nature*, **140**, 501, 1937) the decay of the phosphorescence obeys a second order equation thus indicating the participation of two entities in the radiation process. Photoconductivity undoubtedly demonstrates that one of the particles is an electron, the other must be a positive hole. The impurity in the crystal is thus the point at which electron and positive hole recombine to give up their potential energy as radiation. The function of the impurity then is to provide a discrete energy level in the system lying below that to which the system is initially raised on irradiation and from which reversion to the ground state occurs readily. The

time elapsing between excitation and phosphorescence will therefore depend upon how quickly electron and hole diffuse to the impurity. Since the diffusion coefficient of an electron is estimated to be about 1 $cm.^2$ $sec.^{-1}$ (Gurney and Mott, *Proc. Roy. Soc.*, A, **164**, 152, 1938), that is, about the same as that of a hydrogen molecule in hydrogen gas at atmospheric pressure, and, bearing in mind that the decay period of fluorescence may be a considerable fraction of a second, it is evident that the rate controlling process in any given phosphor will be governed by the rate of diffusion of the positive hole to the impurity.

KAUTSKY'S PHOSPHORS.—Another interesting type of energy transformation in solids has been discovered by Kautsky (*Biochem. Z.*, **291**, 271, 1937). Here the photo-active molecules—chlorophyll, porphyrin, tryptoflavin—are adsorbed on the surface of silica or aluminium oxide gel. When light is absorbed by these dye molecules, the energy so acquired is not transferred to the absorbent but is reradiated as fluorescence and even as phosphorescence, the gel thus functioning as an energy insulator. The decay period of the phosphorescence is strongly temperature dependent—in fact, the radiation may be trapped at low temperatures and subsequently emitted by warming the solid. The remarkable observation is that both fluorescence and phosphorescence are strongly quenched by quite small pressures of oxygen, namely 10^{-4} mm., the phosphorescence being the more sensitive to this specific energy transfer —a sort of collision of the second kind in the solid state. This is of course to be expected since the long life of the excited state of the adsorbed dye gives a much better opportunity for the oxygen to remove excess energy from the dye. In view of the fact that fluorescent radiation lies in the red or infra-red, it is most probable that the oxygen molecule is excited to low-lying metastable states (1Σ, 37·3 k. cal. and 1Δ 22·5, 26·8 and 30·9 k. cal.) rather than to high vibrational levels of the ground state.

In presence of dyes, such as those mentioned above, adsorbed on gels, oxygen may oxidise a variety of substances which act as acceptors. Furthermore, the oxidation process may be made visible by employing leuco-malachite green as an indicator. Kautsky found that if a mixture of gels of both substances and acceptor were made in such a way that the smaller gel particles containing the sensitiser surround those of the acceptor, then on illumination in presence of oxygen the acceptor is coloured green. There is an optimum pressure for the display of this phenomenon (in the experiments quoted 10^{-3} mm.). At very low pressures only a small fraction of the excited dye molecules are deactivated; at

high pressures the metastable oxygen molecules are destroyed by collisions with normal molecules before they can diffuse to the acceptor.

GEOLOGY. By G. W. Tyrrell, A.R.C.Sc., D.Sc., F.R.S.E., The University, Glasgow.

Sedimentation and Sedimentary Rocks.—The *Geologische Rundschau* has published a symposium on sedimentary rocks and sedimentation under the title "Sediment-Heft" (**29**, 1938, 145–461). It contains 23 papers in eight groups, dealing respectively with Fundamentals and Methods; Clays; (Sediments of the) Netherlands, North Sea and Baltic Sea; Deep-sea (Sediments); Continental (Sediments); Ancient Sediments; Unconsolidated Sediments and Soils; and Useful Sediments. A wealth of data for students of sedimentary rocks, impossible to summarise here, is contained in this volume.

The problem of the extraordinary, irregular, downwardly directed cavities in certain granites of Corsica and other localities (called *tafoni*) is dealt with in an exhaustive and profusely illustrated memoir by I. Kvelberg and B. Popoff ("Die Tafoni-Verwitterungserscheinung," *Latvijas Univ. Raksti. Chem. Fac.*, Ser. IV, 6, Riga, 1937, 130–368). On the basis of extensive field and microscopic observations, Popoff and Kvelberg ascribe the origin of tafonis to exfoliation of the upper surfaces of incipient hollows, under the influence of volume changes due to periodically recurring temperature differences, gravity aiding in the removal of the curved sheets split off by the process. Ordinary weathering and chemical changes occasionally take a very subordinate part. The work ends with a classification of hollows and cavities in rocks. The term "tafoni," which has been applied indiscriminately to all kinds of hollows, should be restricted to the Corsican examples and others identical with them.

J. A. Bartrum has studied the well-known "Honeycomb Weathering of Rocks near the Shore-line" (*N.Z. Journ. Sci. and Tech.*, XVIII, 1936, 593–600). The rocks in New Zealand affected by this mode of weathering are homogeneous dacite lavas, tuffs and sandstones. Bartrum suggests that the regular etching is produced by drops of salt spray remaining on flat surfaces long enough for evaporation to take place, with crystallisation of the contained salts. A process of alternate solution and crystallisation might thus go on long enough to disrupt the rock, the resulting fragments being washed away by water, or blown away by wind.

In continuation of his work on the "Rate of Sedimentation in

Salt Marshes on Scolt Head Island, Norfolk," J. A. Steers (*Geol. Mag.*, LXXV, 1938, 26–39) finds that anything up to $2\frac{1}{2}$ cm. of silt was deposited in the 22 months covered by the experiment. While the rate of accretion diminishes as the marsh surface is raised, it is evident that marshes develop rapidly in the geological sense.

An important paper by W. O. Thompson deals with the "Original Structures of Beaches, Bars and Dunes" (*Bull. Geol. Soc. Amer.*, **48**, 1937, 723–52). He describes structural differences between different parts of the beach, illustrated in detail by numerous sections of modern beaches. These structures are compared with those of ancient beaches, and the author concludes that while no single criterion can be decisive, the combination of many, or all, of the structures described, should provide conclusive evidence of the mode of origin of ancient beach formations.

"The Sediments of Barataria Bay" (Mississippi delta) have been thoroughly sampled by W. C. Krumbein and E. Aberdeen (*Journ. Sed. Petr.*, **7**, 1937, 3–17), mechanical analyses made, and statistical constants computed. The areal distribution of the samples in terms of their average grain sizes and their degree of sorting shows that these factors are related to the currents within the bay. It is shown that the study of the size characteristics of sediments yields data for evaluating the physical conditions of the environment under which they are deposited.

W. C. Krumbein and J. S. Griffith have carried out a study of the "Beach Environment in Little Sister Bay, Wisconsin" (*Bull. Geol. Soc. Amer.*, **49**, 1938, 629–52), which shows that it is possible to plot the characters of the sediments on maps in such a way that the essential features of the depositional environment are revealed. Among the characters studied were the mechanical composition of the sediments, shapes of pebbles, and the sizes and shapes of pebbles as a function of position on the beach.

A succeeding paper by W. C. Krumbein on "Local Areal Variation of Beach Sands" (*ibid.*, 653–58) presents data supplementing some of the results of the foregoing investigation. It is shown that there are certain underlying similarities on widely different beaches, which may lead to generalisations valid for beach environments in general.

In a paper on "The Sedimentational Effects of the Work of Marine Scavengers" (*Amer. Journ. Sci.*, XXXVI, 1938, 54–65), E. C. Dapples presents data relating to the influence on sedimentation of the benthonic organisms of the sea floor. These animals ingest such quantities of bottom materials that benthonic life may be considered an active agent in diagenesis. Coprolitic matter may

be important in fine-grained sediments, and some forms of ooliths may be fæcal matter. Fossiliferous strata are considered to be due to sudden annihilation or rapid burial of benthonic life; and, conversely, the view that unfossiliferous rocks are deposited under conditions adverse to life is not necessarily true.

Discussing the "Behaviour of Suspension Currents and Mud Slides on the Continental Slope," H. C. Stetson and J. F. Smith (*Amer. Journ. Sci.*, XXXV, 1938, 1–13) point out that these density currents, to which the origin of submarine canyons has been ascribed by R. A. Daly, will have important effects in sedimentation. When they come to rest the sediment carried by them is distributed over the continental slope and the adjacent ocean floor.

C. S. Piggot describes a new mechanism by which a tube is "fired" into the soft sediment of the ocean floor by means of an explosive charge, and core samples obtained thereby up to 10 feet in length (*Bull. Geol. Soc. Amer.*, **47**, 1936, 675–84; *Smithsonian Report for 1936*, 1937, 207–16; *Sci. Monthly*, XLVI, 1938, 201–17). The study of these cores from the North Atlantic is yielding much new chemical, mineralogical and biological information. One of the most interesting results is that within the 10-feet thickness reached by the cores, there are records of four glacial periods, and two periods characterised by the presence of volcanic ash.

In a suggestive paper "On the Total Amount of Sedimentation in the Deep Sea," P. H. Kuenen (*Amer. Journ. Sci.*, XXXIV, 1937, 457–68) points out that Schott's estimate of the average rate of oceanic sedimentation as 1 cm. in 1000 years, implies a total thickness of 17 km. of sediment on the ocean floors, and therefore a denudation of the continents of 40 km. depth. This, however, is far too large, because the factors of compaction, and the production of igneous rocks, have been neglected. Assuming, therefore, sediments 5–6 km. thick on the ocean floor, Kuenen indicates important repercussions on such problems as the isostasy of the oceans, subsidence of oceanic volcanoes, Wegener's mechanism of continental drift, and the thickness of the continents.

"The Boron Content of Sea Water and of Marine Organisms" has been investigated by I. Igelsrud, J. G. Thompson and B. M. G. Zwicker (*Amer. Journ. Sci.*, XXXV, 1938, 47–63). The results indicate that boron may exist in sea water as soluble complex compounds. Marine plants, and especially animals such as *Conus*, *Cypræa* and certain Hydrocorallinæ, contain such quantities of boron as to suggest that this element may occur quite generally in calcareous and sedimentary structures of oceanic origin.

It is suggested by W. C. Krumbein (*Journ. Geol.*, XLV, 1937,

577–601) that the relations between certain factors in sedimentation can be expressed by curves involving negative exponential functions. Methods of testing such relationships are given for variations of pebble-size on beaches, the thickness of a loess deposit, the profile of an alluvial fan, and the dispersion of boulders in a glacial boulder train. In another paper on the same lines, Krumbein discusses "Korngrössenenteilungen und statistische Analyse" (*N.J. f. Min.* B.-B. **73**, Abt. A, 1937, 137–50).

The determination of the mineral composition of a number of soils, clays and shales from Illinois by R. E. Grim, R. H. Bray and W. F. Bradley (*Amer. Mineralogist*, **22**, 1937, 813–29) has revealed the existence of a mineral belonging to the mica group similar to muscovite, which was previously referred to as the "sericite-like" mineral. By the use of super-centrifuge fractionation technique this mineral has been isolated in a relatively pure state, and has been found to be different from muscovite or sericite, or any other named mica mineral. It has therefore been given the general name *illite* (after the state of Illinois).

"A Sedimentary and Petrographic Study of Certain Glacial Drifts of Minnesota," by F. C. Kruger (*Amer. Journ. Sci.*, XXXIV, 1937, 345–63), indicates that the Pleistocene tills show differences in their heavy accessory minerals which may be used for purposes of correlation. The investigation shows that there has been little post-glacial weathering, and the drifts have a mineral content similar to that of freshly-crushed igneous rocks.

"Two concepts dealing with the character of the differential contamination of sedimentary detritus in transit, and with the retention upon contamination of certain original significant mineral frequencies, can be formulated from the analysis of the behaviour of sedimentary detritus as it is carried through ordinary or glacial streams." The application of these concepts by P. D. Krynine in his paper on "Glacial Sedimentology of the Quinnipiac-Pequabuck Lowland in Southern Connecticut" (*Amer. Journ. Sci.*, XXXIII, 1937, 111–39), "resulted in the differentiation of at least three generations of late-glacial sands, in the identification of their source areas, and in the reconstruction of the drainage and sedimentary conditions under which these deposits were formed."

In his paper on "Unconsolidated Sediments and Topographic Features of the Lower Yukon Valley," A. J. Eardley (*Bull. Geol. Soc. Amer.*, **49**, 1938, 303–42) fully describes the petrographical characters and stratigraphical relations of the alluvial deposits of the Yukon River, consisting of silts, sands, gravels, "mucks" and peat, all comprised under the inclusive term "Yukon Silts." A loam of

tan colour, of uniform mineral and mechanical composition, is the most widespread of these deposits. The " tan loam " is considered to be mainly the deposit of aggrading streams, although at higher levels it may represent loess.

In a valuable paper on the " Mineral Composition of Mississippi River Sands," R. D. Russell (*Bull. Geol. Soc. Amer.*, **48**, 1937, 1307–48) challenges the widely accepted hypothesis that minerals such as the pyroxenes, amphiboles and feldspars are rapidly and progressively eliminated during transport. In the 1100-miles journey from Cairo to the Gulf of Mexico, it was found that feldspars decreased from about 25 to 20 per cent. of the 100-mesh grade, while the pyroxenes and amphiboles showed little or no progressive loss. The " persistent " detrital minerals appear to be those most resistant to chemical processes of decay, mechanical destruction being of minor importance.

Eighteen samples of " The Shore Sands of Cornwall and Devon from Land's End to the Taw-Torridge Estuary " have been investigated by A. Stuart and B. Simpson (*Trans. Roy. Geol. Soc. Cornwall*, XVII, Pt. I, 1937, 13–40). About 60 minerals are recorded. The sands show good correlation with local sources, but southwards the absence or great rarity of kyanite and staurolite shows that Pliocene material does not bulk largely in the modern sands. Glacial material has supplied an influx of rarer and non-local minerals towards the north.

" The Lower Bunter Sandstones of North Worcestershire and East Shropshire," investigated by F. W. Shotton and colleagues (*Geol. Mag.*, LXXIV, 1937, 534–53), consist of sand the average grain size of which is 0·2 mm., with restricted limits of size variation. These facts, especially the high perfection of sorting, indicate an æolian origin. The abundant false bedding also, is only explicable on the same hypothesis.

In a paper on " Shape-Sorting of Sand Grains by Wind Action," G. R. MacCarthy and J. W. Huddle (*Amer. Journ. Sci.*, XXXV, 1938, 64–73) present experimental evidence showing that much of the superior roundness of æolian sands, as compared with sands of other types, is due to a process called shape-sorting. The experiments indicate that wind transport carries grains chiefly by saltation, and that this mode of transport favours the rounder grains.

The presence of small amounts of clastic material, the occurrence along with pure graptolite shales, the presence of fossil radiolarian genera to-day characterised by deep-water habitat, and the occurrence of zones of radiolarite, indicate a deep-sea origin of the " Eastern New York Ordovician Cherts " according to the work

of R. Ruedemann and T. Y. Wilson (*Bull. Geol. Soc. Amer.*, **47**, 1936, 1535–86). The Ghent radiolarite probably represents a radiolarian ooze. The base of the Appalachian geosyncline is thus considered to have reached abyssal depths.

A symposium in English on "Geological Investigations of Agricultural Ores, U.S.S.R." (*sic*) has been issued to members of the 17th International Geological Congress, Moscow, 1937 (*Trans. Sci. Inst. of Fertilisers and Insecto-Fungicides, U.S.S.R.*, No. 142, 1937, 134 pp.). It contains ten papers by different authors on Soviet phosphate deposits, one on datolite deposits, and a general paper by A. V. Kazakov on "The Phosphorite Facies and the Origin of Phosphorites." All of these papers but one by M. P. Fiveg on "The Apatite Deposits of the Khibinian Tundras," a deposit of igneous origin, deal with sedimentary phosphates. Kazakov's paper discusses the principal factors in the formation of phosphates, and outlines a new theory of origin of phosphorite deposits as typical, chemical, salt-water sediments, depending on the activity of bottom currents in the ocean.

Discussing "The Genesis of the Jotnian Sediments," H. von Eckermann (*Geol. För. Förh. Stockholm*, **59**, 1937, 548–77) is doubtful whether continental sedimentation of fluviatile and desert origin, and the formation in some parts of deeply weathered arkosic residual deposits under arid conditions, is the full story. He is inclined to postulate, in addition, a vast tidal action which he admits, however, cannot be paralleled anywhere on the existing surface of the earth.

In their important memoir "Belt Series of the North: Stratigraphy, Sedimentation, Palæontology," C. L. and M. A. Fenton (*Bull. Geol. Soc. Amer.*, **48**, 1937, 1873–1970) describe a large number of sedimentary structures. Much of the Belt formation has been considered to be of fluviatile origin; but the authors show that the formation is unified areally and vertically; several of its members are admittedly marine; and others differ from known marine formation only in their scarcity of fossils. With minor exceptions, the series is therefore believed to be of marine origin.

In his paper "Dating Cretaceous-Eocene Tectonic Movements in Big Horn Basin by Heavy Minerals," M. H. Stow (*Bull. Geol. Soc. Amer.*, **49**, 1938, 731–62) shows that the minerals characterising the continental basin-fill sediments of the Big Horn Basin occur in zones that accord well with the zones previously established on stratigraphical and palæontological grounds, and can thus be used to determine horizons in unfossiliferous sediments of the area in question. In particular, the heavy minerals help to date the uplifts of the basin rims.

The "Petrography and Genesis of the Siwalik Series" has been studied by P. D. Krynine (*Amer. Journ. Sci.*, XXXIV, 1937, 422–46). This immense accumulation of Late Tertiary sediments on the flanks of the Himalaya, 20,000 feet in thickness, is highly fossiliferous, and consists of three main types: 1, sandstones and greywackes, called "schist-arenites" because of their 40 per cent. content of fragments of metamorphic rocks; 2, red siltstones and shales; and 3, intraformational conglomerates made up of siltstone fragments in a matrix of schist-arenite. The facts indicate deposition on a broad flat floodplain, under climatic conditions ranging from tropical-humid to arid, finally reverting to temperate-humid in the later stages of sedimentation.

BOTANY. By Professor E. J. Salisbury, D.Sc., F.R.S., University College, London.

P. H. Synge in an account of an expedition to the East African equatorial mountains (*Jour. R.H.S.*, LXIII, 458, 1938) gives a useful summary of the vegetation zonation which they exhibit. Mixed evergreen rain-forest extends from 6000 ft. to 7500 ft., in which species of Cyathea and *Musa Ensete* are conspicuous features of the undergrowth. This is followed by bamboo forest from 7500 ft. to 10,000 ft., which attains a height of about 50 ft. at its lower altitudinal limit, diminishing to from 15 ft. to 20 ft. towards the upper margin. There is an abrupt change from the bamboo forest to Ericetum above 10,000 ft., where on a very acid peaty soil a scrub some 40–50 ft. high is developed, in which conspicuous species are *Erica arborea* and *Philippia Johnstonii*, accompanied by arboreal Senecios and arboreal Lobelias. This scrub extends up to 13,000 ft.

In the more open drier areas of this scrub zone *Lobelia Wollastonii* is common, whilst the swampy clearings are characterised by tussocky Carex with *L. Bequærtii*. Both tree Senecios and tree Lobelias extend beyond the heather scrub up to 14,500 ft. *L. giberrosa* is altitudinally the lowest of the tree Lobelias on these mountains, and also the largest. It occurs on all the mountains from 6500 ft. to 10,000 ft., and, though normally not more than 15–20 ft. in height, may attain to 29 ft. *L. Aberdarica* occurs at 9500 ft. on Mt. Elgon, on the Aberdare Mountains, and on Mt. Kenya. *L. Elgonensis* is found on Mt. Elgon from 11,000 ft. to 13,500 ft., and is paralleled by *L. sattimæ* on the Aberdare Mountains, and *L. keniensis* on Mt. Kenya. *L. Bequærtii* on Ruwenzori and *L. Telekii* on Mt. Elgon, Aberdare Mountains, and Mt. Kenya both range from 11,000 ft. to 14,000 ft. *L. Wollastonii* occurs

on Mt. Ruwenzori and the Virunga Mountains between 11,000 ft. and 14,700 ft.

Kund, Jessen and A. Farrington give data respecting the flora of some bogs near Dublin (*Proc. Roy. Irish Acad.*, XLIV, 205–60, 1938). In deposits attributed to late glacial times, remains of *Arenaria ciliata*, *Oxyria digyna* and *Thalictrum alpinum* are recorded and also, though this identification is not certain, *Arabis petræa*. All these northern species are now absent from Eastern Ireland. Other species recorded are *Salix herbacea*, *Saxifraga hypnoides* and *Armeria vulgaris*. The flora thus had an Arctic or sub-Arctic facies.

In the *Annual Report* of the East Malling Research Station for 1937 (pp. 117–27), H. M. Tydeman records the results of experiments to test the efficacy of various apple and pear varieties as pollinators. Striking differences were observed when Devonshire Quarrenden was pollinated by White Transparent and by Worcester Pearmain. With the former 83·5 per cent. fruits were set, and of these about 15 per cent. attained maturity. With Worcester Pearmain as the pollinating agent only 52·6 per cent. fruits were set and only 4·3 per cent. attained maturity. Similarly, Stirling Castle pollinated by Cox's Orange Pippin yielded about eight times the amount of mature fruit produced when the pollinating parent was Laxton's Superb. Similar differences were found to obtain with varieties of pears.

That the mode of germination which a species exhibits may effect its occurrence in a particular climatic area is evident. In this connection some germination experiments carried out on various species of *Pinus* at Lingnan University are of interest. Under the field conditions obtaining in Southern China rapid germination of tree seed is apparently highly important for seedling survival, and tests upon the germination of the native *Pinus Massoniana* showed that over 70 per cent. of the seeds germinate in fifteen days after sowing, and that within twenty days the percentage is over 85. Seeds of three southern species of *Pinus* from America were tested under the same conditions, and of these *P. caribæa* yielded 9·2 per cent. germination in twenty days ; *P. echinata* 6·0 per cent. and *P. tæda* only 1·6 per cent. in the same period. After fifty days the percentage germinations of these three species were respectively 55 per cent., 64·6 per cent. and 42·7 per cent. (*Lingnan Sci. Jour.*, **16,** 4, pp. 573–78, 1937).

From experiments with oats and barley grown in the presence and in the absence of weeds, G. L. Blackman and W. G. Templeman furnish evidence that the competition is principally for nitrogen. Barley grown in four different localities in the absence of weeds contained from 1·51 per cent. to 2·8 per cent. of their dry weight of

nitrogen. When weeds were present the barley in the same areas contained only from 1·14 per cent. to 1·68 per cent. of the dry weight of nitrogen. Similarly the dry weight of oats in the absence of weeds contained 1·71–2·33 per cent. of nitrogen and 1·03–1·33 per cent. when weeds were present (*Jour. Agric. Sci.*, XXVIII, pp. 247–71, 1938).

The slow rate of decay of carbohydrates in the soil as compared with nitrogenous compounds and the delaying effect of a high carbohydrate-nitrogen ratio in plant material upon the rate of decay of plant material has been the subject of study by M. F. Spaulding and W. S. Eisenmenger (*Soil Science*, **45**, 427–45, 1938). Plant material of forty-five species was experimented with and a high degree of positive correlation was found to obtain between the rate of decomposition and the total nitrogen content. Finely ground plant material was incorporated in soil and sand, and the effects on the growth of barley, both when lime was added and in its absence, were studied. With most plant materials employed the yield was appreciably greater when lime was added to the soil, but this was notably not the case when the plant material was that of *Bidens* or *Solanum lycopersicum*. Material of such plants as tobacco and potato-tops decay very rapidly, whilst monocotyledons such as rushes and sedges decay at a tenth of the rate or even less. It may be pointed out that such differential behaviour of plant material needs to be taken into account in relation to plant succession, especially having regard to the marked change in reaction that may accompany the various phases of decomposition.

Numerous investigations have shown the great ecological importance of soil reaction both in respect to its direct influence and its indirect effects upon plant growth. The question whether the water content of the soil affects the soil reaction is therefore a matter of some importance. A. N. Puri and A. G. Asghar (*Soil Science*, **46**, 249–58, 1938) have carried out experiments which show that, employing single-base soils, the effect of changing the soil-water ratio from 1 : 5 to 1 : 25 has no significant effect whether the *p*H be low or high. This was found to be true alike for soils in which the single base was sodium, potassium or calcium. On the other hand, the presence of even small amounts of neutral salts had a marked effect upon the soil reaction, and, since this effect varies with the concentration of the salts, it is evident that the soil-water ratio will affect the reaction of natural soils. C. M. Keaton in the same journal (**46**, 259–65) gives data respecting *p*H values of various natural soil types over a range of soil-water ratios and at normal moisture-content. In acid soils the minimum *p*H value was found

when the soil-water ratio corresponds to the water-holding capacity of the soil, but at lower water-contents the *p*H value increased, a phenomenon attributed to preferential dissociation. In soils of a more alkaline character a continual decrease in *p*H value takes place with diminishing water-content. A soil which at normal water-content had a reaction of *p*H 6·71 showed a reaction of *p*H 8·42 when the soil-water ratio was 1 : 5.

The sand-dune areas of South Australia, where sheep suffer from the Coast Disease, consist of calcareous sand of which the top foot contains about 64 per cent. of calcium carbonate and is greyish in colour owing to the presence of a small amount of humus. The reaction is here *p*H 8·5. Below the first foot the soil becomes yellowish, contains between 71 per cent. and 74 per cent. of calcium carbonate, and the reaction increases in alkalinity to *p*H 9·2. The chief herbage species are *Bromus madritensis* and *Lagurus ovatus* with stunted *Melilotus indica*. On ungrazed areas *Swainsonia lessertifolia* is typical. Field trials in which twenty-five species of pasture plants were sown (10 spp. of grasses, 11 spp. Legumes) resulted in failure of most species to survive beyond the seedling stage. Treatments with various artificial manures showed little response to applications of the major elements. Further trials with other treatments indicated that deficiency of copper was the chief limiting factor. The deficiency disease shown by cereals on these calcareous dunes is regarded as identical with the copper deficiency disease in Europe, known as reclamation disease, recorded from marshy, swampy, sandy and moorland soils in Germany, Holland, Norway and Denmark, where the soil reaction is often highly acid and thus in marked contrast with the Australian dunes (cf. D. S. Riceman and C. M. Donald, *Australian Council Sci. Industr. Res.*, Bull. 78, 1938).

Writing of the Zygnemales of Northern India, which constitute the dominant algal vegetation of the ponds, M. S. Randhawa recognises three seasonal groups, namely : (1) Late autumn and early winter annuals whose spores germinate from the end of August to mid-September, after the Monsoon rains, and form their zygospores in October and November. Fourteen species are placed in this category belonging to the genera *Debarya*, *Mougeotia*, *Zygnema*, *Spirogyra* and *Sirogonium*. (2) Late winter and spring annuals which germinate in late November or early December and ripen their spores in February and March or even April. This group comprises forty-seven species belonging to the genera *Mougeotia*, *Zygnemopsis* (9), *Zygnema* (8), *Spirogyra* (23) and *Sirogonium*. (3) This group comprises two species only, *Mougeotia sphærocarpa*

and *Spirogyra dædalea*, both of which complete their life history in a few weeks and may ripen their spores at almost any season (*Proc. Indian Acad. Science*, VIII, 1938).

The genus Botrydium is the subject of a paper by W. Vischer in *Ber. der Schweizerischen Botanischen Gesell.*, **48**, 538, 1938. A variety of *Botrydium granulosum* is described under the name *v. Kolkwitzianum*, which differs from the normal type of the alga on drying mud in having sympodially branched rhizoids. A new species is described under the name *B. cytosum* which has pyriform bladders and the spores are liberated through an apical pore. A second species is distinguished as *B. Bechererianum* in which the rhizoids are long and tenuous and the spores also liberated through a pore.

The Mucorales in soils of various types in Scotland have been studied by Dr. Marie Campbell. Sixteen samples were examined, obtained from seven different types of habitat. The total number of species identified was twenty-three, of which thirteen have not previously been isolated from soils in this country. Despite the wide range of soil types, including sand-dunes, peat-bogs, woodlands and salt marshes, and ranging in reaction from *p*H 4 (heather moor) to *p*H 8 (cultivated soil), there was little evidence of any specific association with particular soil types. The greatest number of species were found in the sand-dune soils (9) and cultivated soils (10). A point of some interest is that the positive forms of the Mucorales, which experiment shows have mycelia more resistant to desiccation than the negative forms, are those which tend to be more widely distributed (*Trans. Roy. Soc. Edinburgh*, LIX, 411–36, 1938).

ENTOMOLOGY. By H. F. Barnes, M.A., Ph.D., Rothamsted Experimental Station, Harpenden.

General Entomology.—The evolution of the Annelida, Onychophora and Arthropoda by R. E. Snodgrass (*Smithsonian Misc. Coll.*, **97**, No. 6, 1938, 159 pp.) is an outstanding contribution of recent date.

A. D. Hopkins (*U.S. Dept. Agric.*, Misc. Pub. 280, 1938, 188 pp.) in this exceedingly useful bulletin deals with bioclimatics which is the science of life and climate relations. In Part 1 the laws, principles, systems and methods of application are considered. In this section one finds summaries of the more important results and conclusions of applied bioclimatics to various problems in economic entomology. In Part 2, time, seasons, zones and zonal types receive attention.

The theory of cold-hardiness in insects up to the present has been mainly based on the purely physical relationship of water and protoplasmic colloids (proteins). I. W. Kozhantshikou (*Bull. Ent. Res.*, **29**, 1938, 253–62) has studied the effect of temperatures below zero and also that of narcotics on the respiration. He concludes that cold-hardiness in insects depends on the physiological state; thus the most resistant are the phases in diapause, less hardy are certain caterpillars stopped in their development, and developing or growing insects are practically non-cold-hardy. The difference in the cold-hardiness depends on the specificity of their cellular respiration. Growing insects show in their cellular respiration the prevalence of oxydases which act in connection with the structural elements of the cells. In cold-hardy insects the cellular respiration is closely connected with the anoxybiotic processes caused by the dehydrases, which are closely associated with the presence of non-saturated fatty-acids. The respiration of growing or developing insects is rapidly destroyed by narcotics, cyanide and low temperatures since they destroy the cellular structures. In cold-hardy insects there is a thermostable part of the respiration which is also resistant to narcotics. Destruction of the cellular structure does not affect this respiration. Cold-hardiness increases with the increase of the percentage of thermostable respiration. Freezing of the protoplasmic water only causes the death of an insect when there is no thermostable respiration. Finally the quality, not quantity, of the fats is probably the important factor.

The relationship between insects and plant diseases was the subject of a recent symposium held by the American Association of Economic Entomologists (*J. Econ. Ent.*, **31**, 1938, 11–44). Insects in relation to diseases of truck crops were dealt with by A. A. Granovsky, those of trees and small fruits by L. O. Kunkel, those of shade and forest trees by J. G. Leach and those of cereals and forage crops by F. W. Poos. N. E. Stevens described the problems involved in the control of plant diseases and insects. Lists of literature cited are given by Granovsky and Poos.

The use of statistical methods in entomology are illustrated by a paper by G. Beall (*Canad. J. Res.*, D. 16, 1938, 39–71) in which he gives a detailed analysis of fluctuations in the activity of the European Corn Borer. He has found that on a given evening the variation in number of moths over an area is distributed with the standard deviation of these chance variations equal to the square root of the observation. The use of this finding is to enable one to determine whether observations made on successive nights differ significantly when the seasonal trend is neglected. This would

enable one to use data from surveys and traps. This investigator has shown also that the importance of weather during a season can be estimated and calculations made as to the probable success in the extension of an insect's geographical range. This was done by an examination of the relation between the flight of the moth and temperature. The correspondence between observed and estimated nightly magnitude of flight indicated that an estimate of flight over a small area is inexact on account of chance variation.

L. O. T. Mendes (*Rev. Ent.*, **8**, 1938, 89–92) has constructed a formula for determining the number of instars of insect larvæ in cases when only limited data are available. It is $n = \dfrac{\log z - \log a}{\log z - \log y} + 1$, where n = number of instars and z, y, and a, are the measurements of the chitinous mandibles, for the last, penultimate and first instars respectively. It is thought that this formula will be useful in determining the number of instars of endoparasitic larvæ.

A. J. Nicholson (*J. Counc. Sci. and Indust. Res.* (*Australia*), **10**, 1937, 101–6) has stressed the rôle of competition in determining animal populations. In fact, he claims that competition in a somewhat wide sense is the only factor that can regulate the population densities of animals.

The influence of Japanese beetle instars on the sex and population of the parasite *Tiphia popilliavora* has been studied by M. H. Brunson (*J. Agric. Res.*, **57**, 1938, 379–86). Apparently the sex of the progeny is determined at the time the egg is placed on the host. Parasites from second instar larvæ were predominantly males and those from third instar larvæ predominantly females. The variation in the relative number of host larvæ that are in the third instar and also in the total host population at different colony points in the area heavily infested with the beetle tends to cause a variation in the parasite population at different colony points. The scarcity of third instar host larvæ during the period of greatest parasite activity also reduces the effectiveness of the parasite regardless of the total population of host larvæ present.

The third part of F. X. William's biological studies in Hawaiian water-loving insects deals with Diptera belonging to the families Ephydridæ and Anthomyidæ (*Proc. Hawaiian Ent. Soc.*, **10**, 1938, 85–119). The first two parts dealing respectively with beetles and dragonflies appeared in 1936 (*loc. cit.*, **9**, 1936, 235–49).

Orthoptera.—M. Hussein has studied experimentally the effect of temperature on locust activity (*Min. Agric. Egypt. Tech. and Sci. Serv.*, Bull. 184, 1937, 55 pp.). The experiments have

proved that the behaviour of locusts is controlled by the temperature of the environment acting through their body temperature. The larger-sized locusts always had higher temperature limits to various activity stages when placed in a rising temperature. Similarly when placed in a falling temperature the smaller insects ceased activity first. Again, it took longer to revive the larger insects when the locusts were cooled down to inactivity and then warmed up to activity. Hussein's investigation has also shown that mass movements of hoppers and flights of adults are caused by excessive temperatures and last only so long as external factors are unfavourable. The three species studied show specific differences in their temperature reactions.

K. H. L. Key (*Counc. Sci. and Indust. Res.* (*Australia*), Bull. 117, 1938, 87 pp.) has brought together all that is known about grasshopper plagues in Australia, paying special attention to seasonal and geographical distribution as well as correct identification.

COLEOPTERA.—I. W. Williams (*J. N. Y. Ent. Soc.*, **46**, 1938, 245–89) has studied the labia and maxillæ of representatives of most of the coleopterous families. This worker agrees with F. S. Stickney (*Ill. Bio. Mono.*, **8**, 1923, 1–104), who, after examining the head capsule, arranged the superfamilies into two principal groups. The first includes the Caraboidea, Gyrinoidea, Hydrophiloidea, Silphoidea, Staphylinoidea, Cantharoidea (in part) and Scarabæoidea. The remaining superfamilies fall into the second group with the exception of the Cerambycoidea, Brentoidea, Curculionoidea and Scolytoidea which are difficult to place in either group because of their specialisations.

Two important papers on the taxonomy of some beetle larvæ have recently been published. One is on the Chrysomelinæ by W. Henning (*Arb. physiol. angew. Ent.*, **5**, 1938, 85–135) and the other is on the Rhynchophora by F. v. Emden (*Trans. R. Ent. Soc. Lond.*, **87**, 1938, 1–37).

G. B. Walsh (*Trans. Soc. Brit. Ent.*, **5**, 1938, 199–222) has given the results of some experiments designed to test the hypothesis that there is some adaptive relation between beetles and the water content of their environment. He has found among other conclusions that the rate of loss of water is dependent on the size of the elytra. Thus the Brachelytra show little resistance to drought conditions and are nearly always found in moist environments. Again beetles in which the elytra are in a close fixed position to the body have a slow rate of loss of water, *e.g.* sandhill beetles.

Of recent years the view has been held that the nervous system exerts its control of rhythmic flashing of the firefly by acting on the

tracheal end-cells. N. S. R. Malœuf (*Ann. Ent. Soc. Amer.*, **31**, 1938, 374–80) has brought forward evidence to show that this tracheal end-cell theory is untenable. He has shown that the flashing is the result of the rising and falling of the osmotic pressure in the photogenic cells under spontaneous cerebral control. The four theories, " accidental " " illusion," " sympathy " and " leader," of synchronous flashing have been analysed by J. B. Buck (*Qtly. Rev. Biol.*, **13**, 1938, 301–14). He is inclined to think that a modified " leader " theory in general is most satisfactory.

Several important papers on the Colorado beetle have recently been published in the *Annales des Épiphyties et de Phytogénétique* (4, 1938). The first, by J. Feytaud (*loc. cit.*, 29–94), deals with the acclimatisation in Europe of American insect enemies of this beetle. They include Tachinids of the genus *Doryphorophaga*, the Carabid *Lebia grandis* and the Asopids *Perillus bioculatus* and *Podisus maculiventris*. Another paper is by A. Couturier (*loc. cit.*, 95–166), who has made a detailed biological study of *P. maculiventris* under French conditions. He has found that the principal obstacle to its acclimatisation is the relative dryness of the summer. Precipitation should be in the neighbourhood of 100 mm. during the hottest months. While such conditions exist in eastern France, Germany and Poland, the dryness of the Mediterranean regions will limit the insect's progress in a southern direction.

LEPIDOPTERA.—The available information on the genetics of sex in Lepidoptera is summarised in a review article by E. A. Cockayne (*Biol. Rev.*, **13**, 1938, 107–32).

The origin and development of entomophagy among Lepidoptera has been discussed by W. V. Balduf (*Amer. Nat.*, **72**, 1938, 358–79). Four types are involved—cannibalism, occasional predatism, habitual predatism and parasitism.

A study of the effect of the sting of the Braconid, *Microbracon hebetor* on the respiratory rate and quotient of *Ephestia kühniella* larvæ by Nellie M. Payne (*Biol. Bull.*, **73**, 1937, 147–54) has shown that, while the respiratory rate is greatly lowered, the quotient is not altered.

HEMIPTERA.—J. W. Evans (*Papers and Proc. R. Soc. Tasmania 1937*, 1938, 1–20), after studying the morphology of the head of Homoptera, has compared the phylogenetic relationships of the various groups. The Peloridiidæ are apparently derived from the lowest level of the protohymenopterous branch, followed by the Fulgoroidea. The latter resemble *Hemiodoecus*, a Peloridiid, both in the structure of the head, the primitive type of genitalia in which no sub-genital plates are developed and their tegminal

venation. Next come the Cicadidæ, followed by the Cercopidæ. The Jassoidea and Membracidæ are highest on account of their reduced tentorium which is believed to be a recent development brought about by the progressive backward migration of the dilator muscles of the sucking-pump on the cranial walls.

H. C. James previously (*see* SCIENCE PROGRESS, No. 127, 1938, 547) showed that in *Pseudococcus citri* there was a higher proportion of males in families derived from over-aged ova than in families resulting from eggs fertilised at the normal time. Now he has carried out experiments (*Proc. R. Ent. Soc. Lond.*, A, **13**, 1938, 73–9) which show that, while drier environments favour the production of females, the differences observed are not sufficiently large to suggest that the humidity of the environment is the main factor involved in the abnormal sex ratios derived from over-aged eggs.

The first contribution by D. H. R. Lambers towards a monograph of the Aphididae of Europe (*Temminckia*, **3**, 1938, 1–44) deals with the genus *Macrosiphoniella* the majority of whose species live on *Artemisia* spp.

A. C. Evans (*Ann. Appl. Biol.*, **25**, 1938, 558–72) has shown that under late summer conditions of light the rate of reproduction of *Brevicoryne brassicæ* is positively correlated with the nitrogen content of the host plant and in particular with the protein content. The development of winged forms is negatively correlated with the protein content of the plant. He has also shown that the chemical composition of the plant affects the rate of growth, length of larval period and final pupal weights of *Pieris brassicæ*.

HYMENOPTERA.—Host specificity by parasites was pointed out by G. Salt in 1935 (*see* SCIENCE PROGRESS, No. 119, 1935, 507) to depend on three processes, *viz.*, host finding, host selection and host suitability. At the same time he discussed host selection. Later Joyce Laing (*see* SCIENCE PROGRESS, No. 129, 1938, 121–2) investigated host finding. A second paper on this subject by this worker has now appeared (*J. Expt. Biol.*, **15**, 1938, 281–302). In the previous paper the way in which the parasite finds its host was described. Now the results are given of quantitative experiments made to discover the effect of variation in the size of hosts and the distance between them upon their chance of being found. G. Salt (*Bull. Ent. Res.*, **29**, 1938, 223–46) has discussed some experiments on host suitability. Even if the host is found and is accepted the attack may be thwarted because the host is unsuitable for the adult parasite. In such cases the host may escape attack, it may resist the attack actively or passively or it may inhibit oviposition. On the other hand the parasitism may be unsuccessful because the

host is unsuitable for the developing parasite. The host may be physically, chemically or biologically unsuitable. Instances of physical drawbacks are size, texture, unfavourable humidity for pupation and covering too thick for emergence. The host may be unsuitable chemically as food. Various biological reasons for unsuitability include the death, movement and moulting of the host, also its phygocytic reactions as well as the interference of other parasites. Salt also discusses the whole subject of host specificity in the light of recent research.

DIPTERA.—A new type of temperature gradient apparatus has been described by R. C. M. Thomson (*Bull. Ent. Res.*, **29**, 1938, 125–40) in a paper on the reaction of mosquitoes to temperature and humidity. For the humidity reactions the alternative chamber described by D. L. Gunn and J. S. Kennedy (*see* SCIENCE PROGRESS, No. 123, 1937, 521) was used. The new temperature gradient apparatus is based on the same principle.

In some notes concerning what may be an acalyptrate Muscid parasite on Mycetophilid larvæ, W. R. Thompson (*Parasitology*, **30**, 1938, 176–80) gives a summary of dipterous parasites of Diptera. Such occurrences are comparatively rare, with the exception of the parasitism of larval Tipulids by several species of Tachinids including *Bucentes* spp.

A key to the Hawaiian Drosophililæ has been made by E. H. Bryan, Jr. (*Proc. Hawaiian Ent. Soc.*, **10**, 1938, 25–42). This includes notes on the characters of chief taxonomic importance in this group. Up to date 64 described species belonging to seven or eight genera have been recorded from Hawaii.

E. T. Burtt in 1937 (*see* SCIENCE PROGRESS, No. 128, 1938, 757) suggested that Weismann's ring in the larvæ of *Calliphora* represented the modified corpora allata. This finding was based on morphological and histological evidence. He also suggested that this organ was responsible for pupation. It has been previously found by Fraenkel (1935) that pupation was induced by a hormone which was secreted in the region of the central nervous system. Hadorn (*Proc. Nat. Acad. Sci., Wash.*, **23**, 1937, 478) came to the same conclusion as Burtt after studying *Drosophila melanogaster*. He transplanted rings from larvæ ready to pupate to younger larvæ and found an acceleration effect. Now E. T. Burtt (*Proc. R. Soc. Lond.*, B., **126**, 1938, 210–23) has done some experiments destroying the ring in larval *Calliphora*. The result was that pupation was prevented. It is concluded that Weismann's ring secretes the pupating hormone.

OTHER ORDERS.—Hitherto the available information concerning

the biology of the Strepsiptera has been practically nil with the exception of Nassonov's studies (1892–3). Now however a detailed account of the bionomics and host relations of *Corioxenos antestiæ*, which is an endoparasite of the "variegated coffee-bug" *Antestia* spp., has been given by T. W. Kirkpatrick (*Trans. R. Ent. Soc. Lond.*, **86**, 1937, 247–343). This investigator recently dealt with the colour vision of the former insect's triungulin larvæ (*see* SCIENCE PROGRESS, No. 128, 1938, 758). The internal anatomy of *C. antestiæ* has been described by B. Cooper (*Proc. R. Ent. Soc. Lond.*, A, **13**, 1938, 31–54). These papers are valuable contributions to our knowledge of the Strepsiptera.

ARCHÆOLOGY. By E. N. FALLAIZE, B.A.

AMONG recent announcements of discoveries relating to early man, by far the most spectacular are those of the discovery of fragments of an additional skull of *Pithecanthropus erectus* in Java, which not only demonstrate the variability in form within the characters of the type of this early member of the genus *Homo*, but, on the testimony of Dr. F. Weidenreich, further emphasise relationship with Peking man (see *Nature*, Oct. 15, 1938, 715); and the records by Dr. R. Broom of discoveries in South Africa of the remains of fossil anthropoids, which not only come closer to the line of descent of early man than any previous discoveries, but also afford evidence, apparently, of the assumption of the erect posture in modifications of the humerus and the bones of the toe, making them almost indistinguishable from the human (see *Nature*, November 19, 1938, 897). These discoveries, however, belong more properly to the study of the physical characters of early man, and bear only indirectly on the development of his culture.

EARLY MAN IN THE MALAY PENINSULA.—A discovery of more directly cultural bearing is the announcement of the discovery for the first time in the Malay Peninsula of stone artefacts in a geological context, which can be assigned definitely to a Pleistocene horizon. The announcement is made by Mr. H. D. Collings in *Nature* of September 24, 1938, 575–6. The discovery was made on the banks of the Perak River, about 3 miles south of Lenggong, Upper Perak, just below the 250-ft. contour line, in what is probably an old terrace of the Perak River. Here a deposit of volcanic tuff overlies a bed of sand and gravel resting on laterite. In this and two other nearby gravel-beds were a number of stone tools, the majority made from pebbles, and comprising choppers and hand-axes, the flaking being of a minimum; also flakes worked up into scrapers of various types and pebble hammer-stones. The place

evidently was a workshop, and perhaps, it is suggested, a dwelling-site. The culture seems to present affinities with the "Pajitan" of Central Java.

EARLY MAN IN SOUTHERN ARABIA.—Another investigation of considerable interest for quaternary prehistory is that of the expedition to the Hadramaut in Southern Arabia made by Miss G. Caton-Thompson, Miss Elinor W. Gardner, and Miss Freya Stark (*Nature*, July 23, 1938, 139–42). The object of the expedition was to investigate the geological conditions and dating of the Wadi Hadramaut, and at the same time, as little of value was known of the archæology of the region, to examine any cultural material that might be encountered which could be assigned to a pre-Islamic dating. The expedition was productive of some very interesting results, especially in their bearing on the early relation, or rather absence of relation, between Africa and Southern Arabia. In the Wadi Amd, a tributary of the Hadramaut, three terraces were distinguishable, at 10, 5 and 3 metres above the wadi floor. In the 10-metre terrace gravels interbedded with æolian silt, identical with that of the valley floor, were stone implements of Levallois type, which, however, were of a crude workmanship and undifferentiated in type. There is here no succession of Stone Age cultures, such as occurs elsewhere, and not only does the industry show no sign of development, but there is a complete absence throughout of hand-axe, blade or burin. An obsidian industry of small blade cores and geometric forms was found, but by its associations is proved conclusively to be historic in chronology. The interpretation which Miss Caton-Thompson and Miss Gardner place upon this evidence is that there must have been in Pleistocene times in this region a complete absence of the stimulus towards progress coming from contacts with more progressive culture groups, while in view of the spread of the hand-axe cultures over the whole of Africa in the Palæolithic period, it may be concluded that the absence of this stimulus in Southern Arabia points to a marine separation of Southern Arabia and East Africa at this early period, eliminating conclusively the cultural interchange through this region, which had been suggested hypothetically. Excavation at Hureidha of remains, dating possibly from the sixth to fourth century B.C., produced evidence of an irrigation system, habitation sites, cave burials, and a temple of the Moon, of no little intrinsic interest and of value as the first fully authenticated archæological evidence to be derived from this region.

CULTURE CYCLES IN ANTIQUITY.—Advantage was taken of the presence of a large number of anthropologists and archæologists

at the Cambridge meeting of the British Association to arrange for the discussion of a number of problems of general import. Among these was a discussion on the Middle Palæolithic, while in his address on "The Orient and Europe" the President, Prof. V. Gordon Childe, handled the problem of cultural diffusion and chronology in the Bronze Age in the light of archæological discovery of the last ten years with consummate mastery of the facts and characteristic breadth of view.

The discussion on the Middle Palæolithic was opened by Mr. Miles Burkitt, who pointed out that the expression "Middle Palæolithic" has itself become ambiguous, and is no longer used by the majority of prehistorians. He was followed by Sir Arthur Keith on the skeletal evidence of Neanderthal man, who pointed out that, while a general type of *Homo neanderthalensis* is recognised in Europe, modifications are distinguished, such as the Chapellian, the Ehringsdorfian, Krapinian, and outside Europe, the Palestinian. The number of these modifications may well increase as knowledge increases, and will probably be found to coincide with the different cultures of the civilization, but the culture cycle as a whole is the product of an undifferentiated *Homo neanderthalensis*. Dr. F. E. Zeuner dealt with the geological evidence, and with the lithic industries of Ehringsdorf and Wallertheim, summarising the work of a large number of authors, in which the stratigraphical position of Mousterian and Aurignacian man can be studied in detail. It was evident, he pointed out, that in the area from Central and West Europe, through the Mediterranean to Palestine, the Mousterian, Levalloiso-Mousterian, and the uppermost Levalloisian, probably representing *Homo neanderthalensis*, lasted from the warm inter-glacial Riss-Würm until the end of the first phase of the Würm glaciation, while the Aurignacian, representing *Homo sapiens*, everywhere appears before the maximum of the second phase of the Würm. It would seem, however, that Aurignacian man did not appear generally in the area under consideration before the middle of the interstadial Würm 1–Würm 2. In North France and possibly in parts of Middle Italy the Middle Palæolithic did not become extinct before the climax of the second phase of the Würm glaciation, when the Aurignacian had become well established in the remainder of the area. Dr. D. A. E. Garrod dealt with the Middle Palæolithic flake industries of the Near East, basing her remarks upon her discoveries in the Palestine caves, where the cultures extend from Tayacian, and date probably from a relatively early stage of the Riss-Würm interglacial. This is followed by a late Acheulean, followed in turn by a long cycle of flake industries, which can best be described as

Levalloiso-Mousterian. A marked change in fauna probably corresponds to the transition from the Riss-Würm interglacial to the Würm 1.

Mr. A. L. Armstrong discussed the problem as it presents itself in South Africa, where the earliest phases, especially in Rhodesia, appear to be linked with the South African Acheulean, and the later ones with the South African Aurignacian. The English evidence was discussed by Dr. K. P. Oakley, who dealt with the industries in relation to the Pleistocene sequence in South-East England, and Mr. T. T. Paterson, who discussed the evidence of the Pleistocene sequence in the Thames valley. The result of the discussion will be found summarised by Mr. Burkitt in *Nature* of September 17, 1938, 512–13, where, while he admits that it cannot be said that the question was entirely dismissed, he points out that the term was not like " Middle Pleistocene ", an expression of time, and that it is obviously unsatisfactory, as cutting across an evolving culture like the Levalloisian. The content of the phrase, he goes on to say, has therefore grown to include all the cultures before the Aurignacian, the industries which for the most part are made from flakes, as is the case with the Mousterian and Levalloisian, in contradistinction to the *coup-de-poing*, and core-tool culture or cultures. Now it is pointed out that these flake-tool industries are not all similar, but are the products of allied, though slightly differing, cultures. Even in Europe there are the Cromerian, Clactonian, Levalloisian, Tayacian, Weimar, Mousterian, etc., their distribution sometimes fairly wide, at others restricted, not all of them contemporary, and some overlapping in time. Some were contemporary with the entirely different *coup-de-poing* culture, but distribution maps show that the two great culture groups are quite distinct, though a line of contact occurs in North-Eastern France and Belgium, and in South-Eastern England. Speaking generally, one can say that the cultures comprising the flake civilisations are found from the North Sea to China, and those of the core-tool civilisations in Western Europe only, and southward over most of Africa. We are, therefore, faced with the fact that as early as a period preceding the Upper Palæolithic, and in fact forming the Lower Palæolithic, there existed in the world two quite distinct civilisations or culture cycles, each made of a greater or smaller number of differing, though allied, cultures. From the foregoing it is evident that it is quite erroneous to name a culture of the flake-tool industry from outside France by the French name, unless the two cultures concerned are not merely allied, but identical. The term Mousterian, for example, should be restricted to the French culture, the industries of which

are well known and the products of a particular branch of Neanderthal man. It simply confuses the issue to use the name to describe allied industries found elsewhere in Europe, the product of differing, though related cultures, simply because both belong to the same culture cycle. Still worse to use this term, or perhaps equally, such terms as Clactonian, etc., to describe industries found in such far-flung parts of the world as South Africa or India.

The importance of the bearing of this argument on Palæolithic studies is further emphasised in a study of the Upper Palæolithic by Dr. D. A. E. Garrod (*Proc. Préhist. Soc.*, 1938, Jan.–July). There she points out that the effect of the researches of the last twelve years and the multiplication of researches outside Europe have placed a very different complexion on studies of the Later Palæolithic, while excavation in Africa, the Near East, Asiatic Russia, and China have opened up new fields for speculation. In these circumstances the inadequacy of the classification of Palæolithic industries first propounded by de Mortillet, based on discovery in Western Europe, or rather in France, has become increasingly apparent. The main outline of the new pattern is already beginning to appear, in which we can distinguish in the Old Stone Age three cultural elements of primary importance. These are manifested in the so-called hand-axe industries, flake industries and blade industries, and we know that the first two run side by side, at least so far as we can see. While this is the old division of Lower, Middle, and Upper Palæolithic in a new guise, it has a new axis, and care must be taken not to make it too rigid, for the cultures do not run parallel and independent, but are perpetually meeting and influencing one another. These general considerations are preliminary to a consideration of the problem of the origin of the blade industries, the cultures which appear in Europe towards the close of the Pleistocene and mark the extinction of Neanderthal man and the appearance of *Homo sapiens*. Essentially they are blade industries, the characteristic industries of Upper Palæolithic times, although in certain areas industries of Mousterian tradition lingered on. It is clear, Dr. Garrod goes on to say, that these blade cultures must have passed through the early stages of their development somewhere outside Europe, during Middle or even Lower Palæolithic times, but at present we have only the faintest clues as to how and where that development took place. As a corollary it is pointed out that in speaking of Upper Palæolithic the limitation it implies is a limitation due to our defective knowledge, and, while the term may be used of the period, we should refrain from falling into the error of calling the industries them-

selves "Upper Palæolithic". Unfortunately it is not possible here to follow Dr. Garrod through her interesting and instructive summary of the results of recent research on a worldwide basis. One example of the type of problem she has in view is afforded by M. Peyrony's recent researches in Perigord, which have shown, not without acute criticism from the Abbé Breuil, that, while the Upper and Lower Aurignacian are there closely related, they contrast with the intrusive character of the Middle stage. Notwithstanding his criticism of detail, the Abbé Breuil emphasises the notion of a double element in the Aurignacian, and it is pointed out by Dr. Garrod that discoveries in the Near East underline this view.

It is unnecessary to stress the importance of the line of analysis taken by Dr. Garrod in this study, especially, for example, in its bearing on such a problem as that posed by Dr. Hrdlička a few years ago in discussing the origin of Aurignacian man, when with his usual critical acumen he placed his finger on the weak point in current argument as to the origin and lines of migration of Aurignacian man, and pointed to the lack of any convincing evidence of origin from outside Europe.

In summing up her results Dr. Garrod, "taking a last general view of this theoretical picture," sees the Chatelperron point as the earliest identifiable phylum of the blade cultures, already emerging in Lower Palæolithic times, in some yet unidentified centre. Ultimately, she says, we may conjecture it sends out two branches, one into East Africa, to influence the evolution of the Capsian, the other into North-East Europe to develop into the Gravettian. Meanwhile another stock, the Aurignacian, pushes westward, and separates the two great provinces. From the Aurignacian and Gravettian centres migrations move into Central and Eastern Europe along the southern edge of the ice-sheet, and cultures, which in their homesteads tend to remain distinct and exclusive, succeed and influence each other, until at the extreme limit of their journey we get the characteristic French sequence, which for so long was used as a standard for the rest of the world.

Cultural Diffusion in the Bronze Age.—Analogous tracing of cultural movement and diffusion is the basis of argument in Prof. Childe's British Association address, to which reference has already been made. Here he goes back to the argument of Montelius, formulated just on forty years ago, in which certain propositions were laid down, as aspirations then, rather than conclusions based on ascertained evidence, bearing on the relations of geographically separated civilisations and cultures. Prof. Childe's aim was to show how the evidence of archæological research of the last ten years

has led to a redrawing of the picture, but fundamentally in its demonstration of the diffusion of culture bears out Montelius in his statements or conclusions.

The propositions as stated by Prof. Childe were (1) civilisation in the Orient is extremely ancient; (2) civilisation can be diffused; (3) elements of civilisation were in fact diffused from the Orient to Europe; (4) the diffusion of historically dated oriental types provides a basis for bringing prehistoric Europe within the framework of historical chronology; (5) prehistoric European cultures are poorer than contemporary Oriental cultures, that is civilisation is later in Europe than it is in the East. Evidence from recent excavation was adduced by Prof. Childe in support of each of these propositions. He pointed out that in Mesopotamia and on the Anatolian plateau chalcolithic civilisation could be traced in effect back to the sixth millennium B.C. and that this could be correlated with discovery in Macedonia and Central Europe. On a long time reckoning this might be taken as giving a date for the earliest stage of the Bronze Age in Central Europe of approximately 2800 B.C., while it is possible to trace a gradation of culture along these lines from the advanced city-culture of Mesopotamia of the Early Dynastic period through zones of ever decreasing cultural achievement ultimately to the food-gathering peoples of North-Western Europe. Even if the short time-scale were to be adopted, as archæological correlations appeared to demand, a similar though narrower system of zoning could be established, which supported the theory of a diffusional spread of culture and at the same time equally corroborated the propositions which had been put forward by Montelius in their entirety.

The theory of cultural diffusion as a method of argument in archæology has recently been the subject of frequent attack in Soviet circles. A statement of how the Marxist doctrine affects archæological argument is well illustrated in a recent summary of Russian work on the Old Stone Age in European Russia by Eugene A. Golomshtok (*Trans. Amer. Phil. Society, N.S.*, 29, 1938; see also *Nature*, October 15, 1938, 693–6) in which cultural development *in situ* is made the dominant factor in progress.

A valuable study of cultural movement as applied to Bronze Age problems in Britain will also be found in "The Early Bronze Age in Wessex", by Stuart Piggott (*Proc. Prehist. Soc.*, 1938, Jan.–July), with a pendant in an account of the excavation of a barrow on Breach Farm, Llanbleddan, Glamorgan, by W. F. Grimes in the same issue of the *Proceedings* of that Society.

NOTES

Recent Views on the Luminescence of Solids (J. Ewles)

The recent conference of the Faraday Society on Luminescence reflects the rapidly accelerating interest and importance of the subject. The interest has been stimulated by the needs of industry and the vigour with which industrial research has been applied in the preparation and use of luminescent powders for television screens and for improving the colour rendering of the modern discharge lamps. The importance of that part of the subject with which this essay deals lies in the recognition that the luminescence of solids is a property of the crystal state. Curie has added to the accumulation of evidence for this view an account of experiments showing that the luminescence of glasses increases as they become more crystalline, while Tomaschek's study of the line spectra of chromium-activated luminophors suggest far-reaching possibilities in the investigation of the solid state.

The preparation of a luminescent inorganic solid often involves the addition of a small trace—sometimes as little as one part in a million—of some *activator* to a transparent crystalline body with subsequent firing at a high temperature. But there appear to be cases where pure crystals may be made luminescent. Thus, besides the well-known pure tungstates, molybdates and platinocyanides, which are strongly luminescent at ordinary temperatures, Randall reports that many substances which are quite inactive at ordinary temperatures glow brightly under ultra-violet light at temperatures below about $-100°$ C., and that in some of these cases there is good reason to believe that the luminescence is a property of the pure solids. Riehl confirms the observations of several previous workers that absolutely pure zinc sulphide is luminescent and Ewles has obtained a strong ultra-violet fluorescence from spectroscopically pure calcium oxide. Nevertheless, it is still doubtful whether a *pure ideal* crystal can be luminescent, for it seems very probable that the phenomenon can always be traced to the presence of some inhomogeneity—a fault, a strain, a departure from stoichiometric

ratio. Further it is fairly certain that in some cases luminescence is a more sensitive test for impurities than the spectroscope.

The theoretical possibility, however, of pure crystal luminescence may be seen in terms of the modern quantum theory of crystals. According to these ideas the valence electrons in a crystal may only lie within certain permitted energy zones separated by forbidden regions (Fig. 1). In an insulator such as a luminescent crystal the occupied bands are quite full and on statistical balance there are always as many electrons moving in any direction as in the opposite

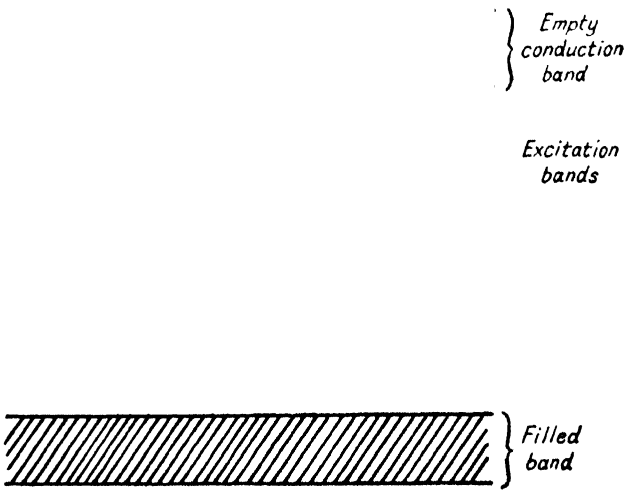


FIG. 1.

way even in electric fields. Only when an electron is removed from a full band to a higher empty band does it become free to move through the crystal and the hole left behind in the lower band acts like a positive electron also free to move. The energy required to remove an electron from a full band to a higher " conduction " band corresponds in a transparent crystal to a quantum of light in the far ultra-violet and that is why such crystals are normally insulators. In addition there exist in the forbidden region narrow bands of permitted energy value corresponding to the energy required to raise an electron to an excited state and giving rise to narrow absorption bands on the long-wave side of the continuous absorption corresponding to the first process. Von Hippel has shown that in the case of the alkali halides the energies of excitation are roughly equal to the energies required to remove an electron from a halogen ion to a neighbouring alkali ion. In the excited state the ions move

into new positions of equilibrium and, according to Seitz, the presence or absence of luminescent radiation depends on the relative positions of the curves representing the potential energy of the states as functions of the positions of the ions. There are reasons for supposing that the two curves will attempt to cross. Actual crossing is forbidden, but they may come very close before breaking apart again.

If as in Fig. 2 (*a*) the upper curve has its minimum before "crossing," the system after the absorption transition AA′ slides down to the point B′ with dispersal of energy to the surrounding lattice and then jumps to the normal state with emission of radiation represented in energy value by B′B. The reason for Stokes' Law is obvious. But if as in Fig. 2 (*b*) the upper curve has its minimum after "crossing," no optical radiation will occur since after the absorption transition AA′ the system slides down to D with thermal dissipation of energy and then slides either to A or to E. In the

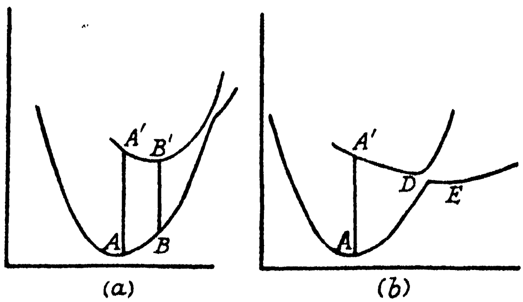


FIG. 2.

latter case thermal energy from the lattice is usually enough to lift it again to D with subsequent sliding down to A.

In general, as we have seen, the absorptions corresponding to the above processes in pure crystals lie in the far ultra-violet whereas in the most brilliant luminescent crystals the exciting absorption is in the near ultra-violet or even in the visible. In some cases there is fairly definite evidence that absorption and emission correspond to energy changes in some group or radical in a complicated molecule; in others such as solids activated by the rare earths or by chromium, to transitions in an incomplete ionic shell shielded by a completed shell. But there is a large class, including many of those of industrial interest such as zinc sulphide and various silicates, activated by metals other than rare earths or chromium, where absorption and emission cannot be so directly linked with energy changes in the activating atom or molecule. In such cases it is supposed that the activator introduces new sharply

defined energy levels in the crystal, localised at the activator. In some cases it is fairly certain that the activator brings in new absorption bands on the long-wave side of the fundamental absorptions and that absorption in these new bands gives rise to luminescence. The absorption process may result either in the raising of an electron from an activator level to a conduction level or from a lower band to an activator level and thence to a conduction band. Emission then takes place either when an electron falls from a conduction band to an activator level or from an activator level to a hole in a lower band. Milner suggests how these processes may account for the time delay involved in phosphorescence. Before an electron can drop back from an activator level to a lower band it must wait for a hole to appear at the position of the activator. This may take a long time since the holes travel slowly and the activators are few and far between. Gurney and Mott suppose that the exciting absorption raises an electron in an activator atom to an excited state whence thermal energy from the surrounding ions of the lattice is enough to raise it further to the conduction band. Here it travels freely through the crystal until it becomes trapped at one of the bound energy levels that exist at surfaces and in the neighbourhood of lattice defects. *Eventually* it may be released and find a positive hole to combine with, giving rise to phosphorescence in the process.

Such ideas imply a direct connection between phosphorescence and photoconductivity, but there are certain cases where phosphorescence is unaccompanied by photoconductivity. It therefore seems necessary to suppose that an excitation state can also be trapped.

Seitz observes that the absorption band exciting luminescence in the alkali-halide phosphors activated by thallium is in the same region as the characteristic absorption of thallium ions as measured by Hilsch and Pohl, and therefore suggests that the luminescence here is due to transitions in thallium ions which have replaced alkali ions in the lattice. In the case of pure zinc sulphide he gives reasons for supposing that the luminescence is due to extra zinc atoms in lattice interstices, while with activated zinc sulphide it is due to interstitial atoms of the impurity. Riehl appears to take a similar view, and describes experiments to show that the failure of bismuth and lead to activate zinc sulphide is due to the fact that they are too big to get into the available interstices. Riehl also raises the fundamentally important question of the seat of the exciting absorption. He has shown that some 80 per cent. of the absorbed radiation is converted into light and argues that this can only be explained if the whole of the lattice is taking part.

It will be seen that although this conference fulfilled its prime function of arousing discussion, most of the fundamental questions are still in debate. Can a pure ideal crystal show luminescence or is some inhomogeneity (foreign impurity, departure from stoichiometric ratio, lattice fault, etc.) necessary ? What is the nature of the absorption and emission and where do they occur ? Why do certain activators work and others not ? All these have yet to be answered with certainty. One thing is clear—they can only be answered on the basis of much more experimental evidence than exists at present.

Annual Report of the Calcutta School of Tropical Medicine and the Carmichael Hospital for Tropical Diseases, 1937 (P. J.)

The investigation into the nature and origin of epidemic dropsy has now continued for over ten years and is still not concluded. It has been shown that people who have not adopted the Bengali diet are rarely attacked by this disease. Europeans and Marwaris living in endemic areas are never attacked.

According to Acton and Chopra, the etiological agent of the disease is a toxin developed in certain rices. According to Chopra, Chaudhuri and Lal, the poor people who consumed the rice water escaped the disease, but the middle-class people who threw away the rice water were the victims. It was said to arrive from rices improperly milled and stored, but outbreaks have occurred among those using sun-dried and hand-pounded rice as well as among those taking parboiled and milled rice. The incidence of the disease and the consumption of opaque grains of rice are not correlated.

McCarrison found that rices freed by repeated washing from the supposed toxin were more harmful than the unwashed rices. With regard to the opacities produced in rice grains by micro-organisms, it has been stated that when rice is stored in hot and humid conditions, the bacteria attacked the grains and produced the opacities, the gram-positive spore-forming organisms gaining entrance through the germ and producing opacities in the interior. It has, however, been shown that opacities occur naturally in certain races of grains, and no organisms can be recovered from the cut surface of the opaque grain if the surface is properly sterilised.

Gram-positive spore-bearing bacteria belong to many different strains, and 28 varieties have been described. It is stated that in almost every case of epidemic dropsy there is a preponderance of gram-positive bacilli in the stools in the early stage, but in 10 per cent. of non-epidemic dropsy cases a similar bacterial picture is seen in the films. Those observers who favour the mustard oil

theory for etiology of epidemic dropsy depend on the epidemiological survey for the detection of the mustard oil responsible for the symptoms. Some of these suspected oils from various sources failed to transmit the disease ; no toxin has been isolated from the suspected samples. The mustard oil theory fails to explain the seasonal incidence of the disease, and the occasional epidemic outbreaks where no mustard oil has been used.

Attempts to isolate a virus from epidemic dropsy cases have failed and it seems unlikely a virus with racial prejudice is at work. Although a large amount of epidemiological data and certain experimental evidence has been adduced in favour of the mustard oil theory, more work is needed before it can be accepted—*e.g.* the nature of the toxin or the chemical method of detecting it in the suspected oil has to be worked out. Also on the bacteriological and virus side more work must be done before "the rôle of any organism in the causation of this disease can be established or definitely rejected."

Weil's Disease (Leptospiral jaundice).—Although no cases of this disease have been published it is probable that Weil's disease has occurred in India for many years. The appearance of two cases at Sambhunath Pandit Hospital started an investigation of the subject, and from one of these cases leptospira was isolated by blood culture and by inoculation of urinary deposits into guinea pigs. This strain has been named "Chopra strain." It is serologically identical with the Dutch strain, and with English strains isolated in London, but differs from many other strains (Burma, Sumatra and Andaman). More recently five more cases have occurred in Calcutta. The disease is not confined to people engaged in any particular occupation, no history of river bathing or contact with dirty water. But all patients lived in rat-infected houses.

The mortality rate, 2 out of 5, is high in this series ; in each case the diagnosis was established by blood culture, animal inoculations with blood or urine.

It is suggested that there is more than one serological type of leptospira in Calcutta, in which case serum reaction as a diagnostic method is valueless and misleading, while all the local strains have not been isolated.

The incidence of leptospira infection in local rats is very low. Out of 180 rats examined in 1925 none gave positive results ; during 1931 out of 193 rats only 2 showed the infection ; more recently out of 62 animals examined, including 2 rats trapped in a patient's house, all were negative.

To determine whether there was an immunity in these rats

protection tests were carried out with the sera of some of these animals against the local human strain, the result was negative in all cases. Further cases have been reported from other districts of India and a detailed account will be published when sufficient data are available.

Radio-sounding Balloons (S. K. L.)

Considerable attention has been given for several years in Europe and America to sounding the atmosphere by means of radio signalling. Most of the known methods suffered from some disadvantage or other, and an investigation was therefore made by the Radio Department of the N.P.L. to overcome these faults. A satisfactory method was eventually developed and is described in *Proc. Roy. Soc.*, A, **167**, 1938, 227–50.

The apparatus is arranged to give a continuous indication of atmospheric temperature and pressure at various altitudes, and measurement is effected at the ground station. A free balloon is used to carry a miniature radio transmitter which emits signals continuously during the flight. The transmitter is suspended by its aerial from a parachute, which in turn is attached to the balloon, so that when the balloon bursts, the transmitter makes a safe descent. An advantage of the free balloon system is that the ultimate loss of the apparatus is of no importance, for the required measurements are completed during the flight. Actually there have been very few instances of lost or damaged apparatus, and often the transmitter is used again for further tests.

The balloon is 4 ft. in diameter, and is filled with 120 cu. ft. of hydrogen, giving a lift of 500 gm. The rate of ascent is 3–4 m./sec. The rate of descent, with a parachute 6 ft. square, is 2·5–6 m./sec. The average duration of ascent is between 60 and 100 minutes, and the height normally achieved is about 10 km.

The radio transmitter contains four valves, supplied from small dry batteries capable of operating the set at constant output for about 120 minutes. A wavelength of 8·6 metres has been found best suited to this purpose. The carrier is modulated continuously with two audio tones, associated with the barometer and thermometer respectively. The barometer is arranged to give a modulation frequency varying between 700 and 1000 c.p.s. according to the pressure, while the range covered by the thermometer is 1400–1700 c.p.s. The barometer is an evacuated aneroid box so arranged that contraction of the box closes a gap in an iron-core inductance determining the frequency of one of the audio modulators. A change of pressure of 1000 mb. corresponds to a change of 250 c.p.s.

in modulation frequency. The thermometer, which utilises the expansion of a very fine wire, controls the gap in the iron-core inductance of the other modulator. A temperature change from +20° to −80° C. corresponds to a change of 200 c.p.s. in modulation frequency.

At the ground station, any sensitive short-wave receiver can be used, and the frequencies of the two audio modulation tones are measured in the normal way. Readings can be taken every half-minute with no difficulty, and automatic recording is under development. Wind direction and velocity are observed from the flight of the balloon, which is followed by means of direction-finding receivers on the ground. Signals from the balloon are still audible at a distance of 100 miles.

Consistency of operation is extremely satisfactory, and comparison tests with a Dines recording meteorograph attached below the transmitter showed good agreement up to 10 km.

Miscellanea.

H.M. the King has approved the recommendations made by the President and Council of the Royal Society that Royal medals be awarded to Dr. F. W. Aston, F.R.S., for his work on isotopes and to Prof. R. A. Fisher, F.R.S., for his contributions to statistics. The President and Council have made the following awards: Copley medal to Prof. Niels Bohr, For. Mem. R.S.; Rumford medal to Prof. R. W. Wood, For. Mem. R.S., for his work in physical optics; Davy medal to Prof. G. Barger, F.R.S., for his work on alkaloids and other natural products; Darwin medal to Prof. F. O. Bower, F.R.S., for his work in Darwin's own field; Hughes medal jointly to Dr. J. D. Cockcroft, F.R.S., and Dr. E. T. S. Walton, for their work on the disintegration of atoms.

Sir William Bragg has been re-elected President of the Society and Sir Henry Lyons, Treasurer. Prof. A. V. Hill and Prof. A. C. G. Egerton have been appointed Secretaries of the Society, and Sir Albert Seward, Foreign Secretary.

The Nobel prize for Physics for 1938 has been awarded to Prof. E. Fermi of the University of Rome in recognition of his work on atomic physics and more especially for his researches on artificial radioactivity produced by neutron bombardment.

Sir Albert Seward has been elected president of the British Association for the year 1939 when the association will meet in Dundee.

Dr. C. G. Darwin, M.C., Sc.D., F.R.S., Master of Christ's College, Cambridge, has been appointed to be director of the National Physical Laboratory. Sir Frank Smith is holding office until Dr. Darwin can take up his appointment.

Dr. G. Stafford Whitby, director of the Division of Chemistry, National Research Council, Canada, has been appointed to succeed Sir Gilbert Morgan as director of the Chemical Research Laboratory, Teddington.

Mr. O. T. Faulkner, C.M.G., has been appointed principal of the Imperial College of Tropical Agriculture, Trinidad.

We have noted with great regret the announcements of the death of the following well-known scientific workers during the past quarter: Mr. W. R. Barclay, metallurgist; Dr. Charles Carpenter, gas engineer; Prof. L. S. Dudgeon, St. Thomas's Hospital Medical School, pathologist; Dr. L. Frobenius, anthropologist; Mr. A. J. Greenaway, chemist; Dr. W. R. Gregg, chief of the U.S. Weather Bureau; Dr. T. C. Hebb, University of British Columbia, physicist; Prof. J. Kunz, University of Illinois, astrophysicist; Sir Robert Mond, F.R.S.; Sir Basil Mott, F.R.S., civil engineer; Miss Agnes T. Neilson, University of Glasgow, geologist; Prof. M. d'Ocagne, mathematician; Sir John Snell, electrical engineer.

Prof. E. O. Lawrence has lent the cyclotron designed by himself and Livingston in 1931 for exhibition at the Science Museum, South Kensington. This instrument is able to accelerate protons to a speed equal to that produced by a fall of potential through $1{\cdot}2 \times 10^6$ volts. Prof. Lawrence's latest model is illustrated by photographs. It delivers deuteron currents of the order of 10^{-4} amp. at speeds due to a potential of 8×10^6 volts or alpha-particle currents of 10^{-6} amp. at $1{\cdot}6 \times 10^7$ volts.

In an address to Section A of the British Association last August, Prof. Blackett gave some account of the properties of the "heavy" electron. Experimental evidence shows that the penetrating component of the cosmic rays consists of these particles, their mass being between one hundred and two hundred times that of an electron and their mean life period 10^{-6} sec. Since this period is so short the particles cannot be present in the cosmic stream when it first enters the earth's atmosphere, and they must be produced in some manner as yet unknown by the action of the incident electrons. The behaviour of the particles provides an explanation for the fact that the intensity of the penetrating rays after they have

passed through a large thickness of air is less than that under the same mass of water or clay.

The existence of "heavy electrons" was first suggested by Yukawa in 1935 (*Proc. Phys. Math. Soc. Japan*) to explain short-range nuclear forces. He concluded that they must disintegrate into electrons and neutrinos.

The Bulletin of the Imperial Institute for July–September 1938 contains a reprint of an address given by the director, Sir Harry Lindsay, descriptive of the work of the Institute. The foundation-stone of the building was laid in 1887 by Queen Victoria, it was opened in 1893 and became bankrupt in 1899, because of the £400,000 available for its foundation no less than £250,000 was expended on the building. The Government came to the rescue, gave half the building to the University of London and supplied funds for continuance of the work. In 1933–34 the financial position was again precarious, but there was now a better appreciation of the services of the Institute; the Colonial Governments doubled their grants, the Dominions resumed part at least of the contributions which ceased during the slump, and the United Kingdom Government helped so that the position seems once more to be secure.

Among the recent achievements of the Institute is the discovery in South Africa and Tanganyika of supplies of the mineral vermiculite used in the manufacture of certain heat- and sound-insulating plaster boards. It is hoped also that a plant recently reported from the Sudan may be valuable as a source of supply of camphor.

The notes in the *Bulletin* include a brief account of "the deepest oil well yet drilled," namely a well 15,004 ft. deep near Wasco in California. It yields 3600 barrels daily, and the temperature at the greatest depth is 268° F. There is also a description of the extraction and possible uses of indium. This metal has not yet been employed on a commercial scale. It increases the corrosion resistance of metal alloys used in jewellery and dentistry; silver-indium alloys have been developed for plating silverware to diminish tarnishing; it lowers the melting-point of Wood's and Lipowitz' alloys, and indium oxide imports a yellow colour to glass.

The Report of the Committee appointed by the Minister of Agriculture and the Secretary of State for Scotland, to review the facilities for veterinary education in Great Britain (H.M. Stationery Office, 2*s*. net) indicates that the existing conditions are very unsatisfactory. It states, in particular, that the veterinary schools are overcrowded, that the teaching staffs are inadequate and that facilities for clinical and practical training are insufficient.

It is recommended that the five-year course for the M.R.C.V.S. diploma should be reorganised so that the first three years may be devoted to a combination of scientific studies in which Universities award degrees; the last two years being spent on clinical work and practical training. It is suggested that there should be six months' pupilage on a farm so as to provide a foundation for more practical training in animal husbandry and that, in addition, each veterinary school should possess a 100-acre field station for further instruction in this subject. The three schools at present aided by Government grants, *i.e.* those in Camden Town, Liverpool and Edinburgh, are considered insufficient to maintain the required output of qualified persons and it is recommended that Government aid should be given to the existing school at Glasgow—which should be rebuilt and reorganised—and also to a small school to be established at Cambridge.

Circular C. 421, issued by the National Bureau of Standards, Washington, D.C. (price 10 cents), contains a detailed account of the "spectral-transmissive properties and use of coloured eye-protective glasses" preceded by a discussion of such incidental questions as the elimination of glare, distortion of colours, confusion of traffic signals and the danger of wearing coloured glasses when driving at night. There is no difficulty in selecting glasses which will eliminate glare from extensive areas of snow, water, sand and city streets in bright sunlight, but the advantage of reducing glare resulting from motor-car headlights by wearing dark glasses is more than counterbalanced by the danger arising from the decrease in general visibility. There are several glasses on the market which give satisfactory protection from both infra-red and ultra-violet radiation. They transmit a greenish light but the maximum transmission in the visible spectrum is under 50 per cent. Incidentally it is stated that the eye is able to change its sensitivity by a factor of about 1,000,000, variation the area of the pupil providing a factor 50, and variation in the response of the retina the remaining factor 20,000.

The Bell System *Technical Journal* for July 1938 contains an interesting paper by W. G. Gustafson on the magnetic shielding of transformers at audio-frequencies. It shows in particular the advantages of a shield composed of a copper cylinder interposed between two permalloy cylinders. The same number of the Journal contains an account of H. R. Hertz by Julian Blanchard and a reprint of C. J. Davisson's Nobel lecture describing the discovery of electron waves.

REVIEWS

MATHEMATICS

A Text-book of Convergence. By W. L. Ferrar, M.A., F.R.S.E. [Pp. viii + 192.] (Oxford: at the Clarendon Press; London: Humphrey Milford, 1938. 10s. 6d. net.)

This is a delightful book. Teachers and students who have the chance to examine it will want to possess it.

The book is intended for the use of undergraduates. It is divided into two sections; the first gives an introductory course which would be suitable for candidates for higher school certificates and scholarships; the second part covers the usual course for honours students.

The development of the theory has been planned with a view to the student's probable state of knowledge about other branches of mathematics. For example, some proofs appeal to diagrams. It is right that the diagrams should be given for they put the gist of the matter clearly to the reader. But the method hides difficulties and the author sometimes gives alternative analytical proofs for the benefit of those who possess a sufficient knowledge of the calculus. Again, at an early stage the theory requires two properties of real numbers which seem reasonable enough yet which require careful proofs. The author has chosen to assume (explicitly) these properties and then to prove them in an appendix which gives an account of the theory of real numbers. This allows an uninterrupted treatment of the convergence theory. In general, the main principle has been to give a clear and elementary account of the fundamental ideas involved.

There are plenty of well-chosen examples, many from university examinations. The harder are indicated and some others are worked or have hints given. The use of the notation, of which an example is

$$\text{“}\,\alpha_n \to 0 \text{ if}$$
$$\varepsilon > 0;\ \exists \mathrm{N}. |\alpha_n| < \varepsilon \text{ when } n > \mathrm{N},\text{”}$$

is encouraged. And it is refreshing to find that ε is no longer "however small" but "any (prescribed) positive number."

Mr. Ferrar remarks that the book is based on lectures he has given at various times. This is very evident from the way in which the theory is presented and in which difficulties, logical and practical, are explained. The book is, in fact, not a cold recital of facts, one after another; on the contrary, the teacher is there all the time ready for the queries which experience has shown must often occur in the student's mind.

Finally, the publishers have made a clear, readable book; and the price is reasonable.

J. W. A.

Frequency Curves and Correlation. By Sir W. PALIN ELDERTON, C.B.E., F.I.A., F.F.A. Third edition. [Pp. xii + 271.] (Cambridge: at the University Press, 1938. 12*s.* 6*d.* net.)

THIS is the third edition of what is now the classic text-book on the practical problem of Curve Fitting. The general character is unaltered. Essentially as each problem is considered, an example illustrates the method of applying certain principles. The first part of the book is devoted to a description of the methods of graduating data culled from actuarial experience by means of the various types of Pearson Curves which arise as particular cases of the integration of the differential equation,

$$\frac{d(\log y)}{dx} = \frac{x + a}{b_0 + b_1 x + b_2 x^2}.$$

The fitting of these curves by the method of moments to data is simply a question of skilled computation. The author gives practical cases in order to show the variations due to the different types of curve, and, where necessary, notes on the various types.

Then follows a chapter on other systems of curves, particularly the Gram-Charlier and the Edgeworth Curves. He illustrates with examples giving the results of fitting by these and the appropriate Pearson Curve. Perhaps in his next edition he will include some comparable illustrations using Gibrat's modification of the Normal Curve.

The second part of the book is devoted to Correlation, again with examples showing the methods of computation, and to problems of sampling. The practical problem of testing the Goodness of Fit is dealt with. On this subject Sir William has some sapient remarks. In particular, I would like to quote from page 204, " the statistics with which we deal in practice nearly always contain a certain amount of extraneous matter, and the heterogeneity is concealed in a small experience by the roughness of the data. The increase in the number of cases observed removes the roughness, but the heterogeneity remains. The meaning, from the curve-fitting point of view, is that the experience is really made up of more than one frequency curve; but a certain curve, approximating to the one calculated, predominates."

The author refers in the Appendix to Prof. Fisher's Method of Maximum Likelihood and to the practical difficulties involved in its use. He considers that one justification for the use of the Method of Moments in curve fitting is that, in practice, it gives curves which satisfactorily fit the data. This is a reasonable practical rule which serves in other walks of life. It is remarkable how well certain crude methods which have little theoretical justification give satisfactory practical results. One can instance various methods of smoothing irregularities in time series.

Although the illustrations throughout the book are mainly from actuarial experience, the book serves all those who want a knowledge of Curve-Fitting, Correlation and Sampling. It is easy to read and the examples are worked out in detail. One must have a fair command of mathematics to follow closely the developments of the methods, but certain of the more complex mathematical problems are relegated to the Appendix, and there is a suggestion for Abridged Reading in Appendix VII.

The book was born in 1906, and it shows no sign of mortality—in which the author takes such a lively interest.

E. C. RHODES.

Modern Higher Algebra. By A. ADRIAN ALBERT. First impression, December 1937; Second impression, August 1938. [Pp. xiv + 319.] (U.S.A.: University of Chicago Press: Great Britain and Ireland: Cambridge University Press, 1937. 18*s*. net.)

RESEARCH in algebra has progressed rapidly on the abstract side in recent years and this book gives an account of the present state of the subject. Prof. Albert has himself contributed largely to the theory, especially on the subject of Riemann matrices.

The first two chapters deal with the basic concepts: group, ring, integral domain and field. Then come three chapters on the theory of matrices whose elements belong to a general field.

The Galois theory is taken next. The author develops this from the point of view of the group of all automorphisms of a normal field. This is considered an improvement on the older treatment in which the Galois group arises as only a sub-group of a group instead of as a whole group.

There follows an introduction to the theory of linear associative algebras, an important branch of modern algebra. This is the theory of linear sets $\mathbf{U} = (u_1, u_2, \ldots, u_n)$, of order n, defined over a field $\mathbf{F}$, which have been made into associative rings over $\mathbf{F}$ by defining multiplication in $\mathbf{U}$ suitably. The treatment is from the matrix point of view and this justified by a theorem that every algebra $\mathbf{U}$ over $\mathbf{F}$ is equivalent to an algebra of square matrices with elements in $\mathbf{F}$.

The last two chapters deal with a subject new to text-books in English, namely the valuation theory of fields and the theory of p-adic numbers. This theory involves infinite sequences and what amounts to the notion of a limit; thus it is sometimes called a transcendental theory of fields. The idea of a valuation comes to this: a field $\mathbf{F}$ is said to have a valuation ψ if there exists a real function $\psi(x)$, defined over $\mathbf{F}$, in the real number field, such that $\psi(0) = 0$, $\psi(x) > 0$ if $x \neq 0$ in $\mathbf{F}$, $\psi(xy) = \psi(x)\psi(y)$, $\psi(x + y) \leqslant \psi(x) + \psi(y)$. The derived field $\mathbf{F}_\psi$ of a field $\mathbf{F}$ is the least extension of $\mathbf{F}$ such that every sequence of elements in the extension, whose values converge in the Cauchy manner, converges to an element of the extension. The valuation ψ is non-Archimedean if and only if $\psi(x + y)$ is at most the maximum of $\psi(x)$ and $\psi(y)$ for every x and y of $\mathbf{F}$. The p-adic numbers are the elements of the derived fields of algebraic number fields with respect to their non-Archimedean valuations.

The author has a concise style and helps the reader by explaining at suitable stages what is to be done and how much has been done. There are many exercises, designed to illustrate the general concepts in the theory; and at the end is a glossary of the main definitions. The book will be a standard work for those interested in advanced algebra.

J. W. A.

British Association Mathematical Tables. Vol. VI: Bessel Functions. Part I: Functions of Orders Zero and Unity. [Pp. xx + 288.] (Published for the British Association at the Cambridge University Press, 1937. 40*s*. net.)

ALTHOUGH the British Association Committee for the calculation of mathematical tables decided fifty years ago that the tabulation of Bessel functions was the most useful undertaking they could promote, many smaller but more urgent tasks have been undertaken, and it was not until the end of last

year that the first volume of tables was actually published. The present volume is concerned with Bessel functions of orders zero and unity, and two further volumes are now promised. The first of these will deal with functions with other integral orders, and the third volume will deal with Bessel functions of various fractional orders, and a miscellaneous group of associated functions. The present volume, edited by Dr. Hendersson of King's College, London, is a magnificent tribute to the enthusiasm and accuracy of the members of the Committee. It is beautifully printed by the Cambridge Press and should be of the greatest assistance in many branches of mathematical physics.

The tables in this volume contain the values of the functions J_0 and J_1 to 10 figures for values of the argument from 0 to 25. The values are given at intervals of ·001 up to $x = 15{\cdot}5$, and at intervals of ·01 from $x = 15{\cdot}5$ to $x = 25$. The next table contains the first 150 zeros of J_0 and J_1 given to 10 places of decimals, together with the value of each function at the roots of the other. The functions Y_0 and Y_1 are tabulated to 8 decimal places at intervals of ·01 up to $x = 25$; and a further table gives the first 50 zeros of these functions, and the values of each function at the roots of the other. Subsequent tables deal in a similar manner with the functions I_0 and I_1, K_0 and K_1, the interval of the range covered being from $x = 0$ to $x = 5$. The intervals for the first two functions is ·001 and for the second two functions is ·01. In each case the values are given to 8 significant figures. Other tables give the auxiliary functions required in the use of the asymptotic series for all these functions, and a concluding group of three tables provides all the coefficients necessary for interpolating in tables in this volume.

The volume is dedicated to Professor Alfred Lodge, who has been a member of the B.A. Committee from the day on which Bessel functions were first discussed until his death last year. It is a wonderful tribute to the admiration, respect and affection which he has won from his colleagues.

G. T.

Lectures and Conferences on Mathematical Statistics. By J. Neyman. [Pp. viii + 163.] (Washington: The Graduate School of the U.S. Department of Agriculture, 1938. $1.25.)

This is a mimeographed transcript of lectures delivered at the Graduate School of the United States Department of Agriculture, and is doubtless produced to supply those who attended them with a record of what was said. There is not enough original matter to justify publication as a book, and too much that is really trivial.

R. A. F.

ASTRONOMY AND METEOROLOGY

The Pageant of the Heavens. By Frederick Warren Grover, Ph.D. [Pp. xix + 157, with 8 maps and 5 figures.] (London, New York, Toronto: Longmans, Green & Co., 1937. 12s. 6d. net.)

This book is written in a scholarly fashion for those who wish to make a first-hand knowledge of the constellations the starting-point of a rather more detailed acquaintance with the universe. Its study will repay both the interested layman with no optical aid and the beginner with a small telescope. The treatment is non-mathematical throughout, and generous quotations from the poets (the older poets—how much less astronomically-minded are

our modern bards !) enliven the pages without detracting from the author's instructional purpose.

Part I describes the constellations and the seasonal changes of the sky. For this purpose eight loose star maps are provided for direct comparison with the sky. Each map is so printed that the stars appear bright on an opaque background when the sheet is illuminated from behind with an electric torch. This arrangement, though not new, is an admirable way of getting over the difficulty of dark-adapting the eye afresh after each consultation of an ordinary map. The maps are drawn for use in the U.S.A. and refer to latitude 40° N. The adjustment needed to relate them to English skies, however, will not be beyond even the beginner.

In Part II the knowledge of the constellations thus gained is used to follow the apparent movements of the sun, the moon, the planets, comets, and meteors. This section is treated thoroughly and forms the greater part of the book.

The 15 pages of Part III serve to introduce the reader to the rest of the universe; giant and dwarf stars, clusters, nebulæ, binaries, variables, and novæ all finding mention. Cosmological speculations are left severely alone. The treatment here, though brief, should tempt many to enquire further among the more advanced works recommended in an admirably full appendix.

The book as a whole is well produced. Its beautiful text is however disfigured by crude line diagrams, and there are no photographs. A very full table of contents forms no adequate substitute for an alphabetical index. In view of the author's interesting and authoritative treatment of his subject, it will be a pity if the price of the book should deter the enthusiastic amateur from making its acquaintance.

A. H.

The Climate of the British Isles. By E. G. Bilham, A.R.C.Sc., D.I.C., B.Sc., F.R.Met.Soc. [Pp. xx + 347, with 101 figures.] (London: Macmillan & Co., Ltd., 1938. 21*s*. net.)

This work attempts to summarise the knowledge of various components of our climate which has been accumulated by precise recorded observations during the last hundred years, mostly of course during the last few decades. After brief introductory chapters on the data and the methods by which it has been obtained, the book consists of descriptions of Characteristic Types of Weather, and of climatic components, *viz*. Wind, Rainfall, Evaporation and Percolation, Temperature of the Air, Sunshine and Cloud, Humidity, Frost and Hail, Atmospheric Obscurity, and Special (local) types of Climate. There is no general summary and no attempt to "place" our climate in the world. It is treated as a matter peculiar to these islands, on the basis of information collected in and near to them.

Every part of the work is soundly based on carefully analysed records; and the author does not go beyond what these records justify. Hence there is none of the broad and picturesque, but unreliable, generalisations which are often made concerning climate. Yet there are many sections likely to be of general interest; such are those on Dew Ponds, based on measurements of dew, of rain, and of evaporation; on London's fogs; and on "Buchan's Periods," which concludes that there is here a case for investigation. The development of air navigation in recent decades has given the facts of atmospheric obscurity, and its complement visibility, great importance; and the

chapter devoted to this will be welcome to all who value a sound summary of what has been done on it. The cartogram (Fig. 92, p. 275) showing the distribution of days with fog at low altitudes bears a very close resemblance to those showing the distribution of our urban population. Except for South Wales the areas of maximum fog are the areas of dense urban population plus a strip to leeward of them, a coincidence which gives evidence in support of the view that we produce a substantial part of our fogs from our chimneys.

Errors and misprints are extremely rare. On Fig. 57, p. 179, it seems that the figures 50° and 60° should be interchanged. In the summary of factors influencing the variations of temperature (p. 159) it is curious that there is no mention of differences of latitude, and/or the resulting differences of insolation, among the reasons why in summer the south of Great Britain is warmer than the north. The author appears to ascribe the whole of this difference to "continentality," a term which he does not use elsewhere but which is widely used as one of those words "of learned length and thundering sound" which can conceal lack of exact knowledge.

In the preface the author states his aim as "to survey the facts . . . and to compile a reasonably complete summary of the main climatic factors of our area, within the compass of a volume of moderate size." This aim he has achieved ; and his book is likely to be for some years to come the standard one-volume work on British Climate.

C. B. FAWCETT.

PHYSICS

The Flow of Homogeneous Fluids through Porous Media. By M. MUSKAT, Ph.D. With an Introductory Chapter by R. D. WYCKOFF, B.S. in E.E. [Pp. xx + 763, with 279 figures.] (New York and London : McGraw-Hill Publishing Co., Ltd., 1937. 45s. net.)

THE volume under review deals with the difficult problem of the flow of homogeneous fluids in a porous medium. As pointed out in the introduction, there are many branches of applied science in which such a study has considerable significance. The flow of underground gases, oils and water to wells and bore-holes, seepage of water under foundations, through filter-beds and the draining of land and soil are of interest to the engineer.

In certain cases met with in practice, as, for instance, the flow of mixtures of oil and gases in underground reservoirs, the assumption of homogeneity is not justified, but it is suggested that certain results obtained from the study of homogeneous fluids can with discrimination be applied to the more difficult problem of the flow of heterogeneous fluids. A superficial approach to the subject of the book reveals at once the necessity of a definition of a porous medium and raises doubt as to how far the porous materials met with in connection with practical problems can comply with the ideal porous medium assumed in the theoretical and experimental investigations dealt with in the text. The ideal porous material, it is suggested, is a body of ordinary unconsolidated sand, in which there are voids of varying sizes and shapes comprising pore spaces connected to each other by constricted channels, the whole forming a network of openings through which the fluid may flow. The conditions of flow in such a medium are, therefore, quite different from that in pipes or capillary tubes.

Commencing with a systematic packing of spheres it is shown that the ratio of the permeability to the porosity depends upon the method of packing,

and this ideal case indicates the impossibility of deriving permeability estimates from porosity measurements. Sands, clays and natural rocks are discussed and tables showing grain size and distribution, porosity and permeability of some typical sandstones are given. The assumption of Darcy's Law makes it possible to develop equations of flow for many cases, while even simple modifications lead to intractable differential equations.

The second part of the book deals with the steady state flow of liquids. Two-dimensional flow problems are first discussed in detail, a solution is obtained for the radial flow into a well, and more general problems are considered. Models and apparatus are described. Three-dimensional problems, taking the well-known Laplace equation as the basic differential equation, are next considered, and the equation is solved for several special cases.

The third part of the book deals with the flow of compressible liquids and gases through porous media. The possibility of obtaining solutions of the equations in a number of special cases is discussed, and reference is made to the particular case of the East Texas oilfields, the production from which is affected by water in the adjacent sands.

There are appendices dealing with "Laplace's equation in curvilinear coordinates," "Some two-dimensional Green's Functions," "The transformation properties of the Modulon Elliptic Function," "Proof of the generalised Poisson Formula," "Tabulation of the specific quantitative results developed in the work, in formula or graphical form." The mention of these appendices indicates that the solution of the problems discussed in the work requires the aid of advanced mathematics, but what is of greater importance is that the fundamental conceptions and assumptions, upon which the solutions are based, are candidly and clearly discussed throughout a book which makes a real contribution to subjects of great practical and scientific interest.

F. C. L.

Materials and Structures. Vol. II: The Theory and Design of Structures. By E. H. Salmon, D.Sc.(Eng.), M.Inst.C.E. [Pp. viii + 796, with 516 figures.] (London, New York, Toronto: Longmans, Green & Co., 1938. 32*s*. net.)

This is the second volume of a treatise on materials and structures, the first one, dealing with materials, having made its appearance in 1931.

It has been prepared with that care and attention to detail which readers of Dr. Salmon's other works have learned to expect from him, and the first claim made for it is that it is a comprehensive book intended for students preparing for degrees or for the Associate Membership of the Institution of Civil Engineers. The claim to comprehensiveness will not be disputed: indeed, its quality in this respect may perhaps prove somewhat of a drawback to its use as a text-book unless it is read under expert guidance. There is so much first-class matter included in it that the selection necessary when preparing for an examination may be somewhat embarrassing to the average student.

It is not uncommon to find in one book chapters on the design of roofs, girder bridges, etc., in addition to the general theory of stress distribution in simply-stiff and redundant structures, but Dr. Salmon is not content even with this. He extends his field to the manufacture of structural steelwork, electric arc-welding and the properties of building materials, and treats these subjects clearly and well within the limits of space available.

The book is thoroughly up-to-date, as shown by the inclusion of moment-distribution analysis and references to the work carried out for the Steel Structures Research Committee.

The second claim, that it is a work of reference for engineers, is fully justified and it should find a place in the library of all structural designers.

A feature of the book, as in others from the same author, are the admirable bibliographies at the end of each chapter. These will enable readers interested in particular branches of the subject to pursue their studies further with the minimum difficulty.

The diagrams are plentiful and well drawn and the printing and binding are excellent.

The book is one which can be thoroughly recommended to all interested in the theory and design of structures and teachers of engineering particularly should find it of great value.

A. J. S. P.

The Principles and Practice of Surveying. By CHARLES B. BREED and GEORGE L. HOSMER. Vol. I, seventh edition. [Pp. xxii + 717, with 217 figures.] Vol. II, fifth edition. [Pp. xxii + 675, with 247 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. 20*s*. net and 17*s*. 6*d*. net.)

THIS excellent American work on Surveying is published in two volumes—*Elementary Surveying* and *Higher Surveying*. Each is printed in clear type on thin paper, has a flexible cover, and can be carried in the pocket. The line diagrams are quite clear and distinct, but the details tend to be rather blurred in many of the half-tones.

Though some parts of the first volume might appropriately be termed "higher" surveying, and portions of the second volume are relatively simple, the division of the subject matter is perhaps as satisfactory as could be arranged. Obviously, it has been necessary to compromise in various ways, so that each volume may be more or less self-contained; the same considerations lead to a certain amount of duplication, though this is not serious.

The general impression one receives is that the authors have taken meticulous care in preparing the text. They have endeavoured to include all possible hints and warnings that might assist the young and inexperienced surveyor: they discuss the relative advantages and disadvantages of the different surveying instruments and methods, and give data as to the accuracy obtainable with them.

Certain sections have particular reference to United States practice, and are not directly applicable to surveys in this country, but the bulk of the text is of universal application. The instruments, which are described most fully, are, as one would expect, of American design, and this differs considerably in some cases from European design. Short descriptions are, however, given of a few European models.

The familiar forms of field book used in this country, and the common "rise and fall" pattern of level book, seem to be omitted: possibly they have been discarded in the United States.

The methods adopted for the setting-out of railway curves do not receive much attention, and transition curves are apparently considered as beyond the scope of the books. This omission is somewhat surprising, since such curves are very frequently used nowadays, both on railways and on roads.

Throughout the two volumes, numerous problems are given to be solved by the student, but unfortunately, and rather surprisingly, answers are not provided.

Astronomical observations are treated mainly in the second volume, though Chapter VIII in the first volume is also devoted to the determination of azimuth and latitude. It may be mentioned that the calculations are based on data taken from the "American Ephemeris and Nautical Almanac," but as the standard meridian is that of Greenwich, astronomical quantities are referred to 0.h. Greenwich Civil Time. It follows, therefore, that the treatment of the subject is very much the same in this book as in British works. The abbreviations used differ from those in common use here, but no confusion should arise on that account.

In the fifth edition of the volume on "Higher Surveying," the chapters on Terrestrial and Aerial Photographic Surveying have been rewritten and extended. The descriptions are very lucid, and sufficiently detailed for the student to understand the principles involved, and to appreciate the purpose and benefit of the expensive and complicated instruments which are used in preparing the maps. A bibliography is added, as, obviously, a full treatment of the subject is impracticable in a general work on surveying.

The books form an authoritative and standard work on surveying, and are well worthy of a place on the shelves—or in the pockets—of all surveyors.

W. N. T.

Automatic Protection of A.C. Circuits. By G. W. STUBBINGS, B.Sc., F.Inst.P., A.M.I.E.E. Second edition. [Pp. viii + 311, with 210 figures.] (London: Chapman & Hall, Ltd., 1938. 15*s*. net.)

THE first edition of this book was reviewed in SCIENCE PROGRESS, Vol. XXX, p. 358. The second edition is very similar to the first, but new and useful subject matter is introduced dealing with standardisation of instrument transformers, busbar protection, and negative phase sequence relays.

The book deals with three main divisions of the subject, namely, the principles underlying automatic protective apparatus, the application of these principles to practical protective systems, and the testing and maintenance of protective apparatus.

It was pointed out in the first review that the book falls into three main divisions, the first dealing with the theory of protective apparatus, the second with its practical application, and the third with methods of testing and maintaining it.

The first division gives an excellent account of the theory of protective apparatus which should be invaluable to a student of the subject. The second division deals with the more controversial subject matter of practical application, and the third division is a useful compilation of maintenance methods.

It is to be regretted that the errors in the figures and titles mentioned in the review of the first edition have not been corrected in the second edition and that the lack of consistency in nomenclature and in the mention of manufacturers of the apparatus described still exists. The value of the book would have been enhanced if these points had received attention.

Like the first edition, the second edition will be a useful text-book to the student who wishes to make himself familiar with the theory of automatic protective apparatus.

B. H. L.

CHEMISTRY

Outlines of Theoretical Chemistry. By F. H. Getman, Ph.D. Sixth edition by F. Daniels. [Pp. x + 662, with 170 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1937. 18*s*. 6*d*. net.)

This, the sixth edition of Prof. Getman's well-known text-book, has been extensively revised and whole chapters have been rewritten or deleted to make way for new material. The difficult task of deciding to what extent new developments in theory, and current or controversial topics are to be incorporated remains a perennial problem in revising a text-book, but Dr. Daniels has been most happy in his selection, and where new ideas are introduced they create an enquiring but never a bewildered mood as so often proves the case.

Whilst the first half of the book dealing with some properties of matter, solutions, electrolytes, colloids, chemical kinetics and kindred topics, is treated in a more elementary fashion than is perhaps customary in a volume of this size, the later chapters on electrochemistry and thermodynamics reveal the influence of the author's compatriots Lewis and Randall, and are presented in much greater detail.

An engaging feature of the book is the idea of relegating to the appendix all detailed mathematical proofs and derivations which so often contrive to destroy the continuity of the text. The many problems have been arranged in two sets which segregate the mere " substitution-into-formula " type from those which presuppose more extensive reading and a measure of ingenuity for their solution.

An occasional cross-reference is incorrect (pp. 41, 613) and there are some lapses in proof-reading (notably p. 613, par. 4, and p. 412, problem 30), whilst from p. 524 might be gained the impression that hydrogen and helium atoms each contain but one electron.

The layout, type and binding are excellent, and this is the only book known to the Reviewer where trouble has been taken to explain the reason for the appearance of subsidiary maxima in the X-ray spectrometric analysis of rock-salt—a minor point which has puzzled innumerable students.

H. Irving.

Modern-Life Chemistry. By F. O. Kruh, R. H. Carleton and F. F. Carpenter. Edited by W. R. Teeters. [Pp. xxvi + 734, with frontispiece, 1 plate and 359 figures.] (New York, Chicago and Philadelphia: J. B. Lippincott Company, 1937. 8*s*. 6*d*. net.)

The authors of *Modern-Life Chemistry* postulate that an up-to-date course in (American) high-school chemistry should function so as to modify human nature in a desirable way—both in its individual and social aspects—and assert that it should be taught not only for the sake of the chemistry involved, but rather for the education of the learner. The result of their collaboration is quite unlike the average English text-book, which is so commonly preoccupied with the syllabus requirements of one or other of the public examining bodies, and therefore restricted as to subject matter and seldom conspicuously original in outlook or treatment.

To start with, the whole subject is here approached from an unconven-

tional angle. The conception of atoms as electrical structures is introduced at the outset and this, and a survey of solutions and " near-solutions " (*i.e.* colloids—a felicitous vulgarisation), logically precedes an account of dissociation in solution and electrolysis. Other excellent portions are devoted to sulphur and its compounds, the halogens and the periodic classification, mineralogy and simple organic chemistry.

There is a wealth of illustrations with both line drawings and photographs. The latter are with few exceptions well-chosen, and illustrate almost everything from smoke-prevention by the Lodge-Cottrell method (before and after treatment !) to soil erosion in Nebraska and an armoured-car firing tear-gas shells to disperse a mob. The statement that " calcium chloride is used to lay the dust on the roads " has always seemed to the Reviewer a case of the wish being father to the thought : that it is so used (at least in the U.S.A.) is put beyond question by the photograph on page 475.

One more aspect of this book demands comment. The authors have striven to make it provide " a complete programme for the *teaching* of chemistry " and have adapted the Morrison unit-problem plan to this end. This scheme is insufficiently well-known in this country, and as an admirably consistent exposition of it *Modern-Life Chemistry* deserves to be read and digested by every live teacher of real-life chemistry.

H. Irving.

Clowes and Coleman's Quantitative Chemical Analysis. Edited and revised by D. Stockdale, M.A., Ph.D., A.I.C., and J. Dexter, M.A., B.Sc., A.I.C. Fourteenth edition. [Pp. xiv + 616, with 130 figures.] (London : J. & A. Churchill, Ltd., 1938. 18*s.* net.)

Although new methods of analysis are continually being worked out, and must be included in a modern text-book, they rarely displace the older methods, consequently the task of revising a standard text-book consists largely in condensing old subject-matter in order to find space for the new. This has been done in the fourteenth edition of Clowes and Coleman by re-setting the type and re-writing much of the text. In this way space has been found for new determinations of aluminium and magnesium by 8-hydroxyquinoline ; the colorimetric determination of aluminium by " aluminon " ; the use of ferrous phenanthroline as internal oxidation—reduction indicator, ceric sulphate, salicylaldoxime in the analysis of cupronickel, etc. The section on the analysis of non-ferrous alloys and the valuation of manures has also been completely revised, but it is specially emphasised that the section (200 pages) on General Quantitative Analysis must no longer be regarded as a work of reference but is " of most value when used as an intermediate text-book to give a preliminary knowledge of the more important special processes."

" Clowes and Coleman " is an old friend, and to recommend it is almost superfluous. Generations of students have used its thirteen editions and many more will continue to do so. On one small point the book does itself a grave injustice. Surely " estimation "—the approximate judgment of number, amount, etc.—is hardly the correct description of the exact experiments contained in this book.

J. N. S.

Outlines of Methods of Chemical Analysis. By G. E. F. Lundell, Ph.D., and James Irvin Hoffman, Ph.D. [Pp. xii + 250, with 115 tables.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. 15*s.* net.)

An outstanding merit of this book containing 115 tables in 250 pages is that there are comparatively few words in the text. An extraordinary amount of valuable matter is compressed within its relatively small compass, and in contrast with encyclopaedic works it can with advantage be read from cover to cover. Everyone concerned with inorganic analysis extending beyond established routines would do well to place it in an accessible place in his laboratory, so that it may be the first work consulted when any unfamiliar analytical problem is encountered.

We live in a time when the difficulties of analytical separations are increasing because of the growing complexity of industrially valuable materials, and the ever-increasing use of the rarer elements. Lack of time usually prevents the industrial chemist from doing more than cope with specific difficulties in a hand-to-mouth manner as they arise. The authors have tackled the problem in a really fundamental way by providing analysts with information concerning the behaviour of *all* the elements in the more important reactions used in analytical chemistry—information based very largely on their own experimental work. Twenty-two diagrammatic outlines of typical schemes of analysis are given illustrating a variety of methods of overcoming difficulties, and exemplifying the differences between "umpire" and routine analyses.

Any criticisms that might be made are of such a minor character that one welcomes unreservedly this novel and useful work.

T. B. Smith.

Semi-Micro Qualitative Analysis. By Paul Arthur, Ph.D., and Otto M. Smith, Ph.D. [Pp. xii + 198, with frontispiece and 10 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1938. 12*s.* net.)

This book describes the application of ordinary analytical methods to the semi-micro scale, and is based on a course used in the Oklahoma Agricultural and Mechanical College. The scheme differs but slightly from the customary macro-procedure, only the technique and a few reagents being changed, and it is recommended as an alternative to the older methods, "for any student or instructor can readily acquire the skills necessary in the handling of small amounts and the identification of the product of the reactions." The instructions are adequate but not always very happily phrased and the book is very expensive.

J. N. S.

An Intermediate Course of Volumetric Analysis. By Gordon Edward Watts, M.A., Ph.D., B.Sc., F.I.C., and Clifford Chew, M.Sc.Tech., F.I.C. [Pp. viii + 224, with 10 figures.] (London and Glasgow: Blackie & Son, Ltd., 1938. 3*s.* 6*d.* net.)

This laboratory manual is based on a course of instruction given by the authors at Brighton Technical College, and covers the syllabus of the Intermediate B.Sc. and similar examinations. The exercises are arranged in twenty-two groups, each of which can be completed in a three-hour labor-

atory period. The choice of experiments is wide and the working instructions clear and adequate, but the tendency to build up equations to their maximum complexity (Chaps. V and VI) might well have been avoided. The book may be recommended with confidence and its very modest price is an additional attraction.

J. N. S.

Calculations in Quantitative Chemical Analysis. By JOHN A. WILKINSON, Ph.D. Second edition. International Chemical Series. [Pp. x + 154.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1938. 10*s*. 6*d*. net.)

THIS book was first published ten years ago. A few minor changes have been made in the new edition and all problems have been re-calculated using the 1937 table of atomic weights. The book is well planned, written in an American style, but the price is quite prohibitive to English students.

J. N. S.

Catalysis from the Standpoint of Chemical Kinetics. By G.-M. SCHWAB. Translated by H. S. TAYLOR and R. SPENCE. [Pp. xii + 357, with 39 figures.] (New York: D. van Nostrand Co. Inc.; London: Macmillan & Co., Ltd., 1937. 18*s*. net.)

THIS volume is a necessarily much revised but not greatly enlarged version of Schwab's book on Catalysis published in German eight years ago. The first half of the book deals with homogeneous catalysis, there being two short chapters devoted to gas reactions, the remainder dealing with liquid phase catalysis. The second half of the book is devoted to heterogeneous catalysis.

Since the book is likely to be used extensively by students the following points may be noted. In the first section on p. 26 in the formula giving the rate of decomposition of polyatomic molecules, *n* is the number of square terms and not the number of degrees of freedom. The reference to Semenoff on p. 47 as being responsible for a wave mechanical treatment of resonance between all molecules in a reaction space is wrong; the references are to his classical papers on critical oxidation limits.

The chapters on catalysis in liquids have not been changed appreciably. Here a full description is given of acid and basic catalysis, and of the theories of Dawson, Brönsted, Bjerrum and others. But surely the statement on p. 167 that "the general theory of reaction rates reduces the problems of kinetics to the thermodynamic calculation of an equilibrium constant for the activated complex and the calculation of a transmission coefficient" was written in a spirit of light-hearted optimism scarcely justified by subsequent events.

In the second half of the book, activated adsorption has a section to itself, but no definite statement is made about the nature of the process—a feature which leaves the uninitiated reader somewhat in the dark. The diagram on p. 242, while probably energetically correct does not necessarily indicate the relative probabilities of the occurrence of reaction, since there is no explicit relationship between the absolute velocity and energy of activation of a heterogeneous reaction. Brief discussions are included on the quantum mechanical interpretation of heterogeneous processes and on heterogeneous chain reactions.

The book thus gives an extensive unbiassed and sufficiently documented account of present ideas of catalytic phenomena. Comparison with earlier books on catalysis cannot fail to impress the reader that the word catalysis no longer conceals an admission of ignorance about chemical phenomena on the part of the chemist.

H. W. MELVILLE.

The Fine Structure of Matter. Vol. II of a Comprehensive Treatise of Atomic and Molecular Structure. Part II: Molecular Polarisation. By C. H. DOUGLAS CLARK, D.Sc., A.R.C.S., A.I.C., D.I.C. [Pp. lxxii + 242, with 35 figures.] (London: Chapman & Hall, Ltd., 1938. 15*s*. net.)

THIS volume forms part of a Comprehensive Treatise on Atomic and Molecular Structure. Each volume is complete in itself and this addition is no exception. Like many other branches of physical chemistry, accumulation of data makes it necessary that an ordered compilation should be attempted from time to time, so that information on a given topic may be made readily accessible to a non-specialist. This is the main purpose of the book under review. Dr. Douglas Clark has, however, gone much further than make a useful compilation, for he has prefaced each chapter by an introduction in order that those not familiar with the underlying theory may readily obtain a working knowledge of it. Even at the expense of increasing the bulk of the book this valuable feature might have been amplified to some extent so that the text would almost have fulfilled a double purpose. Alternatively the appendix might form a useful place where such an extension could easily be accommodated.

The volume is divided into four chapters, the first deals with the dielectric constants of solids, liquids and gases, the second with the Debye theory and the third with molecular refraction. The final chapter is concerned with the measurement of the permanent electric moment of molecules. The literature is surveyed up to 1935.

H. W. MELVILLE.

Dipole Moments: Their Measurement and Application in Chemistry. By R. J. W. LE FÈVRE, D.Sc., Ph.D., F.I.C. Methuen's Monographs on Physical Subjects. [Pp. vi + 110, with 28 figures.] (London: Methuen & Co., Ltd., 1938. 3*s*. 6*d*. net.)

THE present book, like many of the others in the same series, fills a gap caused by the rapid development of a particular field of research. The first three chapters and the final one are devoted to a discussion of the theory and practice of the measurement of the dipole moment of a single molecule from observations on the dielectric constant and/or the refractivity of a gas or solution of those molecules. The discussion of the theoretical side is not wholly satisfactory, the limitations and the assumptions of the methods employed not being sufficiently stressed; nor are the modifications due to the quantum theory mentioned (*e.g.* the explanation of the curious anomaly in ammonia). The remaining two chapters are concerned with the application of the results of dipole determinations to questions of molecular structure. In particular, the author has considered the evidence from dipole moments for the recent theories concerning the existence of "resonance" and of free rotation in simple organic compounds. The author seems to prefer the chemical concept of "mesomerism" to the more fundamental physical one

of "resonance." In a *physical* monograph one would have expected the work of Pauling to have been referred to as the standard and fundamental treatment of such matters. There is a handy table of dipole moments of some two hundred simple molecules. This very useful little book will be welcomed by all interested in questions of molecular structure.

G. B. B. M. S.

Numerical Problems in Advanced Physical Chemistry. By J. H. Wolfenden. [Pp. xx + 227.] (Oxford: at the Clarendon Press; London: Humphrey Milford, 1938. 7*s*. 6*d*. net.)

Students and teachers of physical chemistry generally agree that the working of numerical examples is the one method of acquiring a satisfactory knowledge of their subject. But however unanimous in theory, most of them are only too happy to leave to others the practice of this declared virtue. The reason for this discrepancy is plain when some of the older books on chemical calculations are examined; the reader is not inspired to seek pencil and paper and set to work, rather is he reminded of a detention period at school where a certain time must be passed in uncongenial exercise. Any attempt to change this tradition and approach the subject in a novel manner deserves encouragement.

Dr. Wolfenden's book provides material for the calculation of quantities in which one is really interested. He has selected 130 original publications on physical chemistry which serve as illustrations of most types of recent research. The title, name of author and reference are given and a summary of the data in the paper follows. The "problem" is to recalculate the author's results. This method of presentation takes considerable space and the instructions on methods to be adopted are correspondingly brief. The reader is thereby challenged to solve the problem for himself, while the notes appended indicate the most common errors should the first attempt prove incorrect. Additional help is provided by a table which gives the chapters in a number of common text-books which deal with the subject matter of each example. Finally the answers are given in detail at the back of the volume. In the thirty-five problems, chosen at random, that the Reviewer attempted no serious error was discovered.

The book can be recommended to everyone who is interested in the development of physical chemistry and its modest price should ensure that it is in the possession of every university student who is specialising in chemistry.

R. H. P.

Principles of Organic Chemistry. By H. P. Starck, M.A. (Cantab.). [Pp. viii + 664, with 58 figures.] (London and Glasgow: Blackie & Son, Ltd., 1938. 12*s*. 6*d*. net.)

Text-books on chemistry for medical students generally fall into two classes: (*a*) those which are packed with unintelligible formulæ such as those of the latest in vitamins A to Z, and of the special favourite, chlorophyll; (*b*) those "written for medical students"—this phrase in the Preface being the chief clue to the crime. The above work belongs to neither class. The author has carefully avoided mystifying complexity, and written a book which should be intelligible and particularly useful for medical students, without decreasing its value for the ordinary chemistry student.

Its use to the medical student is apparent in the special consideration given to chloroform (p. 94), ether (p. 140), "Use of Alcohol in Biological Work" (p. 118), tests for alkaloids (p. 630), etc. The general student is well catered for, the early portions dealing with physical chemistry being made clear by numerous examples. The section devoted to petroleum (pp. 65–8) is unusually interesting. Tables of comparative properties of compounds are numerous, well done and are invaluable aids to revision. The explanation of the principles underlying the use of the Polarimeter (p. 260) is a welcome insertion in a work of this size: it is simply and, rare accompaniment, efficiently done. The chapter (pp. 477–86) on the Grignard reagent is excellent. The study of heterocyclic compounds is wisely omitted.

Points deserving mention are: On p. 641, 100 g. is a printer's error for 1000 g.—actually the Tables are of comparatively little value to students for whom the book is written. On p. 379, the modes (*a*) and (*b*) of representing the formula of benzene are apt to mislead a student, and, anyhow, are never used. We cannot agree that printing the benzene hexagon in an elongated form "saves space"; the practice is common but serves no useful purpose, and introduces disadvantages which are avoided by using, as Nature does, the regular hexagon. The Ladenburg formula for benzene (p. 382) is not worth mentioning unless it is interpreted. Dewar's formula lacks (as printed) the diagonal bond. It is unfortunate that the conventional writing of a *di*sodium derivative of malonic ester, etc. (p. 288), persists in all text-books and even in authoritative publications such as *The Life and Work of Professor William Henry Perkin* (Chem. Soc., 1932, pp. 47, 69—note the covering statement). Perkin himself introduced this misleading and inaccurate formulation (*Berichte*, 1884, **17**, 54), but corrected it later (*Journ. Chem. Soc.*, 1885, **47**, 807) when he wrote:

$$2CHNa\!\left<\begin{matrix}COOEt\\COOEt\end{matrix}\right. + \begin{matrix}CH_2Br\\CH_2Br\end{matrix} = \left.\begin{matrix}CH_2\\|\\CH_2\end{matrix}\right>C\left<\begin{matrix}COOEt\\COOEt\end{matrix}\right. + CH_2\left<\begin{matrix}COOEt\\COOEt\end{matrix}\right. + 2NaBr$$

The last quarter of the book is devoted to practical work. In testing for Cl (p. 493) in presence of N, Clark's test (*Journ. Chem. Soc.*, 1936, 1050) is preferable to that given—halide precipitates are permanent but cyanides dissolve in mercurous nitrate solution.

Prolific distribution throughout the volume of questions mostly taken from Papers set in Public Examinations, render the book a comprehensive production.

S. HORWOOD TUCKER.

Laboratory Technique in Organic Chemistry. By AVERY ADRIAN MORTON. International Chemical Series. [Pp. x + 261, with 122 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1938. 15*s*. net.)

THE conventional text-books on practical organic chemistry, whether they deal with "preparations" or "identifications" or both, do not as a rule provide sufficient instruction in manipulative technique. Beginners may get their first notions on the simplest laboratory practices from such text-books, but at a later stage supplementary information and constant guidance from the demonstrator or teacher is required by the majority of students. Advanced workers and researchers in the field of modern organic chemistry have had in the past to rely mainly upon German standard works of reference

such as those by Lassar-Cohn and Houben-Weyl. These books are bulky, expensive and not very conveniently arranged—moreover, they soon become out of date. There is definite need for a handbook of reasonable size which is sufficiently comprehensive and well referenced. These requirements are fulfilled by Associate-Prof. A. A. Morton's excellent little work which, so far as the Reviewer is aware, is the first of its kind in the English language. The purpose of the book is "to improve the student's understanding of ordinary laboratory manipulations and to widen the research worker's knowledge of the apparatus at his command." It consists of a compact yet detailed treatment of theory and practice of fundamental operations, together with brief descriptions from the original literature including recent advances. The main chapter headings are: drying and drying agents, melting-point, boiling-point, fractional distillation, vacuum distillation, steam distillation, crystallisation, filtration, adsorption, extraction, special methods and apparatus, and experiments. The discussion of these matters falls into 189 sections of the book, which is well supplied with explanatory sketches and many useful tables. There is also a good index. Although this small volume is not intended as a bibliographical survey it contains, nevertheless, over 400 literature-references which, judging from a comprehensive selection verified by this reviewer, are both accurate and relevant. Treatment of individual subjects is throughout adequate and some chapters, for instance those on drying and drying agents and vacuum distillation, are exceptionally well done. Classical micro-methods receive due recognition wherever possible, and modern developments such as the molecular still and chromatographic adsorption are fully described and explained. Some useful information has been incorporated from the results of unpublished researches by the author and others. The text is well set out and errors are few and fairly obvious, excepting p. 162, line 6, where 0·15 g. should read 15 g. and p. 205, line 4, where 55 mm. should read 55 cm.

This convenient and up-to-date laboratory handbook will be much appreciated by research chemists and teachers of advanced practical chemistry.

W. D.

Notes on Qualitative Organic Analysis. By F. R. Storrie, Ph.D., A.I.C. Dent's Modern Science Series. [Pp. x + 68.] (London: J. M. Dent & Sons, Ltd., 1938. 2*s.* 3*d.*)

Dr. Storrie's "notes" on qualitative organic analysis are obviously intended for the use of beginners at laboratory work in organic chemistry, since the author has confined his attention to the methods of detection of a brief list of well-known organic substances. The typical reactions of the chief organic radicals are briefly summarised, and emphasis is laid on the preparation of crystalline derivatives by operations carried out on the small scale. Attention is wisely directed to the use of such modern reagents as 3 : 5-dinitrobenzoyl chloride, 2 : 4-dinitrophenylhydrazine and *p*-nitrobenzyl bromide, and details for the preparation of these substances have been included. Teachers will be interested to find that Middleton's reagents (sodium carbonate + sucrose, or zinc dust + sodium carbonate) are recommended for use in detecting elements by the fusion process in place of sodium metal.

There is always a danger that books like this which give "notes" for laboratory guidance will tend to foster the bad practice of the mechanical application of tests by students who may fail to connect together intelligently

their theoretical knowledge and their laboratory experience. In his "Introduction" Dr. Storrie advocates that teachers using this book should recommend to students that they should gain chemical knowledge by taking a known member of each group of substances described in the notes and carrying out with it all the tests that are described. The Reviewer most strongly supports the adoption of such a procedure in the laboratory.

W. A. W.

Fundamentals of Biochemistry. By CARL L. A. SCHMIDT, M.S., Ph.D., and FRANK WORTHINGTON ALLEN, Ph.D. [Pp. xvi + 388, with 29 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1938. 18*s*. net.)

THE parts of this book deal respectively with biological variation and the analysis of experimental results, standard solutions, hydrogen ion concentration, neutrality regulation in the body, enzymes, mineral metabolism, vitamins, endocrines, chemistry and metabolism of the lipids, bile, chemistry and metabolism of the carbohydrates, chemistry of aminoacids and proteins, metabolism of proteins, urine and blood analysis and, finally, energy exchange. Each part contains a clearly written, succinct account of the section of the subject with which it deals, followed by a detailed description of a series of carefully chosen experiments to be performed by the student. The theoretical and practical sections are complementary, each helping to explain and clarify the other. The experiments throughout the book are divided into two series, the one containing those suitable for a first course, the other series containing the rather more elaborate experiments to be carried out after some experience has been gained. Each of the fifteen parts begins with references to literature—usually to recent reviews or advanced text-books—with the object of encouraging the student to read up the subject for himself. At the end of the book are appended useful tables for calculating the calorific values and nitrogen contents of various foodstuffs. Even though some of the articles included are more familiar in America than in Britain, the information given will be found very useful by anyone who may want to work out the dietary value of a meal. In a didactic manual of this kind, where clarity and precision are so important, there is often a tendency to over-simplify and to make statements unduly dogmatic. The authors of this book have contrived with surprising success to avoid this pitfall. They are lucid and precise, and yet, for the most part, strictly accurate. The generally able way in which the volume has been put together, the sensible arrangement and clear exposition, should recommend it to many engaged in the teaching of biochemistry to students of science or medicine.

W. O. K.

Annual Review of Biochemistry. Vol. VII. Edited by JAMES MURRAY LUCK. [Pp. ix + 571.] (Stanford University P.O., California: Annual Reviews Inc.; London: H. K. Lewis & Co., Ltd., 1938. $5.00.)

THE high reputation already established by this Annual Review is well sustained by the present volume. The policy is continued of including articles not only on the main branches of biochemical research, but also on a few selected special topics in which noteworthy advances happen to have been made during the past few years. Included in the present volume for

instance, we find a discussion of Organic Insecticides by F. B. LaForge and L. N. Markwood, of Growth Regulators in the Higher Plants by P. Boysen Jensen, and of The Terpenes, Saponins, and Closely Related Compounds by W. A. Jacobs and R. C. Elderfield. The usual chapter on The Chemistry of Bacteria is this year written by W. H. Peterson and M. J. Johnson. The other nineteen chapters deal with the usual wide range of topics, Oxidations and Reductions, Structural Chemistry, Enzymes, Hormones, Vitamins, Muscle Chemistry, Pigments, and so forth, and a list of contributors includes many names familiar to biochemists all the world over.

The writers of these reviews, who are all experts on their subject, may be expected to criticise as well as to compile. Mere compilation is already done efficiently by abstractors and indexers in at least three different countries, and it is not the purpose of these reviews to supplant either abstracts or indexes, nor to make unnecessary the study of the original literature. The readers for the most part are themselves biochemists, and they wish to know how the reviewer, as a specialised biochemist, interprets the year's results achieved in his own field. The editors are obviously in sympathy with this point of view and they are to be congratulated on the success which they have attained in the difficult task of giving it effect. It is probable, however, that most readers would welcome further modifications in the same direction. This is not of course a criticism of the book, but rather a personal expression of opinion which may encourage the editors to continue the policy they are pursuing with marked success.

W. O. K.

The Chemistry of Petroleum Derivatives. Vol. II (Supplementary). By CARLETON ELLIS. [Pp. 1464, with 354 figures.] (New York: Reinhold Publishing Corporation; London: Chapman & Hall, Ltd., 1937. £5 net.)

THE petroleum world is being well served on the technical and scientific side. There is an active output of research which is published in appropriate journals; *The Science of Petroleum*, in four splendid volumes, has recently appeared; in 1937, Egloff's *Reactions of Pure Hydrocarbons* was published; and in 1934, we expressed high approval of *The Chemistry of Petroleum Derivatives*, by Carleton Ellis.

Now, we are invited to examine Vol. II by the same author, which continues the subject matter of the previous volume, covers the advance during the last few years, and supplies some additional references to earlier work.

The ordinary standards and practice of criticism tend to wilt before such an accumulation of knowledge as has been brought together. The Reviewer's business is to tell the public whether, in his opinion, a book is good or bad, and why. There is no difficulty in the present case in deciding that the book is good, very good; but to say why is only possible in the most general terms. It would be useless even to go through it chapter by chapter—there are 54 of them, each with anything between 60 and 300 references. The number of pages of text is 1343, of Name Index 27 (with nearly 6,000 entries), and of Subject Index 91 (with about 13,000 entries). Thus, concentration defeats detailed examination.

This tremendous accumulation of facts is a proof, if proof be needed to-day, that petroleum is no longer the inert material that it was once considered to be, responding only to such violent reagents as sulphuric and nitric acids,

the halogens, and the rest. Not only is it, in truth, reactive to milder treatment, but nowhere in technology and the laboratory is the use of catalysts more highly developed; they are employed not only in polymerisation, but also in such opposite molecular transformations as synthesis and cracking. And the most remarkable application of advanced science is the use of the Raman effect in the study of cracked gasolene.

Amongst the new matter in this volume is the use of petroleum products in anæsthesia, thermodynamics, synthesis through carbon monoxide and hydrogen, and petroleum bitumen; together with the description of a host of other industrial products. A truly admirable and valuable book.

PERCY E. SPIELMANN.

The Chemistry and Technology of Rubber Latex. By C. FALCONER FLINT, Ph.D., D.I.C., A.I.C., A.R.C.S. [Pp. xx + 715, with frontispiece and 146 figures, including 36 plates.] (London: Chapman & Hall, Ltd., 1938. 42*s*. net.)

ALTHOUGH Dr. Flint has based his work on Georges Génin's *Chemie et Technologie du Latex de Caoutchouc*, it may definitely be stated that the present publication is essentially a new book. It deals in a comprehensive manner with every aspect of latex technology from plantation control to the use of latex in most recent industrial developments. The author has devoted special attention to practical working instructions and detailed recipes which make the book of great value to all interested in the applications of latex.

The processes are lucidly explained and their operation criticised and discussed with the minimum of chemical theory.

The subject matter in this treatise is most carefully arranged and deals first with plantation operations and then with the utilisation of latex in the manufacture of goods by dipping, by electro-deposition and by the impregnation of fibres. The preparation of sponge, latex-bound upholstery, artificial leather, and of artificial latices and the testing of latex are included. Although it occupies over 700 pages, the book is a readable treatise, with abundant references to the original literature and can be thoroughly recommended.

If any criticism can be offered it is only of a minor character and concerns the references. It would have been better if the more usual abbreviations and method of giving the references had been used. The book is well printed and illustrated and Dr. Flint is to be congratulated upon its compilation.

T. J. D.

Chemical Dictionary. By INGO W. D. HACKH, A.M., F.A.I.C., F.R.S.A. With the collaboration of JULIUS GRANT, M.Sc., Ph.D., F.I.C. Second edition. [Pp. x + 1020, with numerous illustrations.] (Philadelphia: P. Blakiston's Son & Co., Inc.; London: J. & A. Churchill, Ltd., 1938. 48*s*. net.)

IT is a poor writer whose preface gives a reviewer nothing to quote, but Mr. Ingo Hackh does not disappoint us, for he begins thus: "The words of Berzelius a hundred years ago hold true to-day; namely that 'the devil should write books on chemistry, for every few years the science changes.'" This hard saying finds an echo in the heart of every struggling lexicographer, and it is no mean compliment to Mr. Hackh to say that he has achieved remarkable success in his attempt to cope with the diabolical swiftness of the flood of new knowledge.

This "Chemical Dictionary" is one in which the word "chemical" is liberally interpreted. All the related sciences are swept into the net, and a colossal book is the result—over 1000 pages of small (but very clear) type.

A feature of the work is the elaborate, and exceedingly useful, system of tables, detailed lists of enzymes, vitamins, hormones, indicators, resins and balsams, hazardous chemicals, tannins, and so forth, and less expected items such as the Greek alphabet, the group numbers of bacteria, a spectral classification of the stars, and a table showing the way in which chemical research is distributed between the countries of the world, as well as the variation in research intensity, *i.e.* the number of chemical papers published in each country per million inhabitants.

Some of the entries are curious, such as the "Miner's inch," which proves to be the volume of water flowing in 24 hours under certain conditions through a square inch hole in a plank of wood. Again, under "Dope" we get four meanings, the fourth of which reads "colloquially: information, experimental results"! Oddly enough, the use of the term "dope" for anti-knock material is not mentioned.

Rhenium is boldly stated to be a "noble metal of the platinum group," a verdict which will not be accepted without qualification by most chemists. It seems strange to find phosphorus described (*s.v.* Brand, Hennig) as "the first non-metallic element" discovered, and Pliny the Elder, statesman and friend of Emperors, might have felt himself inadequately set out as "a Roman soldier noted for his scientific observations and writings."

The book is beautifully produced, and misprints seem to be rare, though the Reviewer has noticed "digitoxegenin" (*s.v.* digilanide), "trypase" (*s.v.* enzyme), Taylor, Hugh Stutt (for Stott), and the entry "Badouin's reagent. See Baudonin," where Baudouin is presumably intended in each case. Under "hormone" a cross-reference is given to "chalone," a term which is not to be found, while "sieve" might well have a cross-reference to "fineness," under which word a table of sieves is given.

To incorporate the multitudinous new names and terms of the last ten years is a difficult task, and Mr. Hackh has made a gallant effort to cope with it. A survey of the letter A reveals that he has succeeded in including aglucones, avertin, ameliaroside, arsepedine, asebotin, ascorbic acid, astacin, adipoin, actiniasterol, atebrin, allotelluric acid, aninsulin, aneurin, and antheraxanthin, all recent coinages, but has not catalogued ascosterol (*Ann.*, 1929, **473**, 300), ataxitic (*Kolloid Z.*, 1931, **56**, 71), ajmaline (*J. Ind. Chem. Soc.*, 1931, **8**, 667), atlantone (*Helv. Chim. Acta*, 1932, **15**, 1481), ahouain (*J. Biol. Chem.*, 1934, **105**, 231), ambaline (*Univ. Philippines Nat. Appl. Sci. Bull.*, 1933, **3**, 353), anomerism (*Kong. Norske Vidensk. Selsk. Skr.*, 1933, No. 7, 1), or austinite (*Amer. Min.*, 1935, **20**, 112).

It would be wrong to review a dictionary without noting defects, but it would be better not to review this book at all than to convey to the reader an impression that it is not a great piece of work. No chemical library should be without it, and every chemist who buys it will find it a constant help and source of interest. Not the least attractive feature, since we are all more interested in human beings than in anything else (even Chemistry), is the series of biographical notices, often with portrait attached. The author is thoroughly international in his outlook and no bias in favour of American chemists could be detected.

K. C. B.

Textile Testing. By JAMES LOMAX, F.I.C., Officer in charge of the Yarn and Textile Testing Bureau at University College, Nottingham. [Pp. viii + 176, with 42 figures, including 12 plates.] (London, New York, Toronto: Longmans, Green & Co., 1937. 7*s*. 6*d*.)

THE most obvious field of activity for scientific workers in the textile industries is provided by "testing." Here is ample scope for the microscopist, physicist and chemist, and no more so than in what would appear to be the most elementary tests. A key dimension in textile testing is yarn thickness, which, because of the rather indeterminate profile of most yarns, is not usually measured by a device like a wire gauge, but by relating weight to a unit length. This sounds simple enough, but actually is one of the least satisfactory operations in textile testing. It is possible for two observers to obtain widely different results for the length measurement, because yarn can be stretched. And the weight of the yarn is subject to variation because textile fibres are hygroscopic and therefore influenced by atmospheric conditions.

It is highly important, therefore, that officials, like the author, who have charge of testing laboratories where routine practices are more or less standardised, should describe and justify their methods. Such a book is long overdue. English textile literature has been lamentably lacking in this department.

In 176 pages the author has done no more than he claims, namely to provide a guide covering the elements of the subject. The book has received much criticism in textile circles and it is to be hoped that the author and publisher will soon take advantage of many suggestions that have been made for its improvement. It holds the field at present as the only general text-book in English on textile testing that merits any consideration from scientific workers.

J. C. WITHERS.

An Introduction to the Study of Spinning. By W. E. MORTON, M.Sc.(Tech.), F.T.I. [Pp. viii + 267, with 162 figures.] (London, New York, Toronto: Longmans, Green & Co., 1937. 12*s*. 6*d*. net.)

A HIGHLY significant change in the approach to the study of textiles has been in progress for more than twenty years. Formerly, the various branches were rigidly divided according to the fibres—a cotton man, for example, seldom giving himself to the study of wool—and, in the cotton industry at least, the boundaries between spinning, weaving and the chemical processes such as bleaching and dyeing were not often overstepped except by students in the more "academic" schools. Further, in the study of the mechanical processes the tendency was to lose sight of principles, and almost of the fibre or yarn, in a detailed study of machine parts. The old teaching produced excellent craftsmen, but it has been felt for many years that broader training is necessary if the industries are to be saved from stagnation, and the modern tendency to use more than one type of fibre in a cloth—not for adulteration, but to secure novel and pleasing effects—also demands at least a general knowledge of all the major fibres and processes.

The Municipal College of Technology in Manchester has taken a leading part in reorganising the teaching of textile technology on these new lines and the author, as professor of the subject, has now made a most important contribution to textile literature by providing the first of an entirely new type of text-book.

The first part (123 pages) describes the production and properties of wool, silk, cotton, flax, hemp and related fibres, and the rayons. The second part (126 pages) is devoted to the "foundations of spinning technology" and is unique in that it traces the principles of the various operations from the original handicraft appliances, and shows how the evolution of modern spinning machinery has been influenced by the character of the raw material. The book stops at the conversion of fibres into simple yarns, but it is to be hoped that companion volumes in the same spirit will continue the story.

The book has been well received in the textile press, though most reviewers have found technical points for criticism. The present reviewer, however, would in this place stress the value of the book to the general scientific reader. No better account of the production and qualities of the various fibres in so few as 123 pages is available and the author's exposition of the principles of spinning is the only one that can claim to be scientific. The author must be congratulated on an excellent piece of writing and on the production of the most significant addition to British textile text-books that has been made for many years.

J. C. WITHERS.

Intermediate Readings in Chemical and Technical German. By JOHN THEODORE FOTOS and R. NORRIS SHREVE. [Pp. xliii + 219.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. 9*s.* 6*d.* net.)

THERE is no need to-day to emphasise the value of a knowledge of scientific and technical German, and several text-books specially written for science students are now available. This book is one (the third) of a series of four prepared jointly by the School of Chemical Engineering and the Department of Modern Languages at Purdue University. It is intended to follow volumes one and two and to be studied in the third semester. It consists of 34 pages of grammar and 50 reading exercises chosen from Ullmann's *Enzyklopädie der Technischen Chemie* arranged in alphabetical order. Words used for the first time are explained in footnotes (to avoid constant references to the vocabulary at the end of the book), and there are also explanatory notes dealing with idiomatic expressions, special points of grammar, etc. The whole is well planned, the text very thoroughly examined and explained and suitable for anyone already possessing some knowledge of grammar and an elementary vocabulary, but it is improbable that English students would find time to work through the whole four-volume course.

J. N. S.

GEOLOGY

Outlines of Geology. By C. R. LONGWELL, A. KNOPF, R. F. FLINT, C. SCHUCHERT, and C. O. DUNBAR. [*Outlines of Physical Geology*, pp. vi + 356, with frontispiece and 297 figures; *Outlines of Historical Geology*, pp. vi + 241, with frontispiece and 151 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1937. 20*s.* net.)

THIS book is a combination in one volume of the *Outlines of Physical Geology*, by C. R. Longwell, A. Knopf, and R. F. Flint, published in 1934 (reviewed in SCIENCE PROGRESS, XXIX, 1935, 566), and the third edition of the *Outlines of Historical Geology*, by C. Schuchert and C. O. Dunbar, published

in 1937, and reviewed in this volume of SCIENCE PROGRESS (see below). The preface states that the book has been made in response to a widespread demand from teachers of geology for a brief text-book covering the salient features of physical geology and historical geology. It may, of course, be a convenience for the purposes of study to have these two excellent texts combined in one volume ; but the fact that the price of the combined volume is 11s. less than that of the two separate volumes will probably weigh most with the student.

G. W. T.

An Introduction to Historical Geology. By W. J. MILLER. Fourth edition. [Pp. xii + 499, with frontispiece and 372 figures.] (New York: D. van Nostrand Co., Inc.; London: Chapman & Hall, Ltd., 1937. 15s. net.)

THIS work has been thoroughly revised for the fourth edition, many parts having been completely rewritten and various new topics introduced. The illustrations are much more numerous compared with previous editions, and they have been greatly improved in reproduction. Instead of the life of each period being discussed separately, in this edition the life of each era is dealt with as a whole, thus avoiding unnecessary repetition of details in common, and presenting a much more unified picture of the evolutionary changes that have occurred.

After an introductory section of eight chapters dealing with general principles, and with the Archæozoic and Proterozoic eras, succeeding sections treat of Early Palæozoic time (4 chaps.), Late Palæozoic time (6 chaps.), the Mesozoic era (5 chaps.), and the Cenozoic era (3 chaps.). The mode of presentation in each section is to describe the rocks and physical history of the formations that comprise the era dealt with, and then to discuss the life of the era as a whole. Valuable summaries of Palæozoic and Mesozoic history and life forms are given.

The literary style of the book is concise and lucid, and much use is made of quotations from older works. By running to a fourth edition this text-book, which was first published in 1916, has proved its usefulness to the class of students it was designed to reach. Its subtitle is: "With Special Reference to North America," and the book thus appeals more to students in the New than in the Old World. Nevertheless, the general stratigraphical and historical matter, for which the stratigraphy of North America affords the detailed background, is so well arranged and so excellently illustrated, that British students would find profitable reading in the book.

G. W. T.

Outlines of Historical Geology. By C. SCHUCHERT and C. O. DUNBAR. Third edition. [Pp. vi + 241, with frontispiece and 151 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1937. 12s. 6d. net.)

THE third edition of this well-known elementary text-book which is based on the authors' larger volume on Historical Geology published in 1933, has been completely rewritten and rearranged, so as to give the student, in one brief course, a general survey of the history of the earth.

The systematic treatment of the subject, period by period, has been

abandoned in this edition, since the student is intended only to obtain "glimpses of the vast panorama." "He needs a telescope rather than a microscope, with which to gain, from a distance, a view that has depth and perspective, rather than detail." To further this aim the book is written in an easy popular style, and is replete with well-chosen illustrations. For instance, the first chapter begins with an engrossing description of the carcass of the woolly mammoth, which was discovered preserved, with flesh and entrails still fresh, in the natural cold storage of the frozen Siberian swamp.

Part I—"The Nature of the Evidence"—contains three chapters on "A Living Record of the Dead," "The Record in the Rocks," and "The Scale of Geologic Time." In Part II—"Earth's Changing Features"—is given a continuous story of the physical history of the earth in five chapters. The concomitant history of life on the earth follows in Part III—"The Pageant of Life"—with three chapters. The fascinating story ends with Part IV—"The Coming of Man"—in one chapter.

We can foresee this almost ideal text-book converting a large number of students to an enthusiastic lifelong interest in the science of geology.

G. W. T.

The Petrology of the Sedimentary Rocks. By F. H. Hatch, O.B.E., Ph.D., and R. H. Rastall, M.A., Sc.D. Third edition, revised by Maurice Black, M.A. [Pp. 383, with 75 figures.] (London: George Allen & Unwin, Ltd., 1938. 15*s.* net.)

The last edition of Hatch and Rastall's *Petrology of the Sedimentary Rocks* was published fifteen years ago. The present edition, while mainly following the lines laid down in the original work, has been completely revised, and largely rewritten by Mr. Maurice Black. The changes are so numerous and so drastic that the work must be treated as new. In particular, the chapters on the metamorphism of sediments which were deemed necessary in the earlier edition have been omitted.

An introductory chapter deals with the environments of deposition and with the influence of climate on sedimentation, while the second chapter treats clearly of the origin and classification of sedimentary deposits. Thereafter, successive chapters deal respectively with Fragmental Deposits; Rudaceous Deposits; Arenaceous Deposits; Argillaceous Deposits; Ferriferous Deposits; Calcareous Deposits—Limestones; Calcareous Deposits—Magnesian Limestones and Dolomites; Organic and Diagenetic Siliceous Deposits; Chemical Deposits; Carbonaceous Deposits; Phosphatic Deposits; Pyroclastic Deposits; Deposits of Modern Oceans; Diagenesis and Related Processes; Weathering, Residual Deposits and Soils. On the whole these chapters are very well done, the latest information has been utilised, and the topics have been illustrated by many quite new and unhackneyed figures. Mr. Black's recent research work has ensured an excellent and original chapter on the Deposits of Modern Oceans.

The only points of criticism that occur to the Reviewer are that greywackes are rather inadequately treated, being only accorded a passing mention, although these rocks, so common especially in Lower Palæozoic formations, are at least as important as arkoses, the description of which occupies two pages. Silt and siltstone, likewise, although only little less important than sand and sandstone, or clay and shale, are barely mentioned.

In the treatment of carbonaceous deposits, Hickling's recent work seems to have been largely overlooked.

But these are merely small blemishes in an excellent piece of work, on which the author is to be cordially congratulated. His book will remain the standard text on the petrology of sedimentary rocks for a long time. The value of the work is enhanced by a comprehensive bibliography and a full index.

G. W. T.

Field Tests for Minerals. By E. H. DAVISON, B.Sc., F.G.S. [Pp. viii + 60, with 12 plates and 3 figures.] (London: Chapman & Hall, Ltd., 1937. 7*s*. 6*d*. net.)

MANY books on methods of mineral determination have been published, but that of the head of the Department of Geology and Mineralogy at the Camborne School of Mines is different. The tests described are of three types: namely, the ordinary blowpipe tests; "spot" tests, which are reactions between a solution of the mineral and an organic reagent which result in the production of a characteristic colour or precipitate; and micro-chemical tests. The Reviewer admits that the "spot" tests are new to him. The only point that occurs to him is that some of the more unusual organic reagents mentioned may be difficult to procure.

The second part of the work deals with the determination of minerals in hand-specimens by their physical properties, and follows familiar lines. A set of determinative tables based on physical properties concludes the book. The second part is illustrated by twelve beautiful plates of mineral structures.

The tests are designed for easy and rapid application in the field; and the prospector, of course, requires a portable apparatus for the numerous reagents and implements that are needed. The book is bound in flexible covers, but is a trifle large for the pocket. This is one of the best books we have seen on the subject.

G. W. T.

BOTANY

Vergleichende Morphologie der höheren Pflanzen. Band I: Vegetationsorgane. 1. Teil, 3. Lieferung. Von DR. WILHELM TROLL. [Pp. xii + 446, with 368 figures.] (Berlin: Gebrüder Borntraeger, 1937. RM.39.50.)

THIS number completes the first part of Volume I of this work which is concerned with the vegetative organs. The present part is mainly devoted to the branch systems of the seed plants, to their symmetry relations and growth forms; to orthotropous and plagiotropous shoot systems and storage organs; to twining plants and those climbing by means of tendrils, and also to the morphology of stem-succulents.

Like the previous parts issued, the work is profusely illustrated with photographic reproductions, drawings, and diagrams, which materially add to the utility of this comprehensive treatment. The subject matter is too diverse to permit of any detailed consideration, but particular attention may be called to the section on subcotyledonary branching, as exhibited, for example, in the genera *Linaria* and *Euphorbia* and in the Orobanchaceae

and Balanophoraceæ; also to the section on storage organs which is a particularly useful summary of an interesting topic.

The bibliography of 550 references which concludes the part is a useful guide to the literature of the subject and is indicative of the thoroughness of the treatment.

E. J. S.

Farm and Garden Seeds. By S. P. Mercer, B.Sc.(Agric.), N.D.A. With a Section on the Seeds Act, 1920, by A. W. Monro, C.B. Agricultural and Horticultural Handbooks. [Pp. 205, with 14 plates and 4 figures.] (London: Crosby Lockwood & Son, Ltd., 1938. 10*s*. 6*d*. net.)

This work comprises an elementary account of the nature of seeds, followed by a short consideration of commercial seed production, methods of seed cleaning and seed testing.

The most important and generally useful section is the illustrated account of the commoner crop and weed seeds, which is accompanied by fourteen plates in which are figured 129 varieties of "seeds." Data are furnished in the text as to the characteristics of the various seeds, but it would have added to the utility of the plates if the respective magnifications had been appended to the figure descriptions. The fifth and final chapter by A. W. Monro is concerned with the provisions of the Seeds Act of 1920 and its administration.

It is a book for farmers, gardeners and horticulturists, rather than for the more technical student, but contains a considerable amount of information that the last named may find useful.

E. J. S.

Timber: Its Structure and Properties. By H. E. Desch, P.A.S.I., B.Sc., M.A. [Pp. xxii + 169, with 90 figures, including 27 plates.] (London: Macmillan & Co., Ltd., 1938. 12*s*. 6*d*. net.)

To the user of timber much recent research on wood, even if comprehensible, must appear detached and of doubtful value; such a state of affairs, however regrettable, is inevitable; papers are often so specialised that their significance is lost on all but a specialist. The author, himself an active investigator, is fully aware of this position, and has been convinced that a summary of such work, giving, in effect, a résumé of our present knowledge of wood, in language intelligible to the layman, would prove of value to the user of timber, to the architect, the contractor, and others. The result is a readable, well-illustrated book, treating of the properties and structure of wood and such topics as seasoning, defects, preservation and grading.

On the biological side the book is not entirely free from loose and inaccurate statements, as, for example, the reference to woodlice as insects, and the statement that most herbs produce seeds in a single season. Bearing in mind the object of the book, it may, perhaps, be objected that an undue proportion is devoted to wood structure, although, in fact, structure appears to be more prominent than it really is because of the lavish manner in which this part of the book is illustrated. Moreover, this section should appeal to the botanist, who will discover therein some useful information on the structure of the secondary xylem.

F. W. J.

Theory and Practice in the Use of Fertilizers. By FIRMAN E. BEAR, Ph.D. Second edition. [Pp. x + 360, with 63 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. 20s. net.)

BOOKS on fertilisers are none too common, and the second edition of Dr. Bear's book is therefore very welcome. Much of the book has been re-written and a quantity of new material has been incorporated and a chapter added on the subject of rare elements and trace elements as he calls them in plant nutrition. An attempt is made to cover the whole aspect of the nutrition of crop plants in so far as it is related to the soil.

It is a difficult matter to write a treatise on this subject and at the same time one which is readable. Dr. Bear has succeeded in this task—and although he has often included either masses of experimental detail or many statistics of fertiliser production, he has produced a book which not only is full of detail interesting and important to the student, but which also presents an interesting and connected whole.

The subject is introduced historically and a clear account is given of the early views on the sources of nitrogen in vegetation and of the mineral theory. The views of Whitney and Cameron are given in some detail and are interesting reading in view of the anti-erosion and anti-deterioration work now so much in the public eye.

In some respects the chapters dealing with nitrogen and mineral economy in soils are the most interesting, though they might have included more as to the effect of climate and through climate of the natural vegetation upon nitrogen and mineral status. It is only now coming to be realised how highly artificial most agricultural soils are, and how much greater are the demands which agriculture makes on them than was made by their natural vegetation. This surely is and must remain the basis of all fertiliser practice. It is unfortunate that the selected references at the end of each chapter have not been kept up to date.

The book is profusely illustrated in the American manner, but the illustrations are poor and often unnecessary. A smaller number of really useful illustrations would have served the purpose better.

No doubt the idea of including portraits of distinguished scientists at the beginning of each chapter will appeal to many—and it may be that the portraits of the earlier authorities are satisfactory, but those of living men are in some cases little more than caricatures.

These are after all minor defects and it is no small achievement to have written an important and readable book on this subject, and one which should prove of great value to student and teacher alike.

C. G. T. M.

ZOOLOGY

An Introduction to Physical Anthropology. By E. P. STIBBE, F.R.C.S. Second edition. With an Appendix by W. A. M. SMART, M.B., B.S., B.Sc. [Pp. viii + 230, with 56 figures.] (London: Edward Arnold & Co., 1938. 10s. 6d. net.)

THE first edition of this book was published in 1930. It is satisfactory to see that eight years has required a second edition. The general form has not been altered, but some additions have been made to the chapters dealing with the comparative anatomy of the primates. A note has been added to

human fossils, dealing with *Sinanthropus*, and there is an additional appendix on mathematical methods. It is perhaps somewhat unfortunate that in the latter the examples have been measured in inches and either to one-eighth or one-sixteenth of an inch. The practice of the majority of anthropologists for many years now has been to use the metric system, both for the infinite advantages from the point of view of computation and also to allow comparisons with continental workers. The transference of inches to millimetres is apt to introduce errors, quite apart from the unnecessary labour involved. Further, the majority of instruments to-day are no longer calibrated in inches. It is to be hoped that should any further edition be called for that the metric system will be adopted. No mention is made of the coefficient of racial likeness. So much work has been done in the last fifteen years or so with this coefficient, whose somewhat intricate character is puzzling to the uninitiated, that a mention at least should be made in any introduction to physical anthropology. If the author does not like it, and many do not, surely it should be fairly criticised and not just passed over in silence. The instructions for measuring remain rather inadequate and more precise definitions might well be given.

L. H. D. B.

MEDICINE

Studies on the Physiology of the Eye. Still Reaction, Sleep, Dreams, Hibernation, Repression, Hypnosis, Narcosis, Coma and Allied Conditions. By J. GRANDSON BYRNE. Re-issue with Supplement and new Index. [Pp. xii + 440, with 48 figures.] (London: H. K. Lewis & Co., Ltd., 1938. 40*s*. net.)

ONE cannot help being impressed by the immense amount of care, attention to detail, work and thought which have made Mr. Byrne's book possible. This work is well written and illustrated, but it is so full of material that the book requires much thought and careful reading to appreciate its true value.

The first part of this volume is devoted to the paradoxical pupil phenomena following lesions of the afferent paths; it deals in detail with the literature on the subject and gives much experimental evidence and discussion on the cause of these phenomena, and points out the value of this subject clinically. The interpretation of inequality of the pupils, and of changes in reaction, are of great importance in many clinical conditions, and many of the causes mentioned by Mr. Byrne are not often appreciated to their full extent by the clinician.

The second part of this book, although of interest to the ophthalmologist, is not on the whole of such practical importance; but it is of greater importance for the neurologist, and we might bring to the notice of anæsthetists the section dealing with the sequence of phenomena occurring during anæsthesia.

The method of paradoxical widening and narrowing of the lens and the theory that the ciliary muscle consists of two antagonistic parts, one of which actuates accommodation for distance and the other accommodation for nearness, together with the retractor and proptosing mechanism of the eye, may throw some light on the clinical conditions of errors of accommodation and convergence, resulting in squint, etc.

The third part of this volume deals with stimulation experiments testing the results obtained, and determining the extent of reciprocal innervation

between the affectivo-sympathetic mechanisms and the critical proprioceptive para-sympathetic mechanisms.

The fourth part deals with the little-known reactions of the pupils during sleep, coma, etc. The inherent constrictor tonus mechanism is discussed, which is of importance clinically, as it plays a rôle in various pathological conditions such as miosis, anisocoria, Argyll-Robertson pupil, etc.

The interesting condition of "feigning death," which occurs in certain insects and animals when frightened, together with hibernation, hypnosis and sleep are discussed, and the physiological explanation and the physiological state during these conditions are put forward in an interesting manner.

The last two chapters of this book are devoted to the pupillary reactions to light and the mechanism of accommodation. The author has performed much experimental work on this section and has brought forward some new factors for consideration. He again deals in detail with the theory of the mechanism of accommodation as mentioned previously and it certainly explains the deficiencies in the other existing theories.

The supplement deals with the effect of cortical stimulation on the mechanisms mediating movements of the iris and membrana tympani.

We would like to congratulate Mr. Byrne on this interesting and painstaking work.

E. E. CASS.

The Cause of Cancer. By DAVID BROWNLIE, B.Sc., F.C.S. [Pp. viii + 208.] (London: Chapman & Hall, Ltd., 1938. 7*s*. 6*d*. net.)

THE author of this little book is an organic chemist and experienced fuel technologist, and it is doubtful if he is really qualified to enter this field of enquiry where so many biologists have worked with so little success.

Surely the title is premature. Starting with the well-accepted and readily demonstrated fact that certain organic products, such as tar and soot resulting from high temperature decomposition of carbonaceous materials (such as bituminous coal, and petroleum oil used in manufacture of carburetted water gas) can produce malignant growths in experimental animals, it is legitimate and perhaps a true scientific deduction to consider that traces of the carcinogenic substances are present, not only in coal tar fractions, but also in most towns' gas—and also in smoked foods.

Is it possible that from the use of gas ovens where food in the process of cooking is exposed to and penetrated by these poisonous substances we are every day in contact with cancer-producing influence—and especially by the inhalation of virulent dust particles from the tarred motor roads? Truly the modern combustion of towns' gas has increased enormously, as shown by Mr. Brownlie's figures, and gas ovens and tarred roads are relatively quite modern developments; it would be interesting to know if there is any positive correlation between the advent and increase of these civilised amenities and the increase of cancer among the community.

P. J.

Chemistry of the Brain. By IRVINE H. PAGE, A.B.(Chem.), M.D. [Pp. xviii + 444, with 2 figures.] (Baltimore: Charles C. Thomas; London: Baillière, Tindall & Cox, 1937. 34*s*. net.)

IN some respects of its chemical composition and behaviour, brain and nerves resemble closely liver and muscle and other living tissues; in other ways,

they exhibit striking contrasts. In either case, an adequate discussion of the chemistry of the brain involves some account of the present position of our knowledge of the chemistry of animal organisms in general. This is evidently the chief reason why the present volume, although entitled *Chemistry of the Brain*, presents a picture, and a very interesting picture, of modern animal biochemistry as a whole. A second reason, no doubt, is the vivid interest which the author obviously takes in all fields of knowledge related to that in which he has himself principally specialised. It is characteristic of the book that it begins with a chapter on the life and work of Thudichum, that amazing and unappreciated genius, who laid the foundations of our present knowledge of the chemistry of the brain—a most interesting and fascinating chapter—and ends with one on the relation of brain to thought, a chapter also interesting and fascinating, though in an entirely different way. The book, though not exactly for " the man in the street," makes very good reading for the biochemist and for others who have a general knowledge of biochemistry. It contains many facts and discusses many difficult problems. Amongst its chapters is one by Dr. J. H. Quastel which deals at considerable length with the subject of oxidation in brain tissue. Dr. Page has evidently spared no effort to make this book accurate and comprehensive. As is to be expected, considerable prominence is given to the chemical findings in abnormal mental conditions. The resulting impression is that of an assortment of detailed facts, which are still for the most part unrelated to each other and to the root causes of the disturbance. But this, after all, only emphasises how incomplete and how rudimentary our knowledge of the chemistry of brain and nerve really is, and how great the need for the intensified prosecution of work of this kind. The present volume is a real contribution to this end, for it summarises the present position, and brings together in an interesting, attractive and even inspiring form, a comprehensive array of facts and references. This is a most valuable book, and can be heartily recommended to all biochemists, as well as to physicians and psychiatrists acquainted with the language of chemistry.

W. O. K.

The Essentials of Human Embryology. By GIDEON S. DODDS, Ph.D. Second edition. [Pp. x + 316, with 182 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. 20*s*. net.)

THE preparation of a new edition of this work is much to be welcomed, as a convenient handbook of the more important facts of mammalian and especially human development. The descriptions and figures are clear, and the work, unencumbered by detailed theoretical considerations, should be particularly useful to medical students, as well as to anatomists and morphologists.

G. R. DE. B.

HISTORY OF SCIENCE

Hevelius, Flamsteed and Halley: Three Contemporary Astronomers and their Mutual Relations. By EUGENE FAIRFIELD MACPIKE, F.R.A.S. [Pp. x + 140, with 4 plates, and one facsimile.] (London: Taylor & Francis, Ltd., 1937. 12*s*. 6*d*. net.)

MR. E. F. MACPIKE will be remembered for his scholarly edition of the *Correspondence and Papers of Edmond Halley*, published in 1932. In the

present work he has linked Halley with two other distinguished observers of the Newtonian age—Johannes Hevelius and John Flamsteed. The first three chapters of his book briefly trace the careers of the three astronomers in turn; and the concluding fourth chapter shows how their destinies were interwoven.

Of the three men, Hevelius is the least known to English readers, and the material for his biography is relatively inaccessible. He was a wealthy Danzig brewer, who built and equipped an observatory on the roof of his house, and who described his instruments and observations in beautifully illustrated books of his own production. The careers of Flamsteed, our first Astronomer Royal, and of Halley, his successor at Greenwich, make more familiar reading; though here, too, the author is able to include some interesting information from recently published sources, such as *Hooke's Diary*. The closing chapter describes how Hevelius was brought into touch with the English astronomers through a controversy as to the advantages of telescopic sights, and how the same dispute led to Halley's visiting the Danzig observatory. It tells also of the early friendship of Flamsteed with Newton and Halley, and of the unhappy feud which ruined it. The book is illustrated with portraits of the three astronomers, and of Hevelius' second wife, Elisabeth, who assisted her husband in his observations.

Mr. MacPike has had considerable assistance from the Royal Society Librarians, Mr. A. H. White and Mr. H. W. Robinson, who carefully revised his manuscript before its publication. The work may be warmly recommended to students of the history of astronomy, and to others interested in the brilliant period with which it deals. It contains a wealth of carefully sifted biographical information, with full references and appendices which constitute a valuable guide to the original sources, printed or in manuscript.

Two slips only have been noticed: the number 964,500 on p. 55 (line 28) needs another cipher; and the date of Gassendi's Life of Tycho Brahe, mentioned on p. 94 (footnote 3), should be 1654.

A. A.

A Hundred Years of Chemistry. By ALEXANDER FINDLAY. The Hundred Years Series. [Pp. 352, with 11 figures.] (London: Gerald Duckworth & Co., Ltd., 1937. 15*s*. net.)

IN this book, recently added to Messrs. Duckworth's "Hundred Years Series," Prof. Findlay describes the development of chemistry during the last hundred years, more especially from the time of Liebig and Wöhler, and their classic memoir of 1832 on the radical of benzoic acid, a research upon the historical significance of which he very properly lays considerable emphasis as marking the beginning of a new period in organic chemistry and the first solid foundation of a theory of organic compounds. Proceeding from a general outline of the historical background of chemistry, from Lavoisier through Dalton, Berzelius and Avogadro, Prof. Findlay proceeds to deal historically with the development of organic chemistry from 1835 to 1865, the determination of atomic weights, the classification of the elements, stereochemistry, physical chemistry in the nineteenth century, organic chemistry in the second half of the nineteenth century, the chemical industry based on coal tar, the constitution and synthesis of naturally occurring compounds, the discovery of new elements, the rare gases, radioactivity and atomic constitution, physical chemistry in the twentieth century and the development of industrial chemistry. His large canvas includes just the right amount of

detail for such an extensive scene; few readers will fail to profit from so able a survey of a century of chemistry, and no reader will rise from the book without being conscious that its author has conveyed to him a vivid impression of the vitality of the science during this period and something of its spirit as a living, growing thing. The biographical notes at the end of the book provide useful information about some of the great chemists of the past; and detailed references to the classical papers are given in each chapter. It might be noted that "Charles" (p. 152) should read "Christopher." The book can be thoroughly recommended: it provides the right kind of historical background for an immediate appreciation of chemistry in its present phase.

D. McKie.

Famous American Men of Science. By J. G. Crowther. [Pp. 414, with frontispiece and 11 plates.] (London: Martin Secker & Warburg, Ltd., 1937. 15*s*. net.)

Dr. Crowther presents here a study of the lives and scientific achievements of four famous American scientists, namely, Franklin, Henry, Gibbs and Edison. His work is inspired by the belief that the problems severally investigated by his subjects are related to the social needs and conditions of their place and time. We are therefore offered explanations of why Franklin developed the theory of frictional electricity, why Henry invented the large electro-magnet and founded the Smithsonian Institution, why Gibbs studied the theory of heat and "created the theoretical basis of physical chemistry," and why Edison produced his many inventions. The causes, according to the author, are to be sought in the needs and conditions of their contemporary American life. In other words, we have here a further application of the ideas expressed by Prof. Hessen in his *Social and Economic Roots of Newton's "Principia."* And so far, so good: the influences from economic life, from social needs and circumstances, are clearly factors in the development of science. But Prof. G. N. Clark has recently shown in his *Science and Social Welfare in the Age of Newton* that these factors are inadequate for a complete explanation. It is perhaps unfortunate that Dr. Crowther's interesting book should have appeared at this time, because the industrious and valuable research that has obviously gone to its making may be overlooked in the current criticism of the thesis that inspires it. The book can be well recommended for the thoroughness with which the author has amassed so many facts and details; and, in this respect, it should have a permanent value.

D. McKie.

MISCELLANEOUS

Under the Pole Star: The Oxford University Arctic Expedition, 1935–6. By A. R. Glen, assisted by N. A. C. Croft. [Pp. xvi + 365, with 48 plates, 4 figures and 18 maps.] (London: Methuen & Co., Ltd., 1937. 25*s*. net.)

This book is an account of the Oxford University Expedition 1935–6 to North-East Land. In the preface the author makes mention of the fact that many tales of low temperature and howling blizzards have been written before. This fact, so honestly admitted, is not swept aside by scientific matter but by the interactions between the members of the party and in noting the effect of the changing environment upon the party collectively and individually.

It is then a personal account and by following this method the diversity of the tasks and operations are knitted together. This feature is well illustrated in "In the Beginning" and in the chance meeting of two pairs (p. 246 *et seq.*). Not only is there much detailed information concerning equipment, dogs, and the work of the various parties but there is also a wealth of amusing incidents—cooking diversions in a food shortage (p. 259) and in the epic fox hunt (p. 163).

The importance of establishing stations on the ice cap is stressed. Two stations were chipped "passage by passage, room by room, out of the one thing nature had provided." At one station, Central Ice Cap, 40 tons of ice were removed during its construction. In this way a warm and comfortable house in the ice was obtained even though outside there might be a 100 m.p.h. wind or 70° of frost.

There is also a very interesting account of the underground grotto, cavern and three tunnel systems where they found water 70 feet below the surface in the coldest month of the year; not a little but hundreds of gallons evidently never frozen.

A very brief description of the scientific work accomplished is given but there is no attempt to anticipate the value of this part of their work.

Appendix I deals with the financial aspect of the expedition and it is remarkable that one so well planned and with so full a programme could be carried out at such a small cost.

There are nearly fifty beautiful photographic illustrations as well as an adequate number of sketch-maps, of which the last one is a summary of all the routes, land and sea, followed by the expedition. A folding map and an index complete this work, which by the richness of its narrative, its human appeal and its excellent illustrations will be appreciated by a wide circle of readers.

J. E. Coleclough.

Land of Ice and Fire. By Hans Ahlmann. Translated by Klares and Herbert Lewes. [Pp. xvi + 271, with 16 plates and 1 map.] (London: Kegan Paul, Trench, Trubner & Co., Ltd., 1938. 12*s.* 6*d.* net.)

In this admirable book, illustrated with well-chosen photographs and excellently translated from the Swedish original, Prof. Ahlmann of the University of Stockholm portrays in a most fascinating manner the life and indefinable atmosphere of present-day Iceland. The vegetation and flowering fields, the grey-brown streams, the mountain walls of black lava, the inhabitants and their hard struggle with an inhospitable Nature, form the components of a motley picture which is here graphically described with enough of historical material, legend and tradition to excite the appetite for more.

The first half of the book is an account of an expedition led by Prof. Ahlmann in 1936 with dog-teams to the eastern part of the Vatna Jökull, Iceland's biggest glacier. The mountains and rounded hills are drowned in a vast mass of ice which expands to the north into a central plateau as empty of life as a desert. The second half describes a journey by horse and motor-car over the coastal belt south of the glacier, a naked and inhospitable landscape of smooth, dark *sandur* which is crossed by fierce rivers and stretches down to a monotonous shore of sand and gravel. Lonely farms and isolated patches of cultivated land alone interrupt the bleak sterility.

An appendix of some twenty pages gives a popular account of some of

the glaciological results of the first part of the expedition's scientific labours. From accurate measurements, partly obtained in deep artificial pits in the snow, the expedition learned for the first time the factors which decide the movement of this glacier and the fluctuating volume of its glacier-streams, as well as the rate of ablation which is found to be much greater than had previously been thought possible.

Interesting evidence is also given of the post-glacial warm period in Iceland, when the sea was tenanted with cockle-shells, not now found in Icelandic waters, and lime trees of powerful growth lived in the country.

If the book has little to say about Iceland's Fire, save for the very interesting *Jökulhlaup* (disastrous floods occasioned by volcanic eruptions beneath the glaciers), its vivid descriptions of the ice move us to ask for another book of this kind from Prof. Ahlmann's pen.

J. K. C.

The Universe Surveyed. By Harold Richards. [Pp. xviii + 722, with 94 figures.] (London: Kegan Paul, Trench, Trubner & Co., Ltd., 1938. 12*s*. 6*d*. net.)

"The speed of the earth . . . is 18·5 miles per second, a very creditable rate for this old earth to be moving. Figure how far you go while reading this page." Such lively injunctions are characteristic of the way in which Prof. H. Richards presents the basic ideas of chemistry, physics, geology and astronomy in his book *The Universe Surveyed*. The layman with a taste for science cannot fail to find this book ideally suited to his aspirations. Against a vivid historical background the progress of science and its many protagonists come to life and the masterly fashion with which abstract ideas are "put across" cannot fail to evoke the admiration of those whose profession it is "to gild the philosophic pill."

Quite apart from the many excellent photographs, the delightful black-and-white illustrations by Mr. Burk Sauls have a unique illustrative value: the cartoon which shows why the stars rise four minutes earlier each night is a characteristic example. The way in which Archimedes' principle is whimsically demonstrated at the bathing-place with the help of a spring-balance, a personal weighing-machine, the diving-board and a friend in a bathing-dress would hold anyone's interest: it has certainly stimulated the Reviewer to try the actual experiment *in vivo*.

When we put down this book for the last time it is with a sense of having travelled far through exciting foreign lands with the perfect travelling companion: we have surveyed the universe and found pleasure and profit in our widened horizon.

For those readers who delight to quizz themselves (or their friends) the book concludes with a series of 850 questions entitled a True-False Review. Some few are childishly simple: others are like this :—"The result of the Michelson-Morley experiment indicates that one star, no matter how fast it travelled away from another star, could not avoid being overtaken by the light radiated towards it by the second."

H. Irving.

The Microscope: Theory and Practice. By Conrad Beck, C.B.E. [Pp. 264, with 217 figures.] (London: R. & J. Beck, Ltd., 1938. 7*s*. 6*d*. net.)

In recent years books on the use of the microscope have not been numerous, perhaps because it was thought that the story had been told so well that

little could be added. There is some truth in such an assumption as the complete treatises produced during the latter part of the last century may still be regarded as reliable works of reference. The book by Mr. Beck is, however, in a different category. It has been produced because the author is in an unusually favourable position to hear of the difficulties encountered by microscope users, amateur or professional, expert or novice, and has made a serious effort to meet their troubles. It follows, therefore, that the reader is provided with a treatise which is lucid in style, practical in its intention, and a sure guide to the use of the microscope in most of its applications. This does not mean that theory is neglected, for in fact the author has treated this side of the subject clearly and without needless technicalities. The optical theory of the microscope is often dealt with by those who have little knowledge or experience of its practical applications. This cannot be said of Mr. Beck's book; it is written for users of the instrument, and yet there are few who are so competent to deal with its theoretical aspects. It is indicated in the preface that some chapters may be omitted, and this is certainly a valuable hint for the novice, although most readers will benefit by careful reading of the whole book. In general, a clear explanation is provided of things a microscopist must know if he is to make good use of his apparatus and rightly interpret what he sees.

J. E. B.

National Fitness. Edited by F. Le Gros Clark, B.A. With a Foreword by Maj.-Gen. Sir Robert McCarrison, C.I.E., M.D., D.Sc., LL.D., F.R.C.P. [Pp. xii + 222.] (London: Macmillan & Co., Ltd., 1938. 6s. net.)

In his foreword Sir Robert tells us there is small chance of our becoming an A 1 nation until we have admitted the true causes of our physical insufficiency, the chiefest of which is faulty nutrition, the result "of poverty, ignorance, prejudice, indifference and indulgence." Among the difficulties of solving this problem is the absence of norms wherewith to compare observed measurements and to determine the degree of deviation from some standard health index.

The report of the American Child Health Association is based on the examination of over 10,000 children of different social and economic status. Statistical analysis of this material allowed the measurements to be reduced to seven—hip width, chest depth, chest width, height, weight, arm girth, and the thickness of subcutaneous fat over the upper arm; these were further simplified to three, (1) arm girth, (2) chest depth, (3) hip width; together these comprise an index termed the "A.C.H." index. This seems to offer a sound method of assessing the physique of groups of children between the ages of six and thirteen. These physical measurements however, though informing as to quantity, tell nothing about the quality of the tissue—for which it is necessary to employ some functional tests; forcibly pulling against the spring of a dynamometer in part vitiated by "knack" and general bodily proportions; or Magee's test requiring a child to stand erect with eyes shut and toes together; a normal and healthy child should be able to stand still in this way for a quarter of an hour without fatigue; children of poor physique seldom for more than 10 minutes. A further functional test is "the vital capacity" estimation using a spirometer, which may provide useful information about the extent of affections of the lungs such as tuberculosis.

Examination of the blood gives most sensitive indications of diet deficiency and nutritional anæmia. Continuous records of growth are most valuable; retarded or arrested growth gives immediate indications of nutritional errors.

Data of a mixed population are available showing the average rate of growth in children of both sexes with which the growth curve of children under observation can be compared.

A useful corrective of the error of passing the not obviously defective child as normal is to estimate its capacity for improvement; is the child likely to improve if all the faults of habit and environment are corrected? If so, a verdict of defectiveness must be recorded.

Communal fitness is the product not of man alone but of man in relation to the natural world about him. He needs space, sunlight, air and an adequate and varied supply of food. His physical and spiritual destiny is so closely bound up with the external world that it would be as absurd to expect him to be healthy if deprived of his internal secretions as it is when we interfere in any degree with his access to vitamins.

The subject is economic and political. We must set before us rigid and scientific standards of human normality and make an effort to rear our children up to them.

P. J.

Heredity and Politics. By J. B. S. HALDANE, F.R.S. [Pp. 185, with 15 figures.] (London: George Allen & Unwin, Ltd., 1938. *7s. 6d.* net.)

THIS interesting account of some aspects of heredity will appeal to eugenists, and perhaps to politicians. It concerns the biology of inequality resulting from the action of natural and nurtural environment and involves the consideration of many controversial questions which need yet more research before their solution is possible.

The author presents the pedigrees of a number of inherited diseases and shortly discusses how far measures of negative eugenics, such as sterilisation, would be efficient in stamping out the various abnormalities. Hæmophilics mostly die young and beget less than a quarter of the number of children produced by their brothers; therefore there is a tendency for the hæmophilic gene to disappear. Roughly one-third of all such abnormal genes are in the X chromosomes of men and two-thirds in the X chromosomes of women; therefore about a fourth of all the hæmophilic genes are wiped out at each generation. Moreover, unless there were some source from which they could be replaced, *i.e.* by mutation, the frequency of hæmophilia would be diminished by 25 per cent. at each generation. This source was found in 6 pedigrees where genes for colour blindness and hæmophilia were associated in the same chromosome.

Of 953 sons whose fathers had died of cancer, fourteen had developed that disease. Of 95,300 men and boys of same ages but taken at random from American population 85 would, on an average, have developed cancer. The probability, as the result of chance, of finding 14 individuals where 0·85 were expected, is about 10^{-12} or one chance in a million million. On the whole we see that the presence of cancer in a near relative increases a person' probability of dying of cancer about ten-fold. It is quite certain that in mice and other animals the tendency to spontaneous cancer is largely inherited.

The author has two strong objections to sterilisation: (1) in cases of men it

is a trivial operation; in women it is as serious as an operation for appendicitis—though probably less than 1 per cent. will die as a result of the operation—but in English law a person's life must not be endangered except to save him from some greater danger. (2) The demand for sterilisation is a symptom of a certain state of mind which we may not find "entirely admirable."

It is characteristic that in U.S.A. sterilisation is legal while contraception is of very doubtful legality.

In the cases of hæmophilia the females who are thought likely to transmit it would be sterilised. The males could not be sterilised, the operation would probably kill them and Nature sterilises them already by killing them off as children.

The author asks whether, had a sterilisation law been in force during the nineteenth century, it would have been applied to the reigning monarch and her daughters; and if so, what would have been the effects on European history.

Prohibition or discouragement of the marriage of cousins would cut down the incidence of juvenile amaurotic idiocy by some 15 per cent., congenital deaf mutism by about 25 per cent., Xeroderma pigmentosum by 50 per cent., etc. This is the only eugenic measure advocated by the Roman Catholic Church.

Coming to the question of mental defect, there is a demand that all mental defectives should be sterilised or given the opportunity of being sterilised. It is stated that these people are very prolific. As a matter of fact, the more serious mental defectives, idiots or imbeciles are not prolific. The feeble minded are moderately so, though this is exaggerated because a large proportion of these people are in institutions where procreation is not possible. It is never possible from a knowledge of a person's parents to predict with certainty that he or she will be a more or less adequate member of society than the majority. Our knowledge of the inheritance of psychological characters is insufficient to make predictions of this kind.

Sterilisation of all mental defectives would cut down the supply of mental defectives in the next generation by 10 per cent. (5 to 30 per cent.). But as mental deficiency is a legal and not a biological conception, if the legally deficient were completely prevented from having progeny, although some mental defectives would not be born, perhaps 10 times as many normal children would not come into the world.

Space does not permit the analysis of the interesting chapter on differential fertility and positive eugenics, and the chapters on the race problem.

P. J.

Political Arithmetic: A Symposium of Population Studies. Edited by LANCELOT HOGBEN, F.R.S. [Pp. 531, with figures and maps.] (London: George Allen & Unwin, Ltd., 1938. 30s. net.)

THIS book, edited by Prof. L. Hogben, who contributes two introductory essays, is a collection of research papers from his department of Social Biology at the London School of Economics. Much has already appeared in the appropriate journals; the publishers state that about half the material is presented for the first time.

It is to be regretted that both publisher and editor appear to give a misleading impression of the scope of the book. On the cover we are told that "The individual chapters . . . deal both with the quantitative and qualitative aspects of population growth"; and again, "The first part con-

tains an analysis of the . . . differential fertility of social classes." The editor in the Introduction to Part II says, "The significance of differential fertility with respect to occupation, locality, and income has been dealt with in the introduction to the first part and in Chapters III and IV." He adds further, "The ensuing chapters are primarily concerned with one of two differential issues involved in the qualitative aspect of differential fertility."

The reader might reasonably expect from these statements that he would find, if not original researches, at least some account of what has been established on this important subject; but it should be explained that the statements quoted are almost entirely unwarranted. Differential fertility with respect to income and its qualitative effects seem both to be topics studiously avoided.

Both the make-up of the book and the prevalent style of the writing are expressive of the Editor's personality. Readers of his previous books will anticipate that his estimable capacity for personal conviction is liable to express itself in hasty and superficial judgments; and that his verbal facility, uncontrolled by self-criticism, falls too readily into prolixity. As these charges may be thought severe it is necessary to quote:

(*a*) *Superficial judgment*

"The phenomena of differential fertility dispose of the illusion that mere spending capacity favours a high reproductive capacity. Hence it is not likely that any changes in the distribution of spending power by such means as family allowances will suffice to re-establish the survival minimum" (p. 38).

Stripped of unnecessary words, the basis of the argument is that the more affluent occupations are less fertile than the less affluent. It might rationally, though not quite securely, be argued from this that a general rise in the standard of living, though desirable on other grounds, will not improve fertility. The author can scarcely think that family allowances, such as those widely used on the Continent, are intended as a means of raising the *general* standard of living. He seems to infer from the differential birth-rate that the economic or prudential motive for family limitation is completely ineffectual, whereas, even supposing it were the only motive for birth control, the facts only warrant the inference that remote financial considerations are more influential in the middle ranks of society than among the poor; and that allowances, on a scale sufficient, by redistributing the total wages, to equalise the standard of living between parents and non-parents doing equivalent work, might materially raise the birth-rate at all income levels. Here is a second example:

"In the light of new evidence derived from the study of twins, conclusions about inborn differences based on intelligence test score comparisons of different occupational and racial groups have little scientific validity" (p. 333).

The new evidence referred to seems to consist of no more than some intelligence tests applied by Prof. Hogben and his assistants to a group of twins. The *data* do not appear to differ appreciably from those obtained in previous studies. The inferences only are novel, and Prof. Hogben seems never to have answered Fraser Roberts' criticism of his conclusions. Nevertheless, it seems that all other studies, previous or subsequent, have in Prof. Hogben's opinion "little scientific validity."

(*b*) *Prolixity*

The whole book, the substance of which could easily have been presented more briefly, is a monument of redundance. The use of unnecessarily pretentious words, which impede rather than assist the understanding, has been obvious in some of the quotations already given. Here are two more particular examples:

" It is equally necessary to insist that science is not an indiscriminate collection of unrecorded facts " (p. 39).

In the more usual form of this *cliché*, the facts are said to be *recorded*; even so, I cannot feel any real necessity for insisting on it. It seems to be unrelated to the paragraph in which it occurs.

The gross reproduction rate is a simple concept of which Dr. Kuczynski has made good use. It is a short descriptive feature of a system of birth rates at each age, defined by the average number of daughters which women would bear, if they did not ever die before the end of the reproductive period. Is this idea elucidated by the following explanation by D. V. Glass ?

" The significance of this rate in terms of actual replacement can be seen by computing from the Life Table for the period or the region the minimum number of children per woman passing through the ages 15 to 49 necessary for maintaining a net reproduction rate of unity " (p. 162).

The book contains two good essays by Kuczynski, " I. The International Decline of Fertility " and " VII. British Demographers' Opinions on Fertility, 1660 to 1760," reprinted from the *Annals of Eugenics*. Some of the papers by Mrs. Hogben (writing as Enid Charles) also deserve notice. In Number II (the numbering in the contents does not correspond with that of the chapters) she describes at length her calculations on the future populations of England and Wales and of Scotland on the suppositions (*a*) that fertility and mortality will remain the same as in 1933, and (*b*) that both continue to fall in a manner prescribed by the authoress, but which does not appear to be realistic. No mention is made of previous calculations of the same type which have been published, but the very serious consequences inherent in the present low level of reproduction in both countries perhaps justifies reiteration, as an attempt to bring some of the facts to the knowledge of a wider public. The article is clear, and has the great merit of being unpretentious.

No. III, although entitled " Differential Fertility in England and Wales during the Past Two Decades," does not deal with what is generally known as differential fertility. It is based on local comparisons worked out on a scale appropriate to a blue book, which illustrate the well-known contrast between town and country. An indirect attempt is made to discuss the effect of industry, through the local statistics of different districts in which different industries are prevalent, but the immensely important differences associated with occupation considered individually, and with grade of employment, seem to have been left wholly out of account. The reader must satisfy himself with such banal conclusions as that urban surroundings and female employment are unfavourable to fertility. This is a poor return for laborious and conscientious, though perhaps misdirected, labour. The authoress argues that the facts presented imply a further substantial fall in fertility, though really they have no bearing on this issue.

R. A. Fisher.

BOOKS RECEIVED

(Publishers are requested to notify prices.)

Modern Higher Algebra. By A. Adrian Albert, Associate Professor of Mathematics, the University of Chicago. Second impression. Cambridge: at the University Press, 1938. (Pp. xiv + 319.) 18*s*. net.

Introduction to Bessel Functions. By F. Bowman, M.A., M.Sc.Tech., Head of the Mathematics Department in the College of Technology, Manchester. London, New York, Toronto: Longmans, Green & Co., 1938. (Pp. x + 135, with 21 figures.) 10*s*. 6*d*. net.

Advanced Mathematics for Engineers. By H. W. Reddick, Professor of Mathematics, and F. H. Miller, Assistant Professor of Mathematics, Cooper Union Institute of Technology. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. (Pp. x + 473, with 130 figures.) 20*s*. net.

Theoretical Hydrodynamics. By L. M. Milne-Thomson, M.A., F.R.S.E., Professor of Mathematics in the Royal Naval College, Greenwich. London: Macmillan & Co., Ltd., 1938. (Pp. xxiv + 552, with 4 plates and 316 figures.) 31*s*. 6*d*. net.

Mathematics for Technical Students. Part I. By A. Geary, M.A., M.Sc., Head of the Mathematics Department, Northampton Polytechnic, H. V. Lowry, M.A., Head of the Mathematics Department, Woolwich Polytechnic, and H. A. Hayden, D.Sc., M.Sc., Head of the Mathematics Department, Battersea Polytechnic. London, New York, Toronto: Longmans, Green & Co., 1938. (Pp. viii + 314, with 244 figures.) 4*s*.

Tables of Functions. With Formulæ and Curves. By Dr. Eugen Jahnke, formerly Professor in the School of Mines in Berlin, and Fritz Emde, Dr.-Ing.E.H., Dr.Techn.E.H., Professor of Electrotechnics in the Stuttgart Technical College. Third edition. Leipzig and Berlin: B. G. Teubner, 1938. (Pp. xii + 305, with 181 figures.) Price abroad, RM.11.25.

Stellar Dynamics. By W. M. Smart, M.A., D.Sc., Regius Professor of Astronomy in the University of Glasgow. Cambridge: at the University Press, 1938. (Pp. viii + 434, with 75 figures.) 30*s*. net.

The Principles of Statistical Mechanics. By Richard C. Tolman, Professor of Physical Chemistry and Mathematical Physics at the California Institute of Technology. The International Series of Monographs on Physics. Oxford: at the Clarendon Press; London: Humphrey Milford, 1938. (Pp. xx + 661.) 40*s*. net.

Grimsehls Lehrbuch der Physik. Neubearbeitet von Professor Dr. R. Tomaschek, Direktor des physikalischen Instituts der Technischen Hochschule, Dresden. Band II, Teil I: Elektromagnetisches Feld—Optik. Eighth edition. Leipzig and Berlin: B. G. Teubner, 1938. (Pp. x + 866, with frontispiece, 1 coloured plate and 1209 figures.) Price abroad RM.19.50.

General Physics (With some Astronomy). Part I: Mechanics, Heat, and Properties of Waves. By F. Oldham, M.A., B.Sc., Headmaster, Grammar School, Hinckley, and E. Langton, B.Sc., Head of the Department of Physics, The College of Technology, Leicester. New General Science Series. London: University of London Press, Ltd., 1938. (Pp. xii + 278, with 228 figures.) 4*s*.

The Physical Properties of Colloidal Solutions. By E. F. Burton, B.A., Ph.D., Head of the Department of Physics and Director of the McLennan Laboratory, University of Toronto. Third edition, prepared with the assistance of May Annetts Smith, M.A., Ph.D. Monographs on Physics. London, New York, Toronto: Longmans, Green & Co., 1938. (Pp. viii + 235, with 36 figures.) 15*s*. net.

The Perception of Light. An Analysis of Visual Phenomena in Relation to Technical Problems of Vision and Illumination. By W. D. Wright, D.Sc., A.R.C.S., Lecturer in Physics, Imperial College of Science and Technology. London and Glasgow: Blackie & Son, Ltd., 1938. (Pp. viii + 100, with 49 figures.) 6*s*. net.

Fluorescence and Phosphorescence. By E. Hirschlaff, Ph.D. Methuen's Monographs on Physical Subjects. London: Methuen & Co., Ltd., 1938. (Pp. viii + 130, with 42 figures.) 3*s*. 6*d*. net.

Ultrasonics and Their Scientific and Technical Applications. By Dr. Ludwig Bergmann, Professor of Physics at the University of Breslau. Translated by Dr. H. Stafford Hatfield. London: G. Bell & Sons, Ltd., 1938. (Pp. x + 264, with 148 figures.) 16*s*. net.

A Textbook of Thermodynamics. By F. E. Hoare, Ph.D., M.Sc., A.R.C.S., D.I.C., Lecturer in Physics at University College, Exeter. Second edition. London: Edward Arnold & Co., 1938. (Pp. xii + 307, with 49 figures.) 15*s*. net.

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SCIENCE PROGRESS

CHEMISTRY OF WINE

By J. T. HEWITT, F.R.S.
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For the purposes of this article, wine will be considered to be the product obtained by fermenting the expressed juice of grapes. In how far it is reasonable to extend the term to alcoholic liquids obtained from other fruits or from grape musts to which additions have been made (water, sugar, salts, etc.) may arise in certain cases but, broadly speaking, wine without qualificatory remarks is usually expected to be fermented grape juice which may, as in the case of port, be subsequently fortified by the addition of an alcoholic distillate.

According to the statistics given in *Whitaker's Almanack* for 1939, the total world production of wine amounts to 4,000,000,000 imperial gallons annually; of this amount France produces 1,150,000,000 and Algeria 335,000,000 gallons, making together over one-third of the whole. Next in order come Italy (745), Spain (335), Roumania (230) and Portugal, U.S.A. and Argentina (each about 175), the figures in brackets referring to millions of gallons. This overwhelming proportion of production from Latin countries, combined with the strict legal control exercised in France to protect one of its chief agricultural industries, justify pre-eminent consideration being given to the practice in that country.

Fermentation by Yeast

The conversion of sugar into alcohol and carbon dioxide according to the equation

$$C_6H_{12}O_6 = 2C_2H_5OH + 2CO_2 + ca.33 \text{ Cal.}$$

is effected by yeast, the species which specially interest the wine-maker being *Saccharomyces ellipsoideus* and *S. apiculatus*. When fermentation is due to yeasts carried into the vats on the skins of the grapes, the latter species is found in abundance at the beginning of the fermentation, but as its action is inhibited when over 4 per

cent. of alcohol is present the fermentation is finished by *S. ellipsoideus*. It is, of course, possible to ferment grape juice with distiller's or brewer's yeast (*S. cerevisiæ*), but where a fresh must is employed there is no reason to use anything but a grape yeast.

Yeast was recognised as a form of plant life by the middle of last century, and the consumption of sugar evidently affords energy to the living cell. (Compare the evolution of heat when sugar is fermented.) That the actual yeast cell is unessential in bringing about the change was shown by Eduard Buchner (1897), who ground pressed beer-yeast with quartz-sand and kieselguhr and, after adding water, pressed out the juice. This juice, even after passage through a Berkefeld-filter, was able to ferment sucrose, glucose, fructose and maltose; the action was due to an enzyme which Buchner called *Zymase*.

Sucrose (cane-sugar) is not fermented as rapidly as the hexoses; it needs first to be hydrolysed to the latter sugars.

$$C_{12}H_{22}O_{11} + H_2O = \underset{\text{glucose}}{C_6H_{12}O_6} + \underset{\text{fructose}}{C_6H_{12}O_6}$$

The enzyme which effects the change is known as *invertase*. This so-called inversion may be brought about more rapidly by the action of dilute strong acids (hydrogen ions), the dextrorotatory cane-sugar solution becoming lævorotatory since the lævorotation of fructose is greater than the dextrorotation of an equivalent amount of glucose. In practice, hydrolysis by mineral acids is of no interest to the wine-maker since the sugars contained in grape must consist essentially of directly fermentable hexoses.

Whilst the enzyme, zymase, is responsible for the conversion of hexose into carbon dioxide and alcohol, various factors must be taken into account. Yeast juice falls off rapidly in activity, and at first this was thought to be due to the action of the tryptic enzyme in the yeast juice on the zymase. In support of this view there is the fact that addition of a tryptic enzyme of animal origin hastens the disappearance of the alcoholic enzyme (E. and H. Buchner and Hahn, 1903), whilst addition of substances (*e.g.* serum) which hinder tryptic action favour alcoholic fermentation (Harden, 1903). Harden and Young (1905) then made the important discovery that the fermentation caused by a given amount of yeast juice (measured by the amount of carbon dioxide evolved) was doubled if an addition was made of a volume of boiled yeast juice equal to that of the yeast juice employed, and this effect was obtained whether the yeast juice were fresh or had undergone autolysis. Eventually the increase in fermentation produced by the boiled yeast juice was traced to (1) the presence of phosphates in the liquid, and (2) the

existence in boiled fresh yeast juice of a co-enzyme which is indispensable for fermentation.

If an alkaline phosphate (Na_2HPO_4 or K_2HPO_4) is added to a fermenting sugar solution the rate of evolution of carbon dioxide increases rapidly whilst free phosphate disappears in the liquid. Then, whilst active fermentation continues, the concentration of free phosphate remains low, but towards the end of the fermentation it rapidly increases.

Salts of hexose (mono- and di-) phosphoric esters have been isolated (Harden, Robison, Neuberg and others) and Harden (*Alcoholic Fermentation*, 4th edit., p. 69) gives the equations

1. (*a*) $3C_6H_{12}O_6 + 2PO_4HR_2 = 2C_6H_{11}O_5(PO_4R_2) + 2H_2O + 2CO_2 + 2C_2H_6O$
   (*b*) $2C_6H_{12}O_6 + 2PO_4HR_2 = C_6H_{10}O_4(PO_4R_2)_2 + 2H_2O + 2CO_2 + 2C_2H_6O$

as representing the action during the addition of phosphate.

Since yeast contains a phosphatase capable of hydrolysing the hexosephosphates the following reactions must be taken into account

2. (*a*) $C_6H_{11}O_5(PO_4R_2) + H_2O = C_6H_{12}O_6 + PO_4HR_2$
   (*b*) $C_6H_{10}O_4(PO_4R_2) + 2H_2O = C_6H_{12}O_6 + 2PO_4HR_2$ (R = Na or K)

Thus phosphate is still available for reactions 1(*a*) and 1(*b*), and its amount increases at the end of the fermentation when the excess of hexose is no longer available.

Isomeric forms of the hexosephosphates are known and it is of interest to note, as we shall see below, that the same mixture of hexosemonophosphates is obtained whether the hexose submitted to fermentation is glucose, fructose or mannose.

The co-enzyme has been concentrated so that a preparation with an activity nearly 500 times that of fresh yeast has been obtained. On hydrolysis, this substance yielded adenine and a reducing sugar from which it would appear to be an adenine nucleotide to the extent of 90 per cent. (Euler, Myrbäck and others. A lecture by H. Euler is reported in *Angew. Chem.*, 1937, **50**, 831).

Whilst a normal fermentation of a hexose gives alcohol and carbon dioxide to the extent of 90 per cent. or more of the amounts required by the usual equation, other substances are formed in smaller amounts and must be reckoned amongst the compounds which communicate flavour and differentiate a potable liquid, whether it be beer, cider or wine, from a dilute solution of alcohol in water. It will be well at this stage to consider further what other enzymes besides zymase (and co-zymase) are present in yeast.

(1) *Carboxylase* (Neuberg and co-workers, 1911 *et seq.*).—All races of brewer's yeast, active yeast preparations and wine yeasts

contain an enzyme which decomposes α-keto-acids according to the equation

$$R.CO.COOH = R.CHO + CO_2$$

As we shall see later, the decomposition of pyruvic acid, yielding acetaldehyde, is of considerable interest with regard to one theory of alcoholic fermentation. It may well be the case that pyruvic acid reacts in its tautomeric form $CH_2{:}C(OH).COOH$ (Nord, 1929).

(2) *Carboligase* (Neuberg and co-workers, 1922 *et seq.*).—This enzyme is synthetic in its action and causes two molecules of an aldehyde to unite, giving a keto-secondary alcohol. The two molecules may be of the same or different aldehydes, thus acetaldehyde yields acetoin (acetylmethylcarbinol, $CH_3.CH(OH).CO.CH_3$), whilst benzaldehyde added to a fermenting mixture of yeast with sugar or pyruvic acid gives phenylacetylcarbinol. The compounds obtained by these reactions are optically active.

(3) *Reducing Enzyme.*—The reducing action of yeast has long been known, and de Rey-Pailhade (1888) found that extracts of yeast gave rise to hydrogen sulphide when brought in contact with sulphur. He gave the name *philothion* to the active substance, and it may possibly be the same as the *glutathion* isolated by Hopkins (1921, 1929) from yeast and animal tissues and identified as a tripeptide of glutamic acid, glycine and cysteine. Yeast will not only reduce sulphur, iodine, methylene blue, etc., but also converts aldehydes into the corresponding primary alcohols.

These reactions involve the addition of hydrogen, the source of which is not always quite clear. It should be noted that living yeast will convert acetaldehyde into a mixture of alcohol and acetic acid (oxido-reduction dismutation) comparable with the Cannizzaro reaction in which an alkali converts two molecules of an aldehyde into a molecule of the corresponding alcohol and a salt of the corresponding acid.

$$2C_6H_5.CHO + KOH = C_6H_5.CH_2OH + C_6H_5.COOK$$

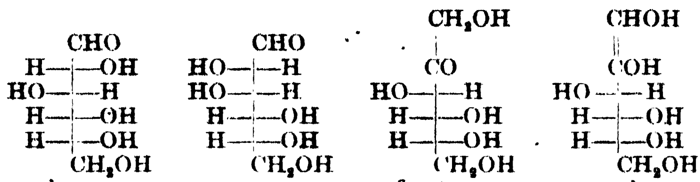
Such dismutation may be internal, for *glyoxalase*, an enzyme occurring in yeast and many animal tissues, will convert methylglyoxal into lactic acid.

$$CH_3.CO.CHO + H_2O = CH_3.CH(OH).COOH$$

Mechanism of Alcoholic Fermentation

Whilst hexoses in an isolated condition are made up of cyclic molecules (Haworth), it may be assumed that in solution there is an equilibrium proportion of open chain molecules of the structures formerly given to the hexoses. In fact, such an assumption seems

to be necessitated by certain reactions, amongst which that discovered by Lobry de Bruyn (1895) is of importance. Whilst each of the three hexoses, glucose, fructose and mannose, is stable by itself, any one of these is converted into an almost optically inactive mixture of all three in alkaline solution. This is explained by supposing that these hexoses have a common enolic form. The fact that glucose and fructose give the same hexosephosphates when alkaline phosphate is added to a fermenting solution of either suggests that a similar equilibrium is set up in this case.



glucose mannose fructose enol

Further action of alkalis on sugar gives a considerable yield of lactic acid and small amounts of alcohol; the latter seems to be produced more readily under the influence of sunlight (Duclaux, 1886; Buchner and Meisenheimer, 1904). Duclaux also observed that alcohol, calcium carbonate and acetate were formed when a solution of calcium lactate was exposed to sunlight. As to the mechanism by which lactate is formed, the prevailing view is that the hexose gives two molecules of glyceraldehyde which passes into methylglyoxal by loss of water.

$$CH_2(OH).CH(OH).CHO = H_2O + CH_2{:}C(OH).CHO (= CH_3.CO.CHO)$$

We have seen previously that lactic acid is formed by the addition of the elements of water to methylglyoxal.

The formation of glyceraldehyde and methylglyoxal from hexoses has engaged much attention; the matter is discussed fully, both from the experimental and theoretical points of view, in Harden's *Alcoholic Fermentation*.

Several schemes have been proposed to account for the course of the reaction; in each case the hexose is assumed to split into two 3-C units.

(1) *Lactic Acid Theory.*—The reactions involved may be expressed as follows:

$$C_6H_{12}O_6 \rightarrow \text{(glyceraldehyde or methylglyoxal)} \rightarrow CH_3.CH(OH).COOH$$
$$\rightarrow CH_3.CH_2OH + CO_2$$

Buchner and Meisenheimer showed that lactic acid was neither formed nor fermented by pure yeast, which seems to be decisive. Wohl has pointed out, however, that the production of lactic acid

from glucose is exothermic, the lactic acid molecules are thus formed in an activated state and may be more prone to further reaction.

(2) *Pyruvic Acid Theory.*—As formulated by Kostytschev (1912), the fermentation of glucose is represented by three fairly simple equations.

$$\begin{aligned} &\text{(i) } C_6H_{12}O_6 &&= 2CH_3.CO.COOH + 4H \\ &\text{(ii) } 2CH_3.CO.COOH &&= 2CH_3.CHO + 2CO_2 \\ &\text{(iii) } 2CH_3.CHO + 4H &&= 2CH_3.CH_2OH \end{aligned}$$

The scheme of Neuberg and Kerb (1913) is more complicated and assumes the intermediate formation of methylglyoxal. Some of the latter undergoes an oxido-reduction giving pyruvic acid and glycerol. In this final stage methylglyoxal and acetaldehyde are supposed to react with formation of pyruvic acid and alcohol.

It may be noted here that small amounts of acetaldehyde and glycerol are to be expected amongst the products; this always happens in practice though the amounts are very variable.

(3) *Formic Acid Theory.*—Schade (1906) suggested that glucose breaks up readily into acetaldehyde and formic acid under the influence of catalytic agents. He found that the latter compounds react in presence of rhodium to give alcohol and carbon dioxide.

$$CH_3.CHO + H.COOH = CH_3.CH_2OH + CO_2$$

Possibly an enzyme effects a similar change during fermentation; the aldehyde might result from lactic acid, which is known to decompose into aldehyde and formic acid under certain conditions (*e.g.* heating with dilute sulphuric acid).

The theory has received very little support, but in view of the fact that formic acid is obtained from glucose by the action of many bacteria, Harden (*Alcoholic Fermentation*, p. 145) says: "Schade's theory of alcoholic fermentation may be said to be a possible interpretation of the facts." The experiment of Buchner and Meisenheimer (1910) to ferment a mixture of formic acid and aldehyde by yeast juice gave a negative result, so that the theory seems to be at variance with experimental facts. Ashdown and Hewitt (1910) found that addition of formates to fermenting glucose solutions diminishes the amount of aldehyde formed, but no argument can be based on such indirect evidence.

That acetaldehyde is a forerunner of alcohol receives support from the fact that the yellow pigment extracted by Banga and Szent Györgyi (1932) from the cardiac muscle of the pig and by Warburg and Christian (1932) from beer-yeast is able to catalyse reductions. Using the term "flavine" in the sense proposed by

Kuhn (1935) to represent derivatives of benz*iso*alloxazine, the yellow ferment may be represented schematically by

Flavine—Ribose—Phosphate
└——Protein——┘

With regard to glycerol, the amount may be largely increased by the addition of sulphites (Neuberg and Reinfurth ; Connstein and Lüdecke). This reaction is spoken of as Neuberg's second form of fermentation and may be represented by the equation,

$$C_6H_{12}O_6 + Na_2SO_3 + H_2O = C_3H_8O_3 + CH_3.CH(OH)(SO_3Na) + NaHCO_3$$

A certain amount of the hexose undergoes the normal alcoholic fermentation, but 15 to 20 per cent. of the sugar fermented may undergo the glycerol change. Even larger quantities can react in this way in presence of a great excess of sulphite. During the war, the process was used in Germany on a large scale.

Alcoholic fermentation may also be modified by the action of mild alkalis, the intermediately formed aldehyde undergoing an enzymic dismutation

$$2CH_3.CHO + H_2O = CH_3.CH_2OH + CH_3.COOH$$

In presence of 4 per cent. of ammonium carbonate as much as 41·3 per cent. of the sugar has been fermented in this way, the change is known as Neuberg's third form of fermentation and is equivalent to a Cannizzaro reaction.

The foregoing account of alcoholic fermentation shows that the fermentation of sugar gives rise to several substances besides alcohol, *viz.* glycerol, aldehyde, acetic acid and (?) formic acid, whose origin may be traced to a hexose. But other substances are also found in fermented liquors, *viz.* the higher alcohols which constitute fusel oil and succinic acid. These may be traced to the amino-acids formed by hydrolysis of the proteins present in beer-wort, apple juice, grape must, etc. The proteins of the yeast itself also play a part, for higher alcohols are formed when glucose solutions are fermented in which the nitrogen has been added in the form of ammonium salts (Ashdown and Hewitt, 1910).

Fusel Oil

Fusel oil is the name given to the mixture of higher alcohols and small quantities of other substances (furfural, higher fatty esters, etc.) obtained when an alcoholic liquid is rectified for the preparation of spirit. Formerly it was thought that these were produced from the sugar, but F. Ehrlich (1903 *et seq.*) showed that

they resulted from the action of the yeast on amino-acids. By adding leucine to a fermenting sugar solution, the inactive amyl alcohol was formed; if, however, *iso*leucine was added, active *d*-amyl alcohol resulted.

$$(CH_3)_2CH.CH_2.CH(NH_2).COOH + H_2O = (CH_3)_2CH.CH_2.CH_2OH + CO_2 + NH_3$$

$$CH_3.CH(C_2H_5).CH(NH_2).COOH + H_2O = CH_3.CH(C_2H_5).CH_2OH + CO_2 + NH_3$$

The reaction proved to be fairly general, thus tyrosine, $C_6H_4(OH).CH_2.CH(NH_2).COOH$, gives *p*-hydroxyphenylethyl alcohol (tyrosol, a substance of bitter taste, partly responsible for the flavour of beer), phenylalanine gives phenylethyl alcohol and tryptophan gives tryptophol. When racemic leucine is treated with yeast and sugar, one stereoisomeride is consumed more rapidly than the other.

Harden (1901) found that aspartic acid is reduced nearly quantitatively when *Bacillus coli communis* is cultivated in a solution of glucose and the acid.

$$COOH.CH_2.CH(NH_2).COOH + 2H = COOH.CH_2.CH_2.COOH + NH_3$$

This is not the case when aspartic acid is added to a yeast fermentation, though when glutamic acid is added no glutaric acid is formed and the amount of succinic acid is increased. Addition of aspartic acid or an ammonium salt decreases the yield of succinic acid. From analogy with the degradation of leucine one might expect glutamic acid to yield γ-hydroxybutyric acid, but its formation has not been observed, whilst both α-ketoglutaric acid and succinic semialdehyde are converted by yeast to succinic acid. One may formulate the steps in the production of *iso*amyl alcohol from leucine and of succinic acid from glutamic acid in the following manner with a fair degree of probability.

$$\begin{array}{ll}
(CH_3)_2CH.CH_2.CH(NH_2).COOH & COOH.CH_2.CH_2.CH(NH_2).COOH \\
\downarrow & \downarrow \\
(CH_3)_2CH.CH_2.CO.COOH & COOH.CH_2.CH_2.CO.COOH \\
\downarrow & \downarrow \\
(CH_3)_2CH.CH_2.CHO + CO_2 & COOH.CH_2.CH_2.CHO + CO_2 \\
\downarrow & \downarrow \\
(CH_3)_2CH.CH_2.CH_2OH & COOH.CH_2.CH_2.COOH
\end{array}$$

Constituents of Grape Must

So far we have dealt with those substances obtained in the course of any alcoholic fermentation whose presence can be attri-

buted to the action of yeast either on sugar or on amino-acids. In the case of wine, the constituents of the grape are of prime importance. Sometimes the grapes are destemmed before or after crushing but, not infrequently, the whole crushed mass, pulp, skins, pips and stalks, goes to the fermenting vat. Hence it is necessary to consider a great number of constituents in order to get an idea of the substances which may be found in the must and the wine produced from it.

The following list includes some of the substances which have been reported as occurring in grape must.

(1) *Sugars.*—In European grapes, only glucose and fructose are known to occur definitely, but sucrose has been reported as present in some American grapes. There is not much probability of finding sucrose in musts as the grapes contain sufficient *invertase* for its hydrolysis.

To produce a wine with 10 per cent. alcohol by volume, the must should contain 170 grams hexose per litre. This is greater than the amount calculated from the equation of alcoholic fermentation but, as has been pointed out, some of the sugar is converted into other products of which glycerol is especially noteworthy.

(2) *Inosite.*—Detected in unripe grapes, must and wine.

(3) *Organic Acids.*—The amount varies; it may be as low as 3 parts per thousand in southern climates or as high as 30 in northern climates. Tartaric (dextro) and malic (lævo) acids occur in greatest quantity, but the presence of formic, glycollic, glyoxylic, succinic and citric acids has also been reported in grape juice; some of these are at least doubtful. Reichard (1936) states that citric acid is present in grape musts but may be entirely destroyed during fermentation.

(4) *Pectin.*—All fruit juices contain substances of the pectic class, and a certain amount persists in the wine. Müntz and Lainé (1906) found small amounts in wine and held them to be responsible for the quality of *velouté* (softness).

(5) *Tannins.*—These occur in the skins, seeds and stalks of grapes. The quantity in solution in the must is small, but more is taken up during fermentation of red wine. In the case of white wine, the marc (skins, seeds, stalks) is usually removed before fermentation, so that it is frequently necessary to add tannin to the wine afterwards. The tannins of the tannic acid class are esters of sugars with polyhydroxybenzoic acids or their condensation products. These yield gallic (3 : 4 : 5-trihydroxybenzoic) acid and a sugar on hydrolysis. They are mixtures which have resisted

separation into their individual constituents, but some of them appear to be derived from digallic acid,

$$\overset{(3)}{C_6H_2(OH)_3}.CO.O.\overset{(4:5)}{C_6H_2(OH)_2}.\overset{(1)}{COOH}$$

A synthetic product, penta-*m*-digalloyl-β-glucose, obtained by Emil Fischer in 1918, strongly resembles Chinese tannin ; its formula is

$$[C_6H_2(OH)_3.COO.C_6H_2(OH)_2.CO.]_5C_6H_7O_6$$

The tannins share with the catechins the useful property of precipitating proteins (tanning process), and use of this is made in the clarification of wine. With gelatine solution the tannin (more added if desirable) gives a precipitate which carries down finely suspended matter. The catechins have been noted as occasionally present in wine (Simmler, 1861) ; when pure they are colourless, crystalline derivatives of phloroglucinol. They are readily changed to amorphous tannins (tannin red) by mineral acids, enzymes and heat.

Resinous substances known as *phlobaphenes* are also present ; possibly they are anhydrides of the tannins. They are practically insoluble in water but dissolve in alkalis, alcohol and tannin solution. The resinous material of grape stalks is usually supposed to be a phlobaphene.

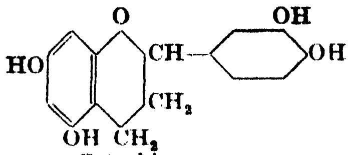
(6) *Colouring Matters.*—The green colour of immature grapes is due to chlorophyll, this disappears as ripening proceeds. The yellow dyestuff of the skins of white grapes is possibly a carotenoid, since compounds of this class are widely distributed in plants and usually accompany chlorophyll.[1]

Regarding the colouring matter of red grapes, Willstätter and Zöllinger (1915) isolated Œnin Chloride, $C_{23}H_{25}O_{12}Cl, 6H_2O$, from a North Italian variety. Hydrolysis proved this to be a glucoside and the resulting Œnidin Chloride was shown to be an anthocyanidin.

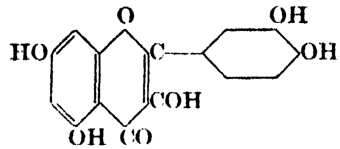
$$C_{23}H_{25}O_{12}Cl + H_2O = C_6H_{12}O_6 + C_{17}H_{15}O_7Cl$$

Small quantities of quercitrin, a rhamnoside of quercitin, are found in wine (Votocek and Fric, 1900) and it will be seen from the structural formulæ that catechin, œnidin and quercitin all contain a pyrogallol residue.

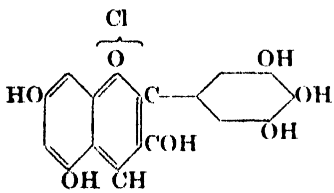
[1] The carotenoids are characterised by long hydrocarbon chains with conjugated ethenoid linkages joined at either end to cyclic or open chain groups. The latter groups are frequently identical and may contain oxygen. The pioneer work of Willstätter (1910) has been greatly extended by the researches of P. Karrer, R. Kuhn and their co-workers. Useful accounts of the more recent work will be found in the Chemical Society's Annual Reports for 1933 and 1935.



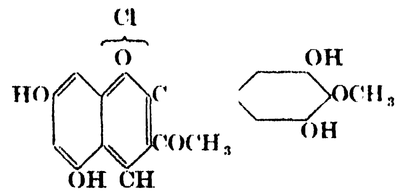
Catechin



Quercitin



Delphinidin chloride



Œnidin chloride

The structural relationships revealed by these formulæ are of interest in view of Gautier's theory (1878, 1892) that there is some connection between the pigments and tannin.

(7) *Fats.*—Must is liable to contain a small amount of fats which may be derived from the seeds rather than the pulp: palmitic, stearic and melissic acids have been isolated from grape seed oil (André, 1922).

(8) *Nitrogenous Compounds.*—Coagulable albumen, albumoses, amino-acids and ammonium salts are present in must and traces of nitrates have been detected. The evidence with regard to the presence of lecithin is, on the whole, affirmative. As we have seen previously, the amino-acids are the source from which the higher alcohols and succinic acid are derived.

(9) *Enzymes.*—Martinand (1907) states that *sucrase* (*invertase*) occurs in all parts of the vine. This would account for the absence of sucrose in the fruit.

Although C. von der Heide is doubtful about the presence of an oxydase, Bouffard and Semichon hold the opposite view, and H. T. Brown (1914) localises the oxydase as being confined to the inner layers of the skin, the vascular bundles occurring just below the skin and the bundles which connect the pips with the stalk.

(10) *Inorganic Salts.*—From 2 to 6 (usually 3 to 4) grams of ash (40–60 per cent. K_2O, 8–20 per cent. P_2O_5) are furnished by each litre of must. The following ions are stated to have been detected in various samples.

Kations.—Sodium, Potassium, Magnesium, Calcium, Aluminium, Manganese, Iron. Copper and Arsenic may be accidental.

Anions.—Fluoride, Chloride, (Bromide), (Iodide), Borate, Silicate, Phosphate, Sulphate.

VINIFICATION

The production of wine from grape-must involves several processes; these may be shortly reviewed. Since the juice of red grapes [1] is almost colourless, either red or white wine can be made from red grapes according to whether fermentation is effected in presence or absence of the skins, whilst white grapes can only be used for making white wines.

The skins of the grapes have an odour which is special for each variety; it is known as the *parfum* and must not be confused with *bouquet*. The *parfum* becomes less noticeable as the wine ages.

When the grapes are picked, they need crushing; this is generally effected by mechanical means, although treading with the feet still survives. A machine is so designed as to rupture the skin and allow the pulp to escape without crushing the pips, since the oil contained in the kernels may communicate a disagreeable taste to the wine. After crushing (*foulage*—the machine is a *fouloir*) the mass is transferred to the fermenting vat (*cuve*) and fermentation starts by the action of the native yeast introduced on the skins of the grapes. Such would be the simplest way to make a red wine. In the earlier stages the must is fairly aerated and the yeast multiplies rapidly; as dissolved oxygen is used up the yeast enters on an anaerobic existence and attacks the sugar with production of alcohol and carbon dioxide.

Improvements on the above simple process have to be considered, and one of the first points relates to the desirability of introducing the whole of the crushed mass into the vat. Machines for de-stemming (*égrappage*) have been introduced and may often be used with advantage; a machine which combines crushing and de-stemming is known as a *fouloir-égrappoir*. (The *égrappoir-fouloir* separates the stems from the grapes before the latter are crushed.) On the one hand it is claimed that removing the stalks gives a somewhat stronger and more brilliant wine and that undue astringency is avoided; on the other hand, opponents of the system state that fermentation is retarded owing to lack of aeration and that too much flavour, due in part to tannin and tartaric acid, is lost. Briefly, it seems desirable to practise de-stemming if the variety of grape gives a wine with too great astringency or sharpness or if the grapes have been gathered when not fully ripe. But when the grapes are very ripe and very rich in sugar, fermentation is likely to be difficult if de-stemming is resorted to.

[1] It is customary to speak of black or white grapes. These are spoken of in French as *cépages rouges* or *blancs*.

The temperature at which fermentation takes place determines the period spent in the vat (*cuvaison*). The reaction whereby sugar is converted into alcohol and carbon dioxide is exothermic, so that temperatures tend to rise. If, however, the initial temperature is too low, the loss of heat by conduction and radiation is not compensated and devices are employed for raising the temperature. On the other hand it is necessary to cool the fermenting mass in hot climates, otherwise the action of the yeast is inhibited and that of bacteria promoted. Whilst the activity of yeast is at a maximum at 35° it falls off rapidly above this temperature, whilst for bacteria the optimum is 38° so that cooling should start when 33° is attained. According to Pacottet, a reduction to 18° is allowable. The time the wine remains in the fermenting vat may be only two or three days or may be extended to ten days or more. In the latter case the extraction of colouring matter and tannin is more complete and the wine tends to be hard (*dur*).

The size of the vats in which the fermentation is effected is variable. Red wine can be made in small casks or in large concrete tanks lined with glass tiles. Some of the largest are found at the co-operative wineries at Marsillargues (Hérault) and Maison Carrée (Alger).

After fermentation, the wine is run off from the *marc* (*vin de goutte*) and a further amount (*vin de presse*) is obtained by subjecting the marc to mechanical pressure which is repeated a second and possibly a third time. The *vin de presse* is richer in tannin than the *vin de goutte*, Coudon and Pacottet quote an example of 1·51 and 0·44 grams per litre respectively with a wine made from the variety *Pinot*. This is a somewhat exaggerated case, the marc was submerged during fermentation but no further steps were taken to ensure circulation and extraction of the tannin.

Whether the run-off and pressed wines are blended depends on the quality desired and the price the wine commands ; in the case of *grands crus*, the vin de goutte is reserved. To raise the tannin content, say to 1 gram per litre, it may be desirable to add some of the wine from the first pressing, but not that from the second and third pressings, in the case of good quality wines.

After removal from the fermenting vat, the wine is stored in casks where it undergoes a secondary fermentation and deposits lees containing a considerable amount of tartar (potassium hydrogen tartrate). The temperature during this period should be about 20°, afterwards a fall in temperature is desirable to assist in deposition of sparingly soluble material. In warm climates, artificial cooling can be resorted to with advantage, for a deposit after bottling

detracts from the value of wine. Before wine is bottled it is racked several times; the operation of fining will receive attention when white wines are considered.

It has been assumed that the must as obtained by crushing the grapes has been fermented directly without addition of other materials. Good wines necessitate the use of musts containing a sufficiency but not an excess of normal constituents, it is therefore of importance to consider the causes of defects and the legitimate means by which they may be rectified. The guiding rule is that nothing should be added to the must which is not a normal constituent of must or wine or which would not be introduced in the usual course of manufacture.

Must may suffer from the grapes being gathered when (1) unripe, (2) over-ripe.

(1) It may be necessary to gather the grapes before maturity if they have been attacked by cryptogamic parasites or insects; in this case the wine is too acid and insufficiently alcoholic. To remove the acid, neutral potassium tartrate has sometimes been added in order to precipitate the sparingly soluble acid potassium tartrate. This assumes that it is tartaric acid which needs elimination, but E. Peynand (*Revue de Viticulture*, 1939, **90**, 3) states that the tartness (*verdeur*) of wines made in years when the summer has been cold and the harsh (*acerbe*) character of new wines is due to malic acid, which diminishes as the grapes ripen and which undergoes a bacterial lactic fermentation in the wine. In a must containing 166 grams of sugar per litre, Peynand found 84·4 milli-equivalents of malic acid, whilst in a White Barsac and a Red Medoc the quantities were 16·6 and 3·0 respectively.

A remedy which has sometimes been used is to add alcohol in successive small doses during the fermentation; this process must not be confused with the subsequent fortification of a fermented wine as in the case of port. The most rational procedure is to add sugar to the must (*chaptalisation*); the practice is to warm a portion of the must, dissolve sugar in it and add the solution to the contents of the vat when the fermentation is vigorous.

(2) When the grapes have been gathered in an over-ripe condition, the must is likely to be deficient in acid, in which case the growth of various micro-organisms is favoured and the sugar is converted into products other than alcohol and carbon dioxide. Direct addition of tartaric or citric acid is the simplest method; indirect methods consist in treating the must with calcium sulphate (plastering) or an acid calcium phosphate. In the case of calcium sulphate, the acid potassium tartrate in the must yields calcium tartrate, free

tartaric acid and potassium sulphate. Care is required since large amounts of potassium sulphate are undesirable ; in fact, an amount greater than 2 grams per litre is forbidden in France. Ammonium phosphate and glycerophosphate are allowable in quantity strictly necessary for the development of the yeast.

If the amount of tannin in the must is too small, the deficit may be made good. Tannin aids in the solution of the colouring matter of red wines and is very necessary for the clarification of white wines where it is often deficient.

White Wines

In making white wines, skins and stalks are removed before the must is fermented. Since the bouquet of a wine depends to a large extent on the substances derived from the skin and stalk, the grapes must be *quite ripe* before picking. In fact, over-ripeness is often desirable and in some regions (Sauterne, Rhine, Hungary) the grapes are picked when covered with mould (*Edelfaul, Pourriture noble*) due to *Botrytis cinerea.*

The juice from white grapes is viscous, and crushing (*foulage*) has to be more thorough than in making red wine. The crushed mass is pressed without removal of the stalks and the must which runs off is then freed from fragments of skin, stalks or other suspended matter (*débourbage*). This can be effected either by settling, fining or mechanical means.

If the clearing is done by settling (6 to 24 hours and even longer) the must has to be partially sterilised (*mutage*) by sulphur dioxide or a bisulphite. Fining by albumen, gelatine, etc., is not often used, the protein forms a coagulum with the tannin of the must and carries down the suspended matter. Since the amount of tannin in white must is naturally small, it is frequently necessary to add more in any case. The removal of suspended matter by mechanical means necessitates the use of special filtering arrangements or centrifuges.

The fermentation is carried out in casks at a comparatively low temperature (18 to 20°) and lasts about 15 days ; the use of large tanks does not give satisfactory results. Musts which have been sulphited lend themselves to the employment of selected yeasts which have been accustomed to sulphurous acid and started at a higher temperature in a separate portion of must. When such a " levain " is fermenting vigorously, it is added to the bulk of the must at a lower temperature ; the carbon dioxide which is evolved should carry off the excess of sulphur dioxide.

After the fermentation has ceased, the wine is allowed to remain in cask in order to deposit lees ; the first racking is sometimes carried

out at the end of autumn but often after Christmas. The second racking may be in February or March and several further rackings are made at intervals.

It has been mentioned that the clearing of must by fining with a protein (albumen, gelatine, etc.) is not often resorted to; when a white wine is fined in this way it is sometimes necessary to add extra tannin as in the case of must. A wine containing too much tannin is astringent and fining may be useful in removing the excess, but if there is not enough the wine is too soft.

In making white wine from red grapes, certain precautions must be observed. The grapes are crushed as soon as possible after picking and the cylinders of the crusher must be set sufficiently far apart not to tear the skins badly and so allow the colouring matter to escape. The pressed juice is collected fractionally. Pacottet obtained from 100 kilos of *Aramon* grapes

65 to 70 litres of colourless,
10 to 15 litres of slightly coloured and
5 to 8 litres of highly coloured must.

Should it be desirable to decolorise the must, sulphiting, animal charcoal or sulphiting combined with aeration are resorted to. It may be remarked that Champagne is largely made with red grapes.

Vins gris and *vins rosés* are obtained from red grapes. For the former, the whole must is fermented without previous *débourbage*. For *vins rosés*, the fermentation is started as for red wine and finished in cask as for white wine.

Composition of Wine

Wine is generally required with a minimum alcoholic content of 10 per cent. by volume, so that with musts of low hexose content an addition of sugar may be desirable. When, on the other hand, the sugar content of the must is high, the addition of water is forbidden in France and other countries. The indigenous yeasts are accustomed to a high sugar content so that fermentation, except in abnormal cases, presents no difficulty. Some yeasts can ferment up to 17 vol. per cent. and the present writer has heard of 19 vol. per cent. being attained under special conditions.

Fortified wines are made by addition of distilled alcohol; in order that the whole wine should be a grape product, the spirit used must be brandy and not any other form of distilled alcohol, however silent. The effect of the addition of alcohol is to stop the fermentation at any stage even when the sugar has been only partially fermented.

The non-volatile substances present in a wine have a marked

effect on its quality ; for instance, sweetness depends on sugar which may have been added after or during fermentation or simply be an excess which has escaped conversion into alcohol. An excess of tannin produces astringency, a sharp taste is due to free organic acids or acid salts whilst softness (*velouté*) is usually ascribed to pectic substances. The casks in which the wine is contained before bottling will also yield a certain amount of material which goes into solution. The more delicate flavours are due to volatile substances and the isolation of these is, in most cases, not easily practicable. However, brandy is a wine distillate and substances identified as existing in a sample of brandy must have had their origin in the wine from which it was distilled.

One of the most complete examinations of the composition of brandy was made by Ch. Ordonneau, a Pharmacist of Cognac, who published the results of his research in a small book, *Alcools et Eaux-de-Vie* (Doin, Paris, 1885). Having constructed an efficient rectifying column, he submitted 300 litres of brandy (*eau-de-vie, fins bois*) to a careful fractional distillation. The cask in which the spirit had been stored had previously contained *rapé*, *i.e.* marc fermented with water, so that the spirit contained an excess of acetic, propionic and butyric acids and their respective esters ; these could, however, be ultimately referred to the grape. The first runnings (*alcool de tête*, 5 litres after two rectifications) contained acetaldehyde, acetyl and ethyl acetate and propionate. The feints (*queue*) were rectified several times and a liquid was collected which, after drying over potassium carbonate and another distillation, gave 1195 grams of " huile essentielle à odeur agréable, représentant le bouquet de l'eau-de-vie employée ". This oil contained *n*-propyl, *n*-butyl, amyl, hexyl, heptyl and even higher alcohols ; propionates and butyrates of propyl, butyl and amyl ; the ethyl esters of caproic, heptoic, caprylic, pelargonic, capric, lauric and myristic acids ; a butylene glycol ; glycerol ; furfural and bases.

The question arises, from what sources are these products derived ? Aldehyde may be due in part to the oxidation of alcohol, but it is a normal product of the fermentation of sugar by yeast. The higher fatty acids are doubtless derived to some extent from the oil extracted from the pips ; some may result from the oxidation of the corresponding alcohols. The higher alcohols are to be referred to the action of yeast on amino-acids, and the proportions found by Ordonneau in the brandy used are of some interest. 100 hectolitres of brandy at 50 volume per cent. contained 40 grams of *n*-propyl alcohol, 218·6 grams of *n*-butyl alcohol and 83·8 grams of amyl alcohol, higher alcohols being present in small quantities.

When compared with the figures for alcohol derived from grain, the large proportion of *n*-butyl alcohol to other higher alcohols is noteworthy. Moreover, Ordonneau remarks that *n*-butyl alcohol does not occur in alcohol obtained with beer-yeast; in the oil from a maize fermentation he could only detect *iso*butyl alcohol. In view of Ehrlich's work, one is inclined to refer the difference to the material fermented rather than to the yeast employed.

It is fairly evident that the quality of a wine depends in the first place on three main factors:

(1) The variety of grape employed.
(2) The soil on which the grape is grown.
(3) The strain of yeast used in the fermentation.

In addition to these factors, the process of manufacture must be taken into consideration, *e.g.* the method of expressing the juice, the temperature of fermentation, etc. These conditions may be varied but the factors mentioned above are fixed. *Gamay* gives good quality wine on the porphyritic slopes of the Beaujolais but is not suited to the Burgundian calcareous marls. *Pinot* grown in Burgundy on a calcareous soil gives famous wines, on a compact clay soil the wines produced are almost ordinary. One may spend an instructive (if somewhat alcoholic) day or two on the road from Dijon to Beaune and note the soil on either side and the position of the more celebrated vineyards.

At one time the idea was prevalent that given two varieties of grape A and B, usually fermented by indigenous yeasts *a* and *b* respectively, one could use *a* to ferment B and obtain a wine resembling that usually given by A (or, *vice-versa* with A and *b*). This expectation has not been fully realised; at the same time it is worth while possessing a good suitable strain of yeast which may be cultivated at the winery.

Changes during Maturation

After fermentation, the wine goes to cask and is racked at intervals; the number of rackings is partly determined by the quality of the wine and the price it commands. When wine is to be consumed as a cheap beverage and not as a luxury, bottling is not necessarily resorted to.

What changes will occur whilst the wine remains in cask? During fermentation the temperature rises and potassium bi-tartrate is more readily dissolved. After removal from the marc and cooling, the excess is largely deposited but in a far from pure condition. Ridding a wine of all the suspended matter and dissolved substances which separate slowly, and solution of substances from the wood

of the cask, lead to an alteration in flavour. In wine districts, newly fermented wine is on sale at the time of the vintage ; it is supposed to have a beneficial effect medicinally. After one trial, the present writer concluded that the physiological effect was somewhat pronounced.

Another question is that of the equilibria between the acids, aldehyde and alcohols as represented by the equations

$$R.COOH + R'OH \rightleftharpoons R.COOR' + H_2O$$
$$R.CHO + 2R'OH \rightleftharpoons R.CH(OR')_2 + H_2O$$

In the absence of any considerable concentration of hydrogen ions the process is likely to be slow. Ribéreau-Gayon and Peynand (1936) state that the neutral esters found in even young wine are formed biologically during fermentation and also observe that the ester-equilibrium is never reached even in very old wines. The ratio of free to combined organic acids has been given for a number of wines by Bremond (1937).

There is a prevalent idea that spirits, once bottled, undergo little or no further maturation ; this is certainly not the case with wine. Wine is decanted carefully for the chief reason that however clear the wine was when bottled, some deposit is always formed subsequently. As to its nature, the present writer is loath to offer an opinion ; that some is formed by alteration of soluble colouring matter is almost certain, but how the change is catalysed is not obvious. The breakage (*casse*, *Bruch*) of wine is due to oxidation ; a wine may be bright and clear when drawn from the cask, but on aeration for a few hours it becomes turbid and forms a brownish red precipitate whilst the liquid assumes a yellowish colour. How the reaction is catalysed is not quite certain but probably traces of iron compounds are responsible (Legatu, Roos, H. T. Brown). Bouffard could detect no organism and the view that *casse* is due to an enzyme (Gouirand, Laborde) is improbable.

The glass of which the bottles are made is worth considering, for in most cases a dilute alcohol is likely to dissolve some constituents from the glass, especially if it is alkaline. A few years ago, attention was given to this question by a Departmental Committee, and samples of wine from different sources were sealed up in vessels made from fused silica and glasses of various qualities. After the lapse of time, vessels were opened and the contents examined.

As might have been expected, the wine kept in silica had undergone least alteration, whilst the same wine kept in different glass materials had been affected as to flavour in different ways. Whether it would be possible to say one sort of glass was most suitable for

bottling wines is more than the present writer would care to speculate on.

Wine usually acquires its " bottle character " in about two years ; if this is the case it seems that further change is mostly due to redistribution of chemical equilibria amongst the constituents of the wine. It is, however, well to remember that the bottles are not hermetically sealed and however good corks may be some leakage of oxygen from the air is almost certain. This may account in part for the ultimate deposit of brown material from the red colouring matter and to some extent to the change in flavour.

Needless to say, many attempts have been made to hasten the maturation of wine. Our forefathers sent sherry on a voyage to the East Indies, port went to Newfoundland and back. In these days various devices are tried including exposure to ultra-violet radiation. A search of the literature would doubtless disclose many attempts at improvement, some feasible, others dubious.

In this article, an attempt has been made to keep as far as possible to the chemistry of wine, and lack of space has prevented much allusion to the biological side of fermentation or the subject of viticulture.

The diseases of wine are chiefly of biological origin ; may they never cause annoyance to the reader or the writer of this article.

ATOMIC NUCLEI, STABLE AND UNSTABLE

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The two questions "How many chemical elements are known?" and "How many different kinds of atom are known?" would have been regarded as equivalent until early in the present century; except at the time when the individuality of the rare earth elements was a matter for controversy, any up-to-date chemist would have been prepared to give an answer and would have had doubts about perhaps only one or two of the number. If asked to write them down in alphabetical order, he might have protested, being accustomed to remember them by their chemical relationships, which towards the end of last century were given systematic representation in the periodic table of elements. This table, in the form to which the advance of chemistry ultimately brought it, made allowance for ninety-two elements, with, of course, the possibility of extension in the event of the recognition of elements heavier than uranium.

The discovery that certain pairs of the heavy unstable elements had identical chemical but different radio-active properties brought about a distinction between the two questions at the head of this page, and when, some years later, the work of Thomson, Aston and Dempster [1] showed that many stable elements consist of a mixture of "isotopes" of different nuclear masses, the second question became a matter for physicists, and only a specialist could be expected to answer it to any close approximation. The growth of the list of known nuclear species, due to the analysis of the elements into their constituent isotopes, was so rapid that by 1934 about 200 kinds of nucleus were recognised, and this number was increasing steadily. In that year a new turn was given to nuclear physics by Curie and Joliot's discovery that radioactive isotopes of certain light elements can be made by bombarding stable nuclei with alpha-particles. The aim of this article is to give some impression of the progress in our knowledge of nuclei that has followed during the last five years; the rapidity of progress has

been such that nearly 300 radioactive nuclei (including the naturally-occurring ones) have now been reported, of which a large majority have been identified beyond reasonable doubt. They include isotopes of all but about three of the known elements. One of the most satisfactory results of this new knowledge is the removal of the air of incompleteness formerly so characteristic of the list of known nuclei; this point, and others, can best be made clear by first reviewing briefly the list of stable isotopes.

In considering this list it is most profitable to think in terms of the now generally-accepted hypothesis [2] that nuclei are built up of neutrons and protons; the number of unit positive charges carried by the nucleus (the *atomic number* Z) is equal to the number of protons, while the total number of neutrons and protons is the *mass-number* A of the nucleus. Owing to the loss of mass entailed by the evolution of energy which would occur if the nucleus were to be formed from the individual free protons and neutrons, and owing to the slight difference between the mass of a proton and that of a neutron, the actual nuclear masses are not exact multiples of a single unit; but on the conventional mass-scale in which the oxygen isotope of mass number 16 is assigned the mass of 16 units, the actual masses differ from the integral mass-numbers by only small fractions of a unit. The constitution of any nucleus may be represented by a point of integral co-ordinates on a cartesian diagram, the abscissa being the number of protons (Z) and the ordinate the number of neutrons (A–Z). In such a "neutron-proton diagram," isotopes of any one element appear in a vertical column, while nuclei of equal mass-number but different atomic number (*isobars*) lie on lines having a negative gradient of 45 degrees. A neutron-proton diagram for the naturally-occurring nuclei is shown in Fig. 1.

The remarkable fact that, with only two possible exceptions,[1] all atomic numbers from 1 (hydrogen) to 83 (bismuth) are known to exist stably in nature, may be expressed by saying that any number of protons less than 84 (except possibly 43 and 61) may be combined with neutrons to form a stable nucleus. About twenty elements have only one naturally-occurring isotope, which means that, so far as we know, only one particular number of neutrons can stably bind together that number of protons which is the atomic number of any one of these elements. For example, we may take iodine ($Z = 53$), the only stable isotope being that of mass-number 127, which contains 74 neutrons. Thus 74 is apparently the only number of neutrons

[1] *viz.* 43 ("Masurium") and 61 ("Illinium"); the existence of the former seems very probable.

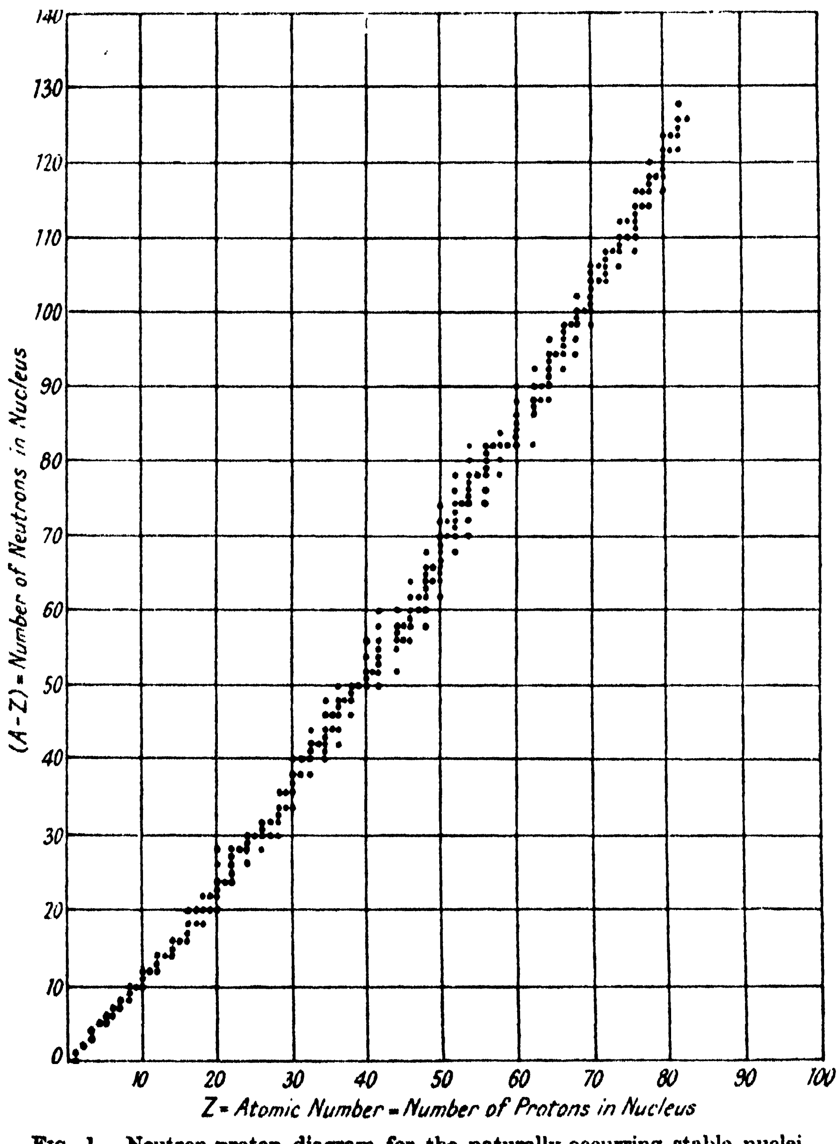

FIG. 1.—Neutron-proton diagram for the naturally-occurring stable nuclei.

Space does not allow the insertion of the chemical symbols corresponding to each atomic number, but it may be worth mentioning that Z = 20 is calcium, Z = 40 is zirconium, Z = 60 is neodymium, and Z = 80 is mercury.

capable of binding 53 protons into a stable nucleus. On the other hand, the next element, Xenon (Z = 54), has at least nine stable isotopes, their masses ranging from 124 to 136, so that 54 protons can be united in a stable nucleus by 70 neutrons, by 82 neutrons,

and by seven intermediate numbers. In drawing these conclusions we are, of course, assuming that an isotope which does not occur in nature is absent because it is unstable—not because it has never existed. We shall return to this question a little later.

When there is more than one stable isotope of an element they are not usually of consecutive mass-numbers; in fact, one of the best-marked regularities in the generally haphazard appearance of the list of stable nuclei is that elements of odd atomic number frequently consist of a pair of isotopes differing in mass-number by two units. One or two other general tendencies of minor interest have been pointed out, but there is one feature of the list which has long been recognised and is of surpassing importance—namely, that the stable nuclei contain protons and neutrons in something like equal numbers. It is true that in the heavier elements there is a definite excess of neutrons, the isotopes of mercury, lead and bismuth having about three neutrons to every two protons, but it is just at this point in the periodic table that stable nuclei end. In terms of the neutron-proton diagram, we may say that there exists a *stability band*, increasing in width from two or three units for the lightest elements to ten or a dozen units for the middle elements, and narrowing slightly once more as we approach the upper end of the table where only unstable nuclei are found. Within this band lie all the nuclei which exist stably on the earth, but about half the places within this band are vacant.

The above facts should make it clear that two of the main problems of nuclear physics are to explain why the naturally occurring nuclei contain very roughly equal numbers of neutrons and protons, and why, within this "stability band," the existing nuclear species are distributed in a fashion which is broadly yet not entirely irregular.

The former of these problems can be answered in principle on the assumptions that the forces which bind nuclei together are forces acting between neutrons and protons, and that these forces show the phenomenon of saturation, a phenomenon which may be crudely represented by the idea of "valency." If each proton must have exactly n bonds with neutrons and each neutron must have exactly n bonds with protons, the only possible nuclei would be those containing exactly equal numbers of the two kinds of particle. This condition may be expected to be relaxed to some extent, not only because the idea of valency is too crude, but because forces between like particles also come into play. In particular, the forces of electrostatic repulsion between protons help to explain the tendency of the heavier nuclei to contain more

neutrons than protons; since these forces do not show saturation they are relatively more important the more particles are contained in the nucleus.

To explain why any given nuclear species does or does not exist in nature is quite another matter; the facts are, as we have seen, so complicated that any satisfactory explanation must clearly involve detailed knowledge of the structure and properties of nuclei. We may nevertheless distinguish between two classes of explanation, not mutually exclusive. One may suppose that missing nuclear species have never existed or that, whether or no they once existed, they are unstable. The one hypothesis throws the onus of explanation upon nuclear cosmogony; the other, directly upon nuclear physics. Since nuclear physics is a progressive experimental science behind which the speculative science of cosmogony necessarily lags, it may be reckoned fortunate that the second explanation has been proved correct by the artificial preparation of the majority of the missing nuclei, which have been duly found to undergo spontaneous transmutation with half-periods [1] varying from fractions of seconds to years.

It is obviously impracticable to attempt anything like individual mention of these hundreds of unstable nuclei, and it seems best to mention very briefly the methods by which radioactive nuclei are made in the laboratory, and then to discuss the various ways in which they disintegrate [3]. The illustrations will be drawn so far as possible from a single small region in the middle of the periodic table, which in this way will receive a reasonably detailed discussion [4].

The bombardment of stable nuclei by neutrons, protons, deuterons or alpha-particles may cause immediate nuclear transmutation. In any of these cases it is convenient and probably correct to suppose that the initial process is the combination of the incident particle with the struck nucleus to form a compound nucleus which will be in an excited state. This nucleus may get rid of its excess energy in an extremely small fraction of a second, either by gamma radiation, which involves no further change in its content of neutrons and protons and means that the incident particle has been captured; or by the emission of one or more neutrons, a proton or an alpha-particle.[2] The probabilities of these secondary processes vary

[1] By half-period is meant the time required for half the nuclei originally present to have disintegrated.

[2] From very recent work of which details are not yet available, it seems that certain very heavy nuclei may, on capturing a neutron, split into roughly equal halves.

with the nature of the bombarded nucleus and with the nature and energy of the bombarding particle ; for example, slow neutrons are ordinarily captured, the next higher isotope of the bombarded nucleus thereby being formed ; on the other hand, the combination of a *fast* neutron with a nucleus is frequently followed by the emission of a proton or of two neutrons, the net results being respectively the formation of the isobar of next lower atomic number or the formation of the isotope of next lower mass-number. In some cases an alpha-particle leaves the compound nucleus, the net change then being the loss of one neutron and two protons—*i.e.* a decrease of two in the atomic number and three in the mass-number. Irradiation by gamma-rays has so far been found to result in the ejection of neutrons only and not of protons or alpha particles.

Since at least one stable isotope exists of almost every atomic number from 1 to 83, and since energetic protons, deuterons, and alpha-particles [1] have been made available in considerable intensities by the development of high-voltage technique and of indirect methods of accelerating charged particles, it is clear that by suitable bombardment of any one stable element we may expect to produce a considerable number of nuclear species which lie, on the neutron-proton diagram, fairly near the isotopes of which the target element consists. Some of the nuclei thus formed will be known stable isotopes of the bombarded element or its near neighbours, in which case no very striking evidence of their formation will appear. But others may correspond to gaps in the list of naturally occurring nuclei, and with few exceptions prove to be unstable, their presence being clearly indicated by the radiations (electrons, positrons, gamma-rays) which they emit subsequently during their spontaneous disintegration.

The assignment of an observed radioactivity to a particular element may often be accomplished by bringing the activated target into solution, adding as " carriers " small amounts of the various elements to which the activity might possibly belong, making a chemical separation of these elements and determining in which fraction the activity is found ; this is, of course, only possible for activities of reasonably long life. The assignment to a particular isotope is usually more difficult and is achieved by comparison of several nuclear reactions which lead to the production of the particular radioactivity in question. For example, the irradiation of silver (stable isotopes ^{107}Ag and ^{109}Ag) by slow neutrons produces two activities of half-periods 20 seconds and 2·3 minutes, electrons

[1] Neutrons and gamma-rays in considerable numbers appear as by-products of nuclear reactions produced by charged particles.

being emitted in each case. It is plausible to assume that these are ^{108}Ag and ^{110}Ag, formed by the capture of a neutron by the two isotopes, but we have no grounds for deciding which period to attribute to which isotope. However, since the irradiation of silver by energetic gamma-rays produces two activities, one of which is identical with the 2–3 minute activity above-mentioned, one assigns this to ^{108}Ag, produced now by the removal of a neutron from ^{109}Ag. The other activity produced by the gamma-ray irradiation is due to the removal of a neutron from ^{107}Ag with the production of ^{106}Ag and is a 22-minute positron-emitter.

We now turn to consider the various kinds of spontaneous radioactive change which nuclei may undergo. They are as follows :

1. Electron emission.
2. Positron emission.
3. Electron capture.
4. Gamma-ray emission.

The emission of an alpha-particle is typical only of the heaviest nuclei and will not be considered here.

In the first of these processes, the atomic number of the nucleus increases by unity (a nuclear neutron changing into a proton) ; in the second and third, the atomic number decreases by unity ; in the fourth, it remains unchanged. Any given nuclear species in a given state of excitation may be regarded as having a certain probability per unit time of changing in each of these four ways, each probability depending upon the nucleus and its state of excitation. Very often one or more of them may be zero [1] or negligibly small, and usually a single one of them is much greater than the other three combined, so that practically all the nuclei decay by the same process. Sometimes, as would be expected, two processes have comparable probabilities and there is a "branching" disintegration with a single time constant of decay for the two types of radioactivity. Though we cannot enter into details, some general remarks on the relative probabilities of the various processes may be of interest. In the first place, we must expect the heavier isotopes of any given element, which lie towards the upper edge of the stability band and therefore have an excess of neutrons, to show a general tendency towards electron emission, whereby their neutron-proton ratio is brought towards the optimum value. The lightest isotopes, on the other hand, should tend to emit positrons or to capture electrons. These tendencies are confirmed by experience, as is illustrated in Fig. 2, but it should be made clear that they are only tendencies and that one sometimes finds a positron

[1] For a stable nucleus in its ground state, all four are zero.

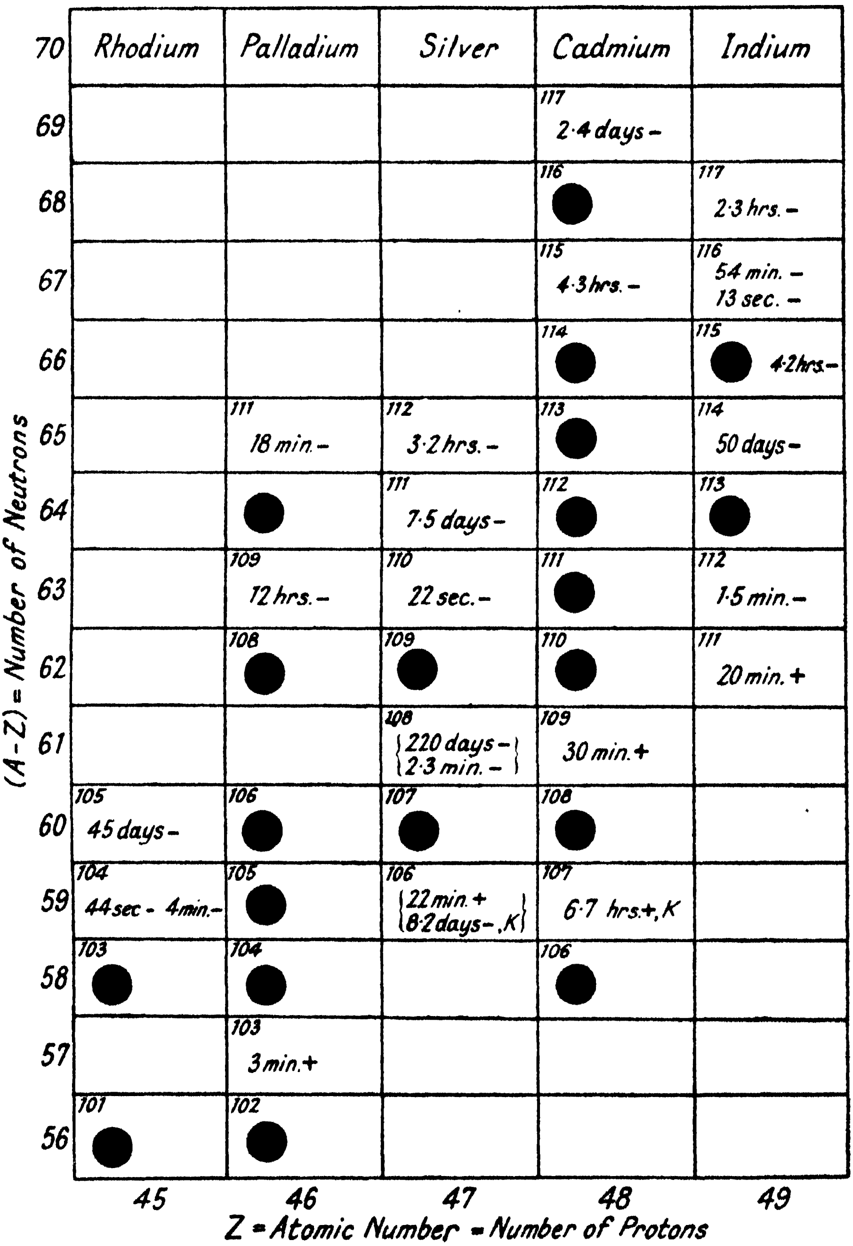

FIG. 2.—Neutron-proton diagram for nuclei of atomic numbers 45 to 49.

The numbers in the upper left-hand corner of the compartments are the mass-numbers A. A black circle indicates a stable nucleus; the symbols +, − and K indicate respectively that the nucleus protons, emits electrons or captures electrons, and the times shown are the half-periods as on p. 649. In cases of nuclear isomerism where the relative energies of the isomers are known, that of higher energy (*i.e.* the excited state) is placed to the right.

emitter which is heavier than an electron-emitting isotope of the same element. The capture by a nucleus of an electron from the innermost or K-shell of its own atom is an interesting alternative to positron emission. Since energy mc^2 is required to create a particle of mass m, and is liberated on its destruction,[1] consideration will show that the capture of a K electron (in which process the electron vanishes) requires less energy to be available than does the emission of a positron (which requires the creation of the positron), the energy difference in favour of electron capture being equal to the energy of creation of a positron-electron pair (about a million electron volts) less the relatively small energy of ionisation of the K-shell of the resulting atom. Thus electron capture may be possible even when positron emission is energetically impossible. Sometimes the two processes occur as competitors in a branch disintegration as is for example the case for the zinc isotope ^{65}Zn. There is some evidence that the seven-hour period assigned to ^{107}Cd is of this character. Just as there are exceptions to the general rule that the electron-emitting isotopes of an element are heavier than the positron emitters so one occasionally finds a nuclear species which branches between electron and positron emission, or between electron emission and electron capture. An example of the latter type is the 8·2 day period of ^{106}Ag.

The probability per unit time that a nucleus having excess energy shall radiate it by gamma-emission apparently varies enormously from one instance to another. Frequently the transition occurs in an immeasurably small fraction of a second, as is usually so when a nucleus captures a slow neutron. On the other hand, there is good evidence that certain nuclei have states of excitation which are stable for minutes or hours with respect to gamma-ray emission. This is the explanation currently accepted for the phenomenon of nuclear isomerism, by which is meant the existence of nuclei of identical atomic number and mass-number but different radioactive properties. For example in addition to the 8·2 day ^{106}Ag already mentioned there exists a silver isotope which from its mode of formation[2] appears to be ^{106}Ag but which decays with a 22-minute half-period, emitting positrons. It is believed that this is an excited state of ^{106}Ag, sufficiently stable against transition to the ground state of 8·2 hours half-period for the emission of a positron to forestall that transition. Another example is ^{116}In which is formed from the stable isotope ^{115}In by the capture of a slow neutron, and which decays by electron emission to ^{116}Sr with two distinct periods of 54 minutes and 13 seconds. There is also

[1] c is the velocity of light. [2] *Cf.* p. 651.

evidence that an excited state of the stable isotope ^{115}In is radioactive with a period of 5·4 hours. Yet another example is ^{104}Rh, where the upper level is believed to decay by branch reactions either to the ground state with emission of a gamma-ray, or to ^{104}Pd with the emission of an electron. These two transitions, being alternative modes for identical nuclei, have the same half-period (4 minutes); but the ground state of ^{104}Rh is also unstable, transforming by electron emission to ^{104}Pd with a half-period of 44 seconds.

The examples given above should serve to show that very great progress has been made in discovering what radioactive nuclei can be made and how they spontaneously transform. It remains mainly as a problem for the future to explain in terms of nuclear structure and nuclear forces *why* a given nucleus transforms as it does; when this is done the reasons for the stability of the familiar nuclei of nature will no doubt appear as corollaries.

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CROP PESTS AND DISEASES [1]

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The importance of pests and diseases as factors affecting crop production will not be disputed, though since there is never a season in which infestations of greater or less extent do not occur, it is easy to fail to realise how great is their effect upon yields and quality. If, however, the attempt is made to assess the extent of the losses suffered and to express it in money value, great difficulties at once present themselves. In the first place, an estimate by mere inspection of crops is liable to be extremely inaccurate and misleading, and though determinations of the amount of damage done in a given crop can, in many instances, be made by examination of samples with counts, it is a very laborious business. Next, supposing the extent of injury to have been reasonably closely determined, there is the further difficulty of deciding upon what basis to value the losses in financial terms. Destruction of produce is always a loss to a country as a whole, but to individual producers it may mean either loss or gain since reduced quantities may command higher prices than could otherwise have been obtained, and in assessing the loss we have only market prices for guide. Figures of losses must therefore necessarily be unreliable but, however conservatively estimated, they run into many millions of pounds per annum in this country alone; and though it may be argued that moderate attacks are only of slight importance to the individual grower, none can afford to ignore the risk of serious loss and must needs adopt protective or remedial measures. The cost of such measures is necessarily a dead loss and, on horticultural holdings, runs to a considerable sum per acre.

The question of measuring the actual extent of loss due to the depredations of insects and diseases in any given case appears to be a somewhat neglected aspect of the subject. It has a very important bearing on the choice of suitable control measures. If

[1] The substance of a paper contributed to a discussion on "The Practical Problems of Crop Production" in Section M (Agriculture) at the meeting of the British Association at Cambridge in August 1938.

cost did not enter into the question, the scientist's job of devising means of control would be a comparatively simple one, but he must take into account the relation between the expense involved in carrying out proposed measures and the value of the injuries that they are designed to prevent. Protective and control measures must be economically practical. This is perhaps a platitude, but it is emphasised since plant pathologists and entomologists are sometimes charged by farmers with being unpractical in their recommendations. It is, however, often a very difficult matter to judge the probable extent of damage to a crop and to decide whether measures of prevention or control will be economically justified, and more work is unquestionably needed on methods for obtaining a much closer estimate than is at present normally possible. The application of the simplest control measures is bound to cost something and it is probable that where losses are likely to be below a certain percentage (a figure which will differ very greatly according to the disease or pest concerned and the crop attacked) the best advice may be " It is not worth while doing anything." To be in a position to take this attitude, we need much more accurate data than are at present available as to losses actually incurred from attacks of pests and diseases, and in passing it may be suggested that there is a line of work here that might profitably be taken up by research workers. It has up to the present received more attention in other countries than here.

It follows, from the need to consider control measures strictly in relation to the value of the crop, that the problems are usually less difficult where the value of the produce per acre is high, since there is then likely to be a larger margin of profit. This is reflected in the results of research work in recent years which have led to greater advances in the control of pests of fruit and other horticultural crops than of purely agricultural crops. One may instance the methods now available for dealing with Apple Scab and with aphides (green fly) on fruit trees, with Raspberry Beetle, Apple Sawfly and Plum Sawfly, all important and destructive pests, for which, not many years ago, no means of control, or at best only unsatisfactory means, were known. It is not, however, desired to give the impression that the more specifically agricultural problems have been neglected, and the introduction of mercurial seed dressings for control of certain seed-borne diseases of cereals may be quoted as an instance of progress on this side of the subject.

It would be obviously impossible in a short paper to review adequately the pests and diseases of crops and means for their control, and it would seem most useful simply to attempt to

indicate briefly some of the general principles underlying the working out of control measures. To do this, it is necessary first to consider the chief factors affecting the *natural limitation* of the organisms concerned since our object should be to supplement rather than to replace such factors.

It will be evident, in the first place, that *climate and weather* markedly influence the abundance of insects and fungi. Certain pests, for example, that cause much loss in other countries fail to establish themselves here although their food plants are grown in quantity; and we have to thank our much maligned climate wholly or partly for that. Studies of the precise relationships between meteorological conditions and particular pests or diseases are also leading to the accumulation of information which is helpful in attempting to define their possible geographical range, and in some instances, notably in America, this has been done with considerable accuracy. The subject is of much practical importance in connection with measures that may be taken to try to prevent the introduction of new pests into a country, for it is useless to enact legislation to keep out pests which could not establish themselves if introduced.

In other directions, too, the study of weather in relation to the movement and abundance of insects has given results of much practical value, and an interesting example concerns the possibility of growing stocks of seed potatoes relatively free from virus diseases. The investigations of Maldwyn Davies have shown that the meteorological conditions in certain areas are such that the population of aphides (green fly) which are carriers of virus diseases is at a minimum, and in such districts virus-free stocks can successfully be raised.

A second important factor is the *resistance to attack* shown by the plants themselves. As is well known, different varieties and strains of both wild and cultivated plants exhibit wide differences in the extent to which they are subject to infestation by pests and diseases, some being highly susceptible, others highly resistant or even completely immune. Plant breeders can take advantage of such variability by attempting to raise new varieties incorporating high resistance or immunity with other desirable qualities, and it will be realised that where this can be successfully done, we have the most fundamental and satisfactory means of dealing with the problem, for artificial control measures then become no longer necessary. Considerable success has, in fact, already been obtained in some instances, particularly in regard to resistance to fungus diseases. For example, the well-known work of Professor Biffen with the Yellow Rust of Wheat may be recalled, in which, by

hybridisation, he successfully combined resistance to the Rust with other desirable qualities, work which led to the production of the variety Little Joss. In more recent years efforts have been made, and are continuing, in this country and also in Germany and the United States, to breed varieties of potato resistant to "Blight" (*Phytophthora infestans*). The testing and selecting of seedlings and varieties of potato for resistance and immunity to Wart Disease is another example, and work on resistance to Anthracnose of beans is also in progress. There is here a fruitful field of work for the further attention of plant breeders.

Instances of the production of new varieties of plants highly resistant to insect pests are less numerous, but mention may be made of the long-continued investigations at Oxford on the breeding of oats resistant to Frit Fly, which have reached the stage of practical trials with certain promising strains.

A third factor of no less importance, affecting the natural limitation of insects and fungi, is the effect of *natural enemies* of the pests or disease-producing organisms. Parasites and predatory insects, birds, and certain mammals, fungous and bacterial diseases all take their toll of insect pests; and fungi are also subject to attack by certain insects and by other fungi. Of these, insect predators and parasites are the most important. They may be entirely responsible in certain instances for preventing the numbers of a potential pest from reaching the level at which it becomes harmful; a balance normally ensues, determined, in the case of parasites, mainly by the relative rates of reproduction of the parasite and its host under the prevailing conditions. In special circumstances the introduction of natural enemies of particular insects has led to strikingly successful practical results and further work is likely to extend the usefulness of such biological control methods.

It must not be thought that the various factors mentioned act in isolation. The relative abundance under natural conditions of a pest or disease is the resultant of the interaction of any or all of them: climatic conditions, host plant resistance, natural enemies, available food supply. As Dr. W. R. Thompson has put it: "Outbreaks of species are simply due to the fact that conditions momentarily correspond or approximate to the ecological optimum; control means simply a departure in one or many directions from optimum conditions to the point where the species is just able to maintain itself." The ultimate practical aim of the study of the various natural agencies acting upon pests and diseases is to place us in possession of accurate data on which to base methods for altering the conditions in favour of the crop—*i.e. methods of control.*

We may roughly classify control measures into two main groups:

(*a*) Those which are aimed directly at destruction of the pest or disease organisms; these are usually chemical or mechanical measures, though biological control methods also come under this heading.

(*b*) Those which are aimed at assisting the crop to escape from attacks or at any rate to avoid the more serious forms of injury. The production of resistant varieties and certain cultural measures come under the second group.

It is not proposed to discuss direct methods of control by the use of insecticides and fungicides, for the subject cannot be usefully treated in very brief fashion. It is, however, necessary to say that insecticides and fungicides (sprays, dusts, poison baits, and other forms) are at present our chief weapons in the fight against pests and diseases of crops. In a sense they are palliatives and we may imagine a time in the future when only immune varieties of cultivated plants will be grown or when cultural methods are so well understood and applied that the natural resisting power of the crops will be such that pests and diseases affect them little. At the moment, however, this state of affairs seems a long way off; we must continue to look to chemical methods as our chief means of control, at any rate so far as horticultural crops are concerned, and it is indeed in this direction that most progress has been made.

We come therefore to control measures that may be conveniently grouped under the term *cultural.* Most of the ordinary processes of crop husbandry, in so far as they aim at the production of healthy, vigorous plants, may be said to have an effect on the extent to which the resulting crops suffer from infestation by pests and diseases since, other things being equal, healthy crops may be expected on the whole to be more resistant than those in poor condition, or at least better able to withstand attack. Sir Albert Howard has recently stressed this point of view in several cogent addresses and papers. He holds that insects and fungi attack *only* unsuitable varieties or poorly grown crops and that all our efforts should be directed towards the production of perfectly healthy plants, in particular by seeing to it that the supply of humus in the soil is well maintained. On this view, it is a mistake to use insecticides or fungicides since the pests or diseases are useful in indicating crops poorly grown or unsuited to the particular soil or climate—and such crops should either be better grown or their cultivation abandoned. Although probably few biologists find it possible to go the whole way with Sir Albert Howard, it is most valuable to have had attention drawn so strongly to the important

influence of the factor of plant health on infestation by pests and diseases. That many other factors (some of which have been referred to) are also concerned seems certain, and the difficulty is to disentangle the effect of the health factor from the others. Instances of severe attacks upon apparently healthy and vigorous crops can be cited, and we cannot afford to abandon measures aimed more directly at control or protection.

The importance of *cultural control measures* in this connection is apt to be underestimated. It is true that they are rarely completely effective, but in a great many instances their judicious use at the appropriate time may greatly reduce losses from pests or diseases and they are usually cheap as compared with the use of insecticides or fungicides. Measures such as the application of fertilisers or thorough cultivation, ploughing, or rolling, may at times so help the crop that it is enabled to grow away from attack. Such methods are, for example, frequently helpful in reducing the harmful effect of wireworms, and since chemical methods against this pest, such as the application of soil insecticides, are seldom economically practicable at present, suitable cultivation has to be relied on to a great extent in our efforts to keep it in check. The satisfactory control of wireworms, one of the most troublesome of agricultural pests, is, however, still an unsolved problem, and it is one to which investigators are giving special attention at the present time. In spite of all the work that has been done on the subject (a review of the literature, published in 1930, listed more than 470 papers and there have been many since) there is still much to be learned about wireworms.

An interesting example of the possibilities of cultivation as a control measure is the outcome of quite recent work at Rothamsted Experimental Station on the " Take-all " or " Whiteheads " disease of wheat due to the fungus *Ophiobolus graminis*. The fungus persists on the infected stubble and only slowly disappears through natural decomposition. It appears that the rate of its disappearance is closely associated with the numbers and activity of the micro-organisms in different soils, and it has been found that their activity may be increased and the destruction of the fungus hastened by incorporation of additional organic material with the stubble and by subsequent cultivations of the soil. Further field work on the subject is in hand, and it is possible that similar methods may be applicable in connection with other fungi in infected plant residues.

Clean culture and destruction of weeds are also important in relation to pest and disease control, for weeds often serve as host

plants on which populations may be built up ready to infect the crop when it reaches the appropriate stage.

Alteration of the time of sowing or planting, or the use of varieties naturally earlier or later than the normal, is another means which may enable crops to escape attack, and has proved specially valuable in dealing with certain pests of cereals, such as the Frit Fly and the Hessian Fly. The latter was formerly a most destructive insect in America, but is now little feared, since the precise time for sowing, in order to escape its attentions, is known for all the important wheat areas.

Finally, under the heading *cultural means* is the important subject of *crop rotation*. Where the same crop is grown for a number of years in succession on the same land, or on closely adjoining areas, the chances of infestation by pests and diseases are at a maximum, and one of the advantages of the ordinary practice of rotation of crops is to diminish this risk. Obviously no hard-and-fast rules can be laid down, since so many considerations other than pest control must be taken into account; but as a general principle, wherever conditions allow, a crop should be grown as far removed as possible, both in time and space, from fields carrying the same crop or other crops susceptible to the same pests and diseases. This is perhaps a counsel of perfection, but the matter undoubtedly deserves more attention than it often receives and avoidable losses are not infrequently incurred by failure to bear it in mind. It is probable that the apparent spread of the "Take-all" disease referred to above, is connected with changes in methods of farming in which the interval between cereal crops is shorter than in the older rotations. It is, no doubt, present-day economic conditions that lead to neglect of the old principle of rotation, but such neglect will almost inevitably involve certain penalties.

Crop rotation and the resting of land for some years from a particular crop are of special importance in connection with certain pests, of which the Potato Root Eelworm is a prominent example. This eelworm is associated with the "Potato Sickness" which has caused such serious losses in recent years. The problem of its control is one of special difficulty and has been and continues to be the subject of intensive investigation by research workers. Progress is being made in various directions but no economically practical and generally effective method for destroying the eelworms in infected land has in fact yet been found. To avoid growing potatoes year after year on the same land seems to be at present the only way to prevent infestations from reaching serious proportions; in areas where the pest is abundant, it is risky to grow potatoes more

than once in four or five years, and a small point, perhaps worth mention, is the importance of removing and destroying the odd plants that will grow from tubers left in the soil ("volunteer" plants) in the intervening years. There are several strains or races of the eelworm that attack various plants, but the potato race is not known to infect any other usual farm crop, so that no special precautions in that connection are necessary. These other strains of the Root Eelworm, which attack oats, sugar beet, and peas, are, however, becoming of increasing importance also, because of the tendency to grow these crops too frequently on the same land.

As another example, certain troublesome pests of peas—the Pea Midge and the Pea Thrips—are exceedingly difficult to deal with except by resting the land from this particular crop, and in certain districts in Switzerland where these pests cause serious losses, a regular rotation over wide areas is made compulsory. Similarly, in certain areas in the United States, the growing of crops of peas in the same districts, without a suitable interval, is prohibited officially.

Closely related to the question of rotations is the bearing on the relative severity of infestation of the increasing tendency to specialise in particular crops in certain districts. By growing the same plant over wide areas, inevitably conditions are made even more favourable for the insect and fungus enemies of that plant than where general mixed farming is the rule. It may be that under modern conditions this tendency towards concentration of crops of the same kind in particular areas is inevitable, and it is well that we should face the fact that, with increasing specialisation, there is likely to be an increased incidence of pests and diseases and that the correct application of suitable control measures will become more, not less, necessary. Continually more favourable conditions for rapid multiplication of pests are being provided in the larger and larger "single crop" areas.

Though it seems certain that, for a very long time, work on the direct problems of control of pests and diseases must take precedence, yet it is of the utmost importance that the longer-range problems concerning the plants themselves and the environmental factors affecting their response to attack should receive more attention. Entomologists and mycologists concerned with the direct study of the insects and fungi attacking plants and of bacterial and virus diseases will be in a better position to make their contribution to the outstanding practical problems of control when they are in possession of fuller information about the part played by the plant itself.

THE HYDROGEN BOND AND OTHER LINKS BETWEEN MOLECULES

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The group of atoms which constitute a "molecule" in the usual sense is generally held together by forces very much greater than those which unite adjacent molecules in the solid state. Were the intermolecular forces as strong as those holding together the atoms within the molecule itself we might expect the crystals of organic compounds generally to be as hard as diamonds, instead of which they are frequently among the softest substances known. When ordinary hydrocarbons, like paraffin wax or naphthalene, are dissolved or vaporised, the weak binding forces between the molecules readily give way. The molecules come apart as individual units and retain their identity.

It has long been known to chemists, however, that the molecules of certain organic compounds, especially those containing oxygen and hydrogen, do not separate so completely into individual units when dissolved or vaporised. They are "associated" in some way, and remain together in small groups. Furthermore, it is well known that the crystals of such compounds, of which the sugars are typical, are considerably harder and more brittle than those of hydrocarbons like naphthalene. It would appear, therefore, that the forces which hold together the molecules of these associated compounds are somehow intermediate in character between the very weak, residual, or "van der Waals" forces which unite ordinary organic molecules (hydrocarbons, etc.) in the solid state, and the much stronger forces which unite the atoms in the molecules themselves.

The powerful X-ray methods which have recently been developed for studying matter in the crystalline state (see, for example, Science Progress, **32**, 246, 1937) have thrown a great deal of light on the manner in which different types of molecule link themselves together, and the results are particularly interesting when we contrast the "associated" organic compounds with the more ordinary hydrocarbon type. In this article we consider first very briefly some

chemical facts bearing on such association, and then review the matter in the light of recent X-ray results from the solids.

Chemical Aspects

The association of hydroxylic compounds has been familiar to chemists for about half a century. Carboxylic acids, alcohols, and phenols are well known to yield a variety of abnormal physical data, leading to high molecular weight values. The acids are usually found to be dimeric, in non-associated solvents, and with formic and acetic acids the double molecules persist even in the vapour at the boiling point. With the alcohols and phenols, on the other hand, the polymerisation may be less in dilute solutions but can go on apparently to much higher upper limits.

In all these cases the power of polymerisation appears to reside in the OH group. Oxygen and hydrogen both appear to be necessary. There are, however, other compounds without oxygen, but containing instead the neighbouring elements in the periodic table, nitrogen or fluorine, which are also found to associate in this way. HF and HCN are examples. The conclusion seems to be that the hydrogen atom at least is one essential constituent in such polymerisations, and hence the conception of an intermolecular hydrogen bond arose. The term " hydrogen bond," although generally used, is not a very good one in this connection as it might equally well denote the usual covalent link by which hydrogen is attached to another atom in any ordinary compound. For the intermolecular link, " hydrogen bridge " is probably a better term, as employed by Huggins.

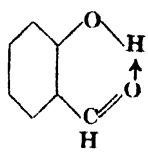
The essential rôle of the hydrogen atom in such linkages between molecules was first clearly formulated in terms of the electronic theory by Huggins, and by Latimer and Rodebush, in 1920 (*J. Amer. Chem. Soc.*, **42,** 1419, 1920). They argued that in NH_3, hydrogen is readily added to form ammonium compounds, but there is little tendency to give up hydrogen. In HCl, on the other hand, the tendency is just the reverse. Hydrogen is readily given up, but not added. Now H_2O is intermediate. The tendencies to add and to give up hydrogen are almost equally balanced, as shown by the formation of the ions H_3O^+ and OH^-. Thus they supposed that a free pair of electrons on the H_2O molecule might exert sufficient force on the H of another H_2O molecule to bind the two molecules together :

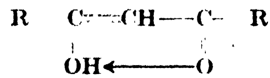
$$\begin{array}{ccccccc} & & & & H & & \\ & & \cdot\cdot & & \cdot\cdot & & \\ H & : & O & :H: & O & : & \\ & & \cdot\cdot & & \cdot\cdot & & \\ & & & & H & & \end{array}$$

The process might go on to unite more than two molecules, the hydrogen acting as acceptor for two electrons from the oxygen as donor. The situation was later formally represented by means of co-ordinate links :

$$\begin{array}{ccccc} R & & R & & R \\ OH & \leftarrow & OH & \leftarrow & OH \end{array}$$

Sidgwick and his co-workers from 1915 onwards had realised the necessity for such an explanation of the peculiar properties of certain ortho-substituted phenols and enolised β-diketones (*J. Chem. Soc.*, **527**, 1924 ; etc.). The association which would normally be expected in such hydroxylic compounds is decreased or eliminated, and this can most readily be explained by assuming an *intra*-molecular bridging through the hydrogen in these compounds, as shown by the formulæ :





These conceptions, however, are revolutionary because they give the hydrogen atom a covalency maximum of 2. From the chemical point of view this appears to be definitely necessary, as we have seen. But on the physical side it soon became clear that this view was untenable in so far as it implied a group of four shared electrons for the hydrogen atom. The hydrogen atom can only have two electrons in the first quantum group, and as for the second group, it was pointed out by Pauling (*J. Amer. Chem. Soc.*, **53**, 1367, 1931) that no strong enough bonds could be formed. He proposed a purely electrostatic structure for the hydrogen bond, *e.g.* instead of

$$\left[:\ddot{\underset{\cdot\cdot}{F}}:H:\ddot{\underset{\cdot\cdot}{F}}: \right]^-$$

we should have $[F^-H^+F^-]^-$ and he showed that there is some evidence for the ion $[O^{=}H^+O^{=}]^{\equiv}$

More recently it has been pointed out that the theory of resonance may give the most satisfactory explanation of the hydrogen bond (Sidgwick, *Chem. Soc. Annual Reports*, **31**, 40, 1934). The hydrogen atom may belong to one or the other of the two molecular structures involved in the resonance, and the resulting system will have a lower potential energy and be more stable than the uncombined form. The theory is very attractive, and depends largely on the evidence

of internuclear distances obtained by diffraction measurements, which we consider more fully in the following pages. But the whole question of intermolecular resonance is a difficult one, and its investigation by modern physical methods is only just beginning.

Physical Methods of Investigation

We now come to consider the more precise picture of these molecular associations obtained by modern physical methods of investigation, particularly by the determination of crystal structures by X-ray analysis. These measurements have played a large part in giving definite information about the hydrogen bond, and it is now possible to study very precisely the manner in which molecules group themselves together in the solid state. With regard to the actual mechanism of association, however, diffraction measurements themselves cannot be conclusive, because of their inability to locate the hydrogen atom directly. The scattering power of the hydrogen atom for X-rays is so small compared with that of the neighbouring oxygen atoms that its actual position has never been determined directly in any of these experiments.

Spectroscopic methods overcome this difficulty, and by this means it is sometimes possible to estimate the distance between a hydrogen atom and its neighbouring oxygen atom. In associated hydroxylic compounds a shift of the characteristic OH frequency is obtained, indicating a modification of this group. The complete analysis of these results is a very complicated matter, and not by any means complete, although a great deal of work is at present being carried out in this direction. A discussion of the results at their present stage is beyond the scope of this article.

Another important line of investigation relating to the mechanism of association has recently been commenced by means of experiments with the heavy isotope of hydrogen. Isotope substitution has no effect on the electronic situation, but must lead to important vibrational differences which affect the intermolecular approach distances. Further reference to these experiments is made in the last section.

X-Ray Results. Hydrocarbons and Non-associated Compounds

An essential feature of any crystal is its periodic structure. It consists of a regular array of identical units, as in Fig. 1. Each of these units may consist of an atom, a molecule, or even a group of molecules, but in every case there is some distribution of electrons capable of scattering X-rays. If X-rays are incident on the crystal,

at certain definite angles (θ) we get a strong diffracted beam in accordance with the Bragg relation

$$n\lambda = 2d \sin \theta.$$

In this way we can determine d, the spacing of the crystal planes, and so find out the distances between the structural units which build up the crystal. If these units are single atoms, the analysis is simple. But if they consist of complex molecules, as in the structures we are now studying, the analysis is much more complicated. It is then necessary to find out the arrangement of the atoms within the molecule, and the orientations of the molecules relative to each other.

This is done by studying the intensities of the diffracted beams

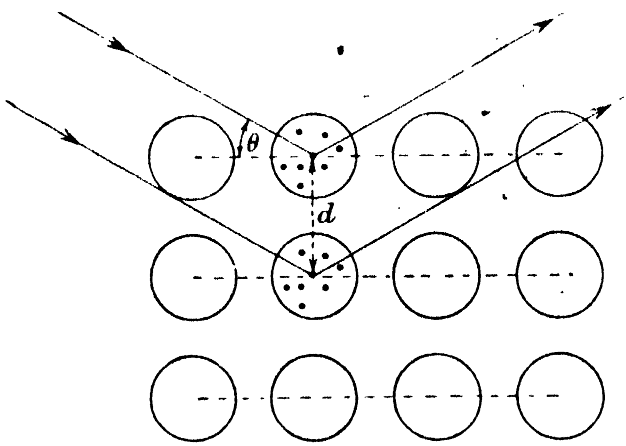


FIG. 1.

from a large number of different crystal planes. Each recorded intensity gives a measure of the distribution of scattering electrons normal to the plane in question, and by a mathematical analysis of the results from many planes it is often possible to derive an accurate picture of the electron distribution, and determine the positions of all the atoms. We deduce in this way not only the structure of the molecule itself, *i.e.* the relative positions of all the atoms in one of the circles of Fig. 1, but also the precise arrangement of these molecules with respect to each other. It is with this latter aspect concerning the grouping of the molecules that we are at present concerned.

It should be mentioned that a lengthy research is generally involved in these accurate determinations of structure, and consequently the number of examples which we can study is limited,

It is now possible, however, to contrast the molecular grouping of some typical hydrocarbons (non-associated) with that of certain hydroxylic compounds (associated). The results are expressed by means of skeleton diagrams drawn to scale (Figs. 2–7) which represent normal projections of the structures along certain crystallographic directions. As the molecular planes are frequently inclined at high angles to these projection planes the distances between the atoms in the diagrams are often foreshortened to a considerable extent. Nevertheless, the figures express clearly the general character of the molecular grouping.

Fig. 2 illustrates the arrangement in durene (sym. tetramethylbenzene). The very open structure with large gaps between the

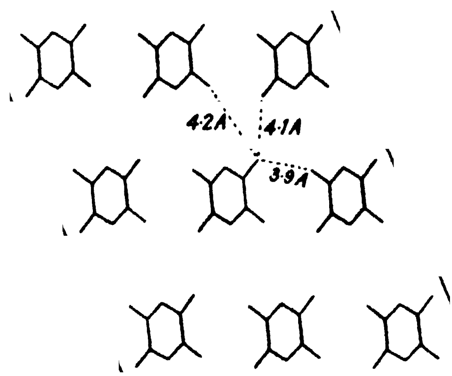


FIG. 2.—Durene.

molecules is typical of compounds of this class, where the outer parts of the molecule are completely saturated with hydrogen atoms and no electronegative groups are present. Only weak, residual or van der Waals attractive forces hold the structure together, and the crystals have a soft, almost soapy texture. The hexamethylbenzene arrangement (Fig. 3) is very similar (although in this case the planes of the molecules practically coincide with an important crystal plane which has been chosen as the plane of the drawing, and so none of the distances appear foreshortened). In all these drawings a few of the minimum intermolecular approach distances between the carbon atoms [1] are indicated by dotted lines with the values written alongside in Angstrom units. In durene and hexamethylbenzene

[1] As it is impossible to locate the hydrogen atoms by X-ray analysis, they are not represented in any of the drawings. The dotted lines connect the nuclei of the heavier atoms.

these minima range from 3·9 to 4·2 A, and we may especially note that there is no particular order in the arrangement of the dotted

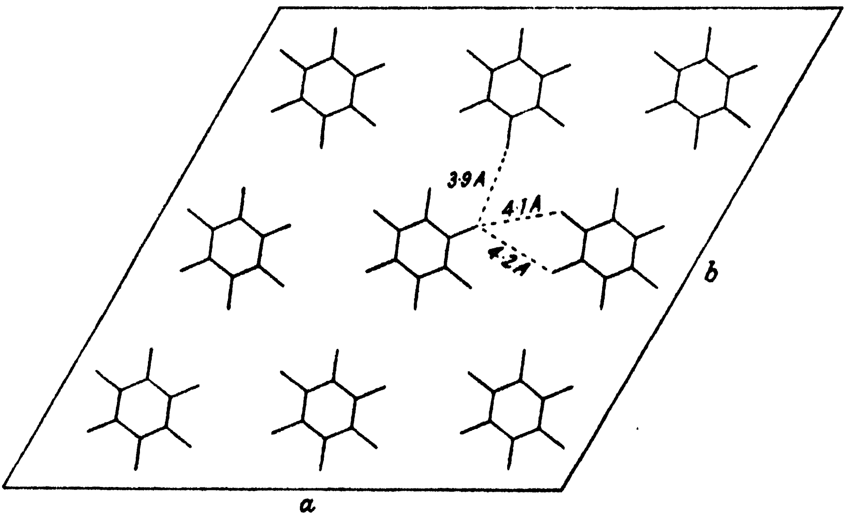


FIG. 3.—Hexamethylbenzene.

lines. Many other approach distances nearly as short as these can be found, and there is certainly nothing in the nature of inter-

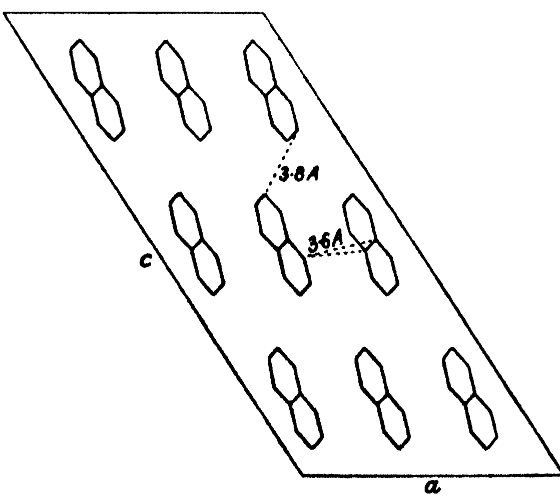


FIG. 4.—Naphthalene.

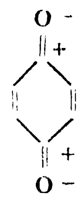
molecular " bonds " uniting the molecules. They are simply packed together in the most economical way.

In the aromatic hydrocarbons the situation is very similar, but

the minimum distances are somewhat less. Many examples have been determined, and the minimum distances are found in general to range from about 3·5 to 3·8 A. The arrangement in naphthalene is shown in Fig. 4. The planar aromatic skeletons consist of approximately regular hexagon rings, but these are inclined at about 65° to the plane of the drawing. Alternate molecules lie above and below the plane of the drawing, and are oppositely inclined to it. These details do not seriously affect our general view of the structure, and we see again that it is largely a matter of convenient packing.

It is now of interest to compare these results with a similar organic compound containing oxygen, but not the associating group OH.

The molecule of benzoquinone



contains strong dipole groups, and we might well expect some closer grouping in the crystal

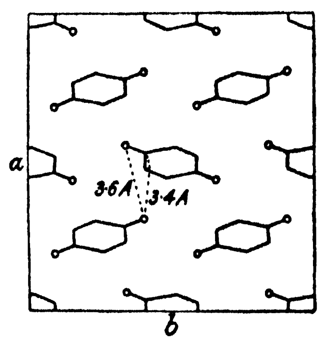


FIG. 5.—Benzoquinone.

that would tend to bring these oppositely charged parts into proximity, especially as there are no surrounding hydrogen atoms attached to these groups. We find (Fig. 5) that the minimum approach distance does occur between the positive carbon and negative oxygen of neighbouring molecules, but the separation is still as large as 3·4 A, not very much less than in the aromatic hydrocarbons.

Associated Compounds

Now let us examine a structure isomeric with benzoquinone except for the *addition* of a hydrogen atom to each of the ketonic

oxygens. Hydroquinone would be the best example, but the crystal analysis is complicated by various factors. Instead, we take resorcinol, the meta-derivative, and the projection is given in Fig. 6. The situation is now suddenly and drastically changed. All the hydroxyl groups are directed towards each other, and the approach distances drop from 3·5 to 2·7 A, between the hydroxyl groups only. All other intermolecular distances are as usual greater than 3·5 A. There is now definite order and arrangement in the dotted lines. The hydroxyls are neatly grouped together and evidently govern the whole situation. The groups of four are actually spiral arrangements extending throughout the crystal, but this cannot be shown in the projection, and it does not affect the present discussion. The angles between the dotted lines, when allowance is made for the perspective of the drawing, are found to be close to the tetrahedral value of 109°.

There is something peculiar about the structure apart from the nearness of approach of the molecules, because taking it all over, it is a comparatively *open* structure. The density is only 1·28. In anthracene the density is 1·25, and in benzoquinone 1·33. Now this low density of resorcinol compared with benzoquinone is maintained in spite of a decrease of 0·7 A in the minimum approach distances between the molecules. It is evident that we are dealing here not only with some kind of intermolecular bond, but with bonds which have considerable directive power, and are able to maintain an unusually open structure.

The situation reminds us at once of the structure of ice, which consists of an unusually open tetrahedral arrangement of oxygen atoms, 2·76 A apart. The open structure of ice and the directive power of the bonds which maintain it are shown by the fact that when ice melts the resulting water has a higher density. The open structure has evidently collapsed to some extent.

An interesting analogy to this behaviour has recently been discovered in the case of resorcinol (Robertson and Ubbelohde, *Proc. Roy. Soc.*, A, **167**, 122, 1938). When ordinary resorcinol (α-resorcinol) is heated to about 74° C. it does not melt, but we may say with some justification that the hydroxyl bonds melt. The crystal structure changes, and the molecules rearrange themselves to form a new structure of considerably higher density (1·33 instead of 1·28). When this dense, high-temperature form, called β-resorcinol, is once achieved it is reasonably stable and only slowly reverts to the ordinary form. It is possible to obtain crystals of the two forms simultaneously, and they differ widely in habit and general properties, as can be seen from Plate I. The long needles, sometimes

joined at definite angles, are the α- or low-temperature form, while the plates, which in some cases actually grow out of the needles, are the β- or high-temperature form.

The transformation in resorcinol can be studied more accurately than the ice-water change. We do not know the exact arrangement of the molecules in water, but the structure of the high-temperature form of resorcinol has been completely worked out by the X-ray method, and the arrangement of the molecules is shown in Fig. 7. The open, directed structure of the low-temperature form (Fig. 6) has disappeared, and the molecules have now achieved a more parallel and compact form of grouping which is reminiscent of the hydrocarbons.

The exact mechanism of the change is somewhat difficult to

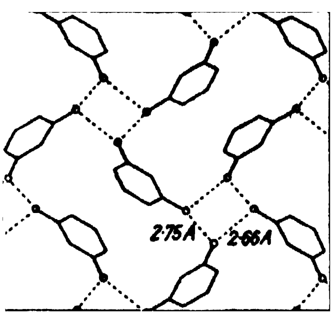


FIG. 6.—Resorcinol.

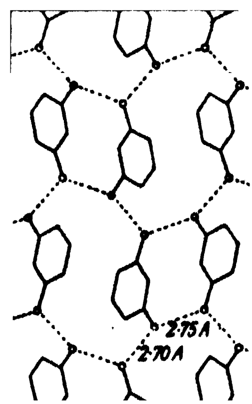
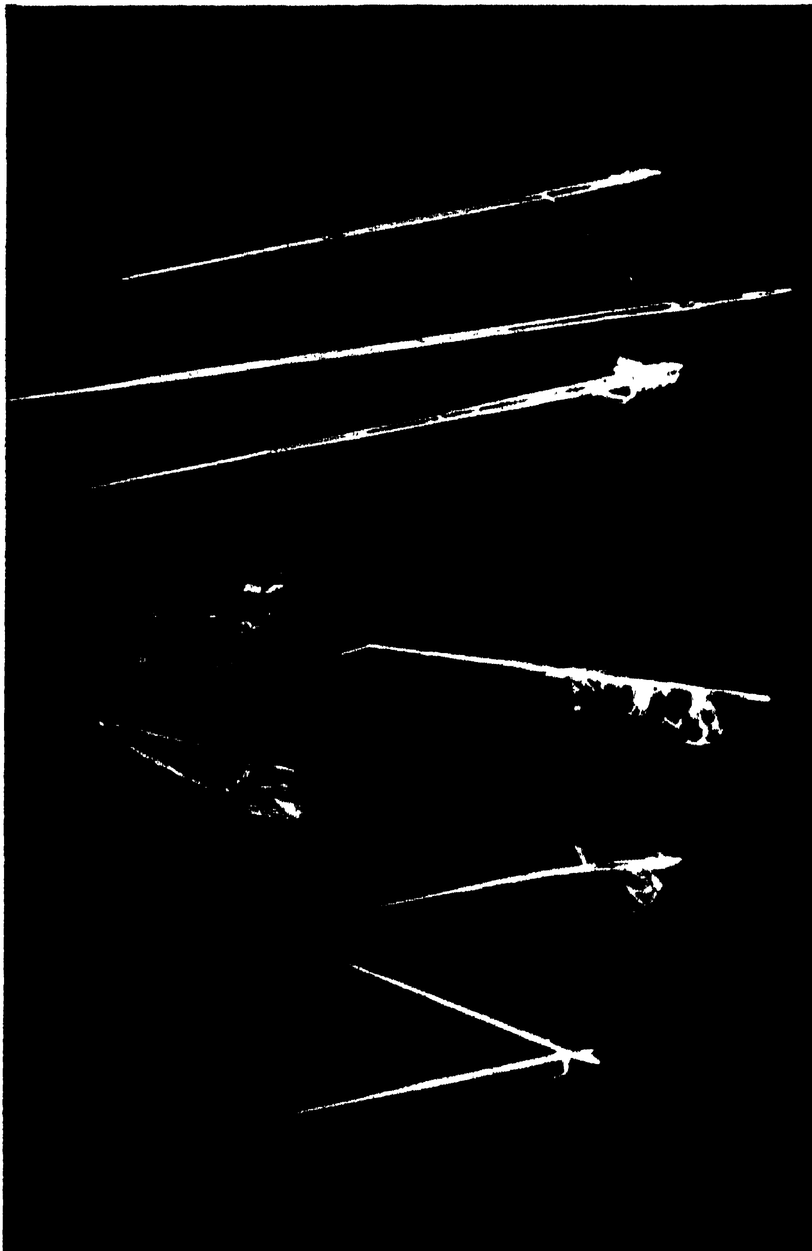


FIG. 7.—β-Resorcinol.

follow. When the temperature is raised the rigidity of the resorcinol molecule appears to break down to some extent, with considerable distortion of the hydroxyl bonds. The molecules can thus achieve a lower van der Waals and polarisation potential, and a more compact structure of higher density results. The hydroxyl bonds, which have re-formed in the β-structure, are somewhat longer and the angles between them now depart considerably from the tetrahedral value. The energy of transition is small, and apparently the lower van der Waals and polarisation energy of the dense β-form compensates to a considerable extent the higher strain energy of the bonds.

The essential point for our present discussion is that, just as in water, a new and more dense arrangement has been formed at a higher temperature. This unusual result is possible because the



Crystals of α and β resorcinol. α (needles), β (plates).

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directive power of the intermolecular bonds in the low temperature or ordinary form maintain a very open structure.

This example has been extensively studied because of its many interesting applications, but strongly directed hydroxyl bonds are not unusual. In fact, they are characteristic of the OH group. In pentaerythritol, $C(CH_2OH)_4$ (Llewellyn, Cox and Goodwin, *J. Chem. Soc.*, 883, 1937), and in oxalic acid dihydrate (Robertson and Woodward, *J. Chem. Soc.*, 1817, 1936) the arrangement of the hydroxyl bonds is very similar to that in resorcinol. Illustrations of these structures will be found in SCIENCE PROGRESS, **32**, 246, 1937. Numerous examples in inorganic chemistry are also known, and a good survey of these has been given by Bernal and Megaw (*Proc. Roy. Soc.*, A, **151**, 384, 1935).

Stronger types of intermolecular hydrogen bond, characterised by a still closer approach of the related groups, are also known. In these a hydrogen atom is usually attached to only one of the adjacent groups, *e.g.* C=O and C—OH. An example occurs in oxalic acid dihydrate, where there is one approach of only 2·52 A, together with larger distances like those in resorcinol. In $NaHCO_3$ and KH_2PO_4 the approach of certain oxygen atoms with a hydrogen atom between is again only 2·55 A (Zachariasen, *J. Chem. Phys.*, **1**, 634, 1933 ; West, *Z. Krist.*, **74**, 306, 1930). In KHF_2 the FHF bridge may be even shorter, a value of 2·25 A having been reported (Bozorth, *J. Amer. Chem. Soc.*, **45**, 2128, 1923). An interesting NHN bridge of about 2·65 A occurs within the phthalocyanine molecule (Robertson, *J. Chem. Soc.*, 1195, 1936).

DISCUSSION OF MECHANISM

The most striking fact which has emerged from the diffraction measurements on associated structures is the great decrease in the normal intermolecular approach distance. In many cases this amounts to nearly an Angstrom unit. This fact in itself gives strong support to the view that some form of intermolecular resonance is involved. But the matter is complicated, and it might for example be possible to explain the hydrogen " bonds " solely by electrostatic attractions, due largely to the unique property of the hydrogen atom of containing no inner electrons and so permitting a close approach to another atom without contributing to the repulsive forces. This consideration undoubtedly does play a large part. As the X-ray measurements fail to determine the position of the hydrogen atom we must consider briefly some other lines of investigation bearing on the problem of mechanism. The main question involved is

whether it is necessary to assume the existence of special resonance forces.

The spectroscopic results reveal, as we have seen, a considerable shift of the OH frequency with association, and this points to a certain modification of the OH group. The alteration, however, is stated not to be enough to change the force constant or alter the position of the hydrogen atom very much ; not enough, for example, to allow the hydrogen atom to be placed midway between the two oxygen atoms which are involved in the bridge. It is reasonable to expect that further analysis will define the position more clearly.

A new line of investigation, in experiments involving the substitution of deuterium for hydrogen, has recently given interesting results (Ubbelohde, *Trans. Faraday Soc.*, **32**, 525, 1936 ; Robertson and Ubbelohde, *Nature*, **139**, 504, 1937). In an isotope substitution of this kind, the electronic situation is not changed, but there is an important vibrational difference, in that deuterium has a smaller zero point energy than hydrogen, in the ratio 1 : $\sqrt{2}$. This should have a definite effect on most intermolecular distances where hydrogen atoms occur on the outer parts of the molecules concerned. In general, for *normal* intermolecular grouping, we would expect slightly *closer* approach distances for the deuterium compounds, in keeping with the smaller space requirement of the deuterium atoms. This seems to be borne out by experiment. For example, in the ionic lithium hydride lattice the spacing has been reported to show a decrease of from 4·080 A to 4·060 A on the substitution of deuterium (Zintl and Harder, *Z. Physik. Chem.*, **28B**, 478, 1935). For covalently bound hydrogen, as in the hydrocarbons, there are no direct measurements, but from a study of the relative densities of C_6H_6 and C_6D_6, for example, it can be inferred that there is a change in the same direction, *i.e.* a small contraction in the intermolecular distances on substituting the heavier isotope.

On the other hand, if intermolecular resonance between two or more structures should be involved, we have a different situation. The problem is a complicated one, but for a rough qualitative treatment we may simplify it by considering only the two oxygen atoms involved in a hydrogen bridge, with the hydrogen (or deuterium) atom somewhere between them. We neglect the rest of the molecule. If the oxygen atoms are far apart, the hydrogen will be attached to one of them, and it will have to pass a considerable energy barrier to reach the other. As the oxygen atoms are brought closer together the passage of the hydrogen atom from one to the other is facilitated. Now the hydrogen atom with its higher frequency of vibration and larger amplitude (greater zero point

energy) will maintain a higher position on the potential energy curve of the structure than the deuterium atom, and will consequently possess a greater tendency to tunnel through the barrier to the other oxygen atom than that possessed by a deuterium atom in the same situation.

The conclusion from this argument is that there should be a greater tendency for intermolecular resonance when a hydrogen atom is involved than when a deuterium atom is involved. If the intermolecular bond is due to such resonance, then we should expect greater resonance and stronger bonds with *shorter* distances, for hydrogen compounds than for corresponding deuterium compounds. In other words, we would anticipate a reversal of the normal effect —an expansion of the intermolecular spacings on substituting deuterium instead of the contraction experienced in non-associated structures.

This conclusion has been tested by experiment for a large number of substances (Robertson and Ubbelohde, *Proc. Roy. Soc.*, A, 1939, *in press*). In oxalic acid dihydrate, which is characterised by a very strong hydrogen bridge of 2·52 A, a marked expansion of the spacings is found to occur. The expansion is directional and can be explained by assuming a considerable expansion of the 2·52 A bridge accompanied by a smaller contraction of the other longer intermolecular links. $NaHCO_3$ also shows an expansion, but of smaller amount. Other small expansions are recorded for various carboxylic acids, and for α-resorcinol. In β-resorcinol and in ice there is very little effect. It should be noted, however, that no effect means that the normal effect (contraction) has been compensated in some way.

The provisional conclusion from these experiments is that in the shorter types of hydrogen bridge at least special resonance forces do seem to play some part.

THE DIRECTIONAL SENSITIVITY OF THE RETINA

By W. S. STILES, Ph.D.
The National Physical Laboratory, Teddington

Apparent Brightness and Pupil Area

It has generally been assumed that the *apparent* brightness of a surface is proportional to the area of the pupil of the viewing eye. By this is meant that if the area of the pupil is increased, let us say doubled, and at the same time the actual brightness of the surface is reduced to one-half, then the surface will appear to the eye to have the same brightness as before the change. Thus the eye was assumed to act in this respect like a simple camera in which the blackening of the photographic plate is determined, for a fixed exposure, by the product of the brightness of the object photographed and the area of the camera stop. Recently, however, it has been found that the apparent brightness perceived by the eye does not follow this simple law, the discrepancy being most marked when the pupil is large. The assumption of proportionality between apparent brightness and pupil area leads to no conclusions which are readily checked by observation under ordinary conditions of vision. The failure of the law was first brought to notice by the anomalous results obtained with an instrument designed to measure the area of the pupil by a photometric method.

In this instrument the eye viewed a photometric field divided into two halves, U and L (Fig. 1(a)). The upper half U was illuminated by the method of Maxwellian view, an image of the small source S_1 being formed by the lens l at the centre of the pupil of the observing eye O (Fig. 1(b)). All the light in this beam was collected by the eye and the apparent brightness of U was independent of the pupil size. The lower half L, on the other hand, was illuminated by the diffuse light from the opal glass D and the cones of rays from each point of L completely filled the pupil of the eye at O. Thus the apparent brightness of L depended on the pupil size. The idea of the instrument was to match the brightnesses of U and L by varying the calibrated optical wedge W and

to deduce from the readings the size of the pupil under different conditions of illumination. Pupil diameters obtained in this way never exceeded 5·5 mm., even when the pupil was fully expanded

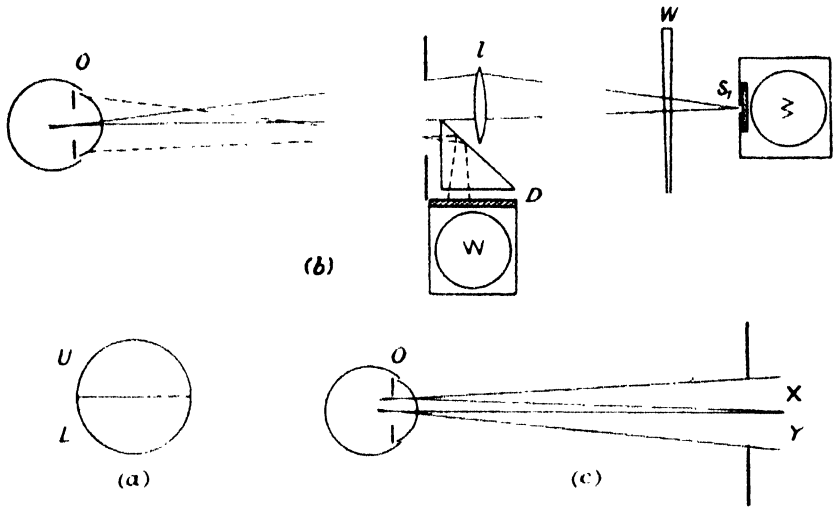

FIG. 1.

and flashlight photography gave values round about 8 mm. A discrepancy of this kind would arise if a given flux of light entering through the outer zones of the pupil contributed less to the apparent brightness than an equal flux entering near the centre.

Luminous Efficiency of Rays entering the Pupil at Different Points

The above suggestion was tested directly using a modified apparatus in which both halves of the photometric field were illuminated by the Maxwellian method (Fig. 1(*c*)). The converging beam from one half of the field (Y) was brought to a small focus at the centre of the pupil. The converging beam from the other half (X) could be turned through small angles so as to bring the focus to any desired point of the pupil aperture.

Suppose that to obtain a brightness match in the apparatus the beam from X had to be given an intensity I_0 when it entered through the centre of the pupil and an intensity I_p when it entered through any other point P. The ratio $\eta = I_0/I_p$ is a measure of the effectiveness of the rays through P in producing the sensation of brightness compared with the rays through the pupil centre. η is termed the relative luminous efficiency of the rays through P.

In Fig. 2 the values of η obtained for a particular eye as the point of entry P of the beam from X was traversed across the horizontal diameter of the pupil are plotted against the distance from P to the pupil centre. Similar curves were obtained for other eyes and for traverses of other diameters. These results show that the relative luminous efficiency is greatest when the ray enters the eye through a particular point P_M which is usually near the centre of the pupil. As the point of entry moves away from P_M the relative luminous efficiency diminishes, slowly at first and then more rapidly, until at 3 mm. from P_M it is equal to about

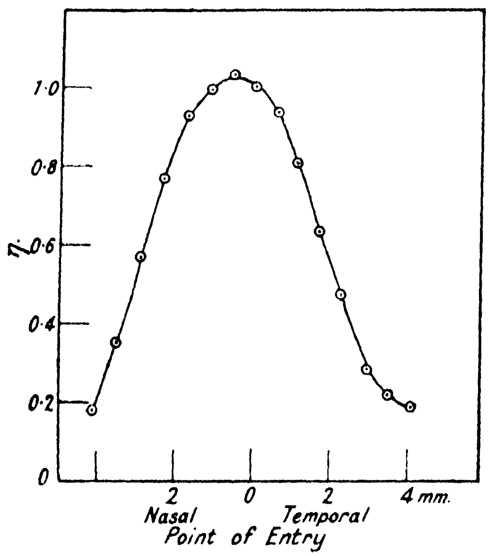

FIG. 2.

one-third its maximum value. To a first approximation, the relative luminous efficiency is the same at all points of entry lying on a circle with P_M as centre. Putting η_m for the relative luminous efficiency at P_M, its value at a point P, distant r mm. from P_M, is given approximately by the empirical formula $\eta = \eta_m 10^{-pr^2}$ where p has a value round about $0{\cdot}05^{-2}$ mm.

The variation of relative luminous efficiency with point of entry satisfactorily accounts for the fact that apparent brightness is not proportional to pupil area.

The above results, obtained in the first instance by B. H. Crawford and the writer [1], have since been confirmed by the work of other investigators [2, 3, 4, 5].

DIRECTIONAL SENSITIVITY OF THE RETINA

In the diagram of an equatorial section of the eye shown in Fig. 3(a), the track y represents the path of the pencil of rays from the test field when the pencil enters at the centre of the pupil, and the track x when it enters near the edge of the pupil. The tracks converge to the same point Q of the retina since the subject always looks directly at the test-field which is therefore imaged on the most sensitive spot of the retina—the fovea. The light losses suffered in the eye media and at the refracting surfaces by ray pencils

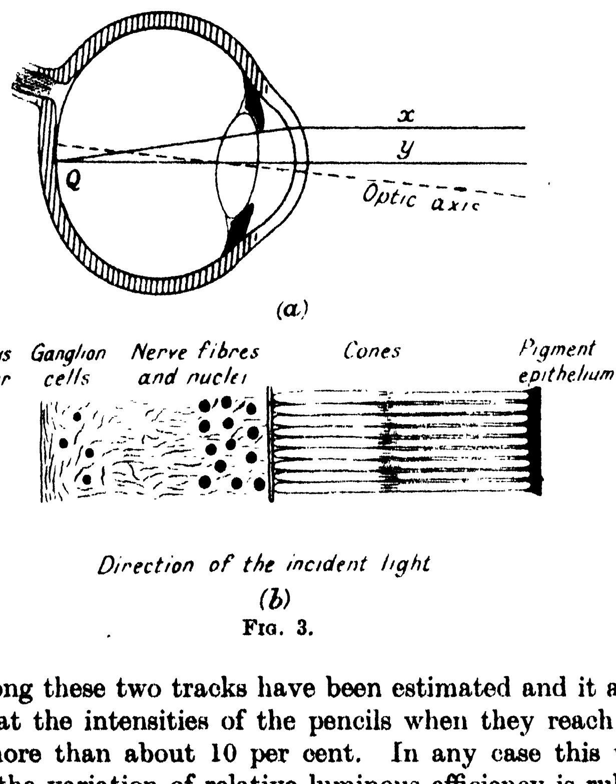

FIG. 3.

incident along these two tracks have been estimated and it appears unlikely that the intensities of the pencils when they reach Q can differ by more than about 10 per cent. In any case this way of explaining the variation of relative luminous efficiency is ruled out by later observations. It must be concluded that we are dealing with a property of the retina and that the sensation of brightness produced by a ray depends on the angle at which it strikes the retina. If the ray track y is regarded as defining the normal to the retina at Q then the angle of incidence for the ray track x will equal $\left(\frac{57 \cdot 3r}{24}\right)^\circ$ approximately, where r is the separation of the two tracks outside the eye. When $r = 3$ mm., the angle of incidence

equals about 7° and in this small angle the brightness sensitivity of the retina has already dropped to a third of its value for normal incidence.

The first attempt to explain this highly directional property of the retina in terms of retinal structure made use of the fact that in the light-adapted eyes of some animals the light sensitive elements —the rods and cones—are separated by cell processes containing pigment particles. The diagram of Fig. 3(*b*) represents a section of the human retina in the foveal region. The incident light penetrates the layer of ganglion cells, nuclei and nerve fibres, passes through the cones (rods are absent at the fovea) and is finally absorbed by the pigment epithelium. The cones are long, thin and close-packed and it can be calculated that the ray incident on the retina at an angle of 7° will pass through 3 cones before reaching the pigment epithelium. The presence of pigment particles between the cones would prevent such a ray from passing beyond the first cone so that the length of the light track in the cone substance would be no more than a third of that for a ray incident normally and passing down the axis of the cone. Add the assumption that the visual effect of the light is determined by the volume of cone substance illuminated and a variation of visual effect with angle of incidence is obtained which is of the right order of magnitude. Unfortunately for this theory, ensheathing of the cones by pigment particles probably does not occur to any great extent in the human eye. Moreover, even though the histological evidence is not conclusive, the pigment theory in the form just given is difficult to reconcile with the observations to be discussed later.

Another suggestion, due to Wright and Nelson, is that the rays which enter the end of a cone are prevented from escaping through the longitudinal surface because the cone substance has a greater refractive index than the surrounding medium and total internal reflection occurs. This will hold good until the angle of incidence of the light exceeds a certain value when some of the rays will escape. The critical angle will depend on the precise shape of the cone and on its refractive index relative to the surrounding medium. Although there are difficulties, this suggestion is a very interesting one and is being explored further.

Observations with Monochromatic Light

It was shown by the earlier work that the variation of η was similar for white light and for coloured lights. The effect of colour was later re-examined using narrow spectrum bands [6]. It is convenient to express the magnitude of the variation of η by giving

the value of the coefficient p in the empirical formula $\eta = \eta_m 10^{-pr^2}$. As shown in Fig. 4(*a*), p varies in some measure with wave-length being greatest in the blue end of the spectrum. In making these measurements with narrow spectrum bands it was observed that as the point of entry of the light rays traversed the pupil, the apparent colour of the test field varied as well as its apparent brightness. For example, with light of wave-length 579 $m\mu$ the traversing beam became relatively red as its point of entry was

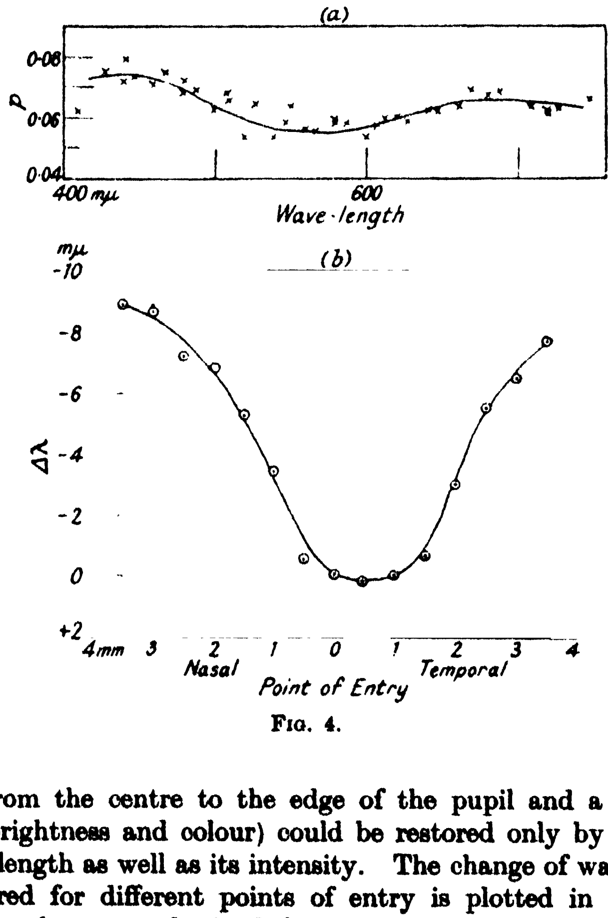

FIG. 4.

moved from the centre to the edge of the pupil and a complete match (brightness and colour) could be restored only by changing its wave-length as well as its intensity. The change of wave-length $\Delta\lambda$ required for different points of entry is plotted in Fig. 4(*b*). Similar results were obtained for other wave-lengths in the red, orange and blue, but in the green and blue-green the curves were of a more complicated form and the change of hue was accompanied by a change of saturation.

For a coloured light made up of wave-lengths from different parts of the spectrum, changes of colour of the kind just described

might arise because the apparent intensities of different wave-lengths are reduced to different extents as the point of entry of the beam moves from the centre to the periphery of the pupil. Obviously this explanation cannot apply to the colour changes observed with monochromatic light and an appeal must be made to the trichromatic theory. According to this theory our sensations of colour are mediated by three mechanisms which, when excited in relatively different degrees, give rise to all the variations of hue and saturation perceived by the eye. It is frequently assumed that there are three distinctive types of cone, the spectral sensitivity curves of all cones of one type being identical or nearly so. It may be, however, that the spectral sensitivity curves of the individual cones vary, almost in continuous fashion, from one cone to another, and a grouping of the cones into three systems with appropriate resultant spectral sensitivities may be effected in higher levels of the nervous system (Guild [7]).

On either of these views, the change in the apparent colour of monochromatic light with change in its angle of incidence on the retina would be explained if the cones in each group possessed different directional sensitivities. This implies that if the cones belonging to each group could be stimulated by themselves and the variation of apparent brightness determined, we should obtain different values of the coefficient p for the three groups. Knowing the spectral sensitivity curves of the three groups of cones it is possible to calculate by how much the coefficients p would have to differ to account for the observed colour change under different conditions. Using the spectral sensitivity curves for the cones obtained by Wright [8], it can be calculated that the observed change of hue of yellow light (590 $m\mu$) in passing from centre to periphery of the pupil would be accounted for if p for the "green" group of cones exceeded p for the "red" group by about 12 per cent. Similarly a difference of about 23 per cent between p for the "blue" and p for the "green" cones would give the observed hue change of blue-green light (490 $m\mu$).

To obtain agreement with existing data on the hue change it must be assumed that the percentage differences between the p values of the different types of cone vary with the wave-length. This is a complication which may call for some modification of the views outlined above. However, the conclusion that the different types of cone have different directional sensitivities is supported by evidence obtained by a completely different method which will now be considered.

Observations by the Threshold Method

A great deal can be learnt about the sensitivity of the retina by measurements of the smallest quantity of light which the eye can perceive under different conditions. In some recent work by the writer, the test light was a uniformly bright patch of 1° in diameter, exposed at intervals for a fixed period of about 1/20th sec. After each exposure the subject recorded whether he had seen the test patch. The brightness of the patch when there was a 50 per cent chance of seeing it was taken as the threshold value. Measurements were made with the subject looking either at the test patch (foveal vision) or at a point a certain distance to the side of the test patch (extrafoveal vision).

The light from the test patch could be sent into the eye through different points of the pupil and the corresponding variation of

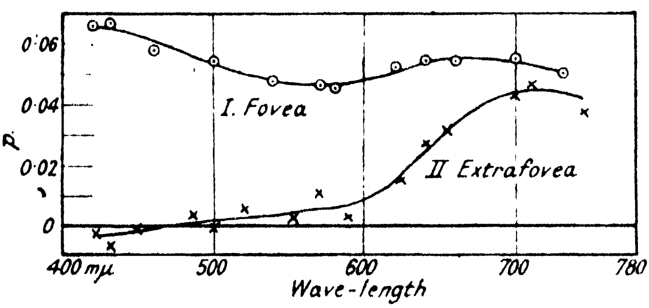

Fig. 5.

the threshold determined [9, 10]. We shall consider first the results obtained when the eye is dark-adapted and the field of view is completely dark except for the flashes of the test patch. If the test patch is imaged on the fovea, the threshold value increases as the point of entry shifts from centre to periphery of the pupil, in precisely the way we should expect from the observations on apparent brightness. The relative luminous efficiency can be obtained from the threshold measurements by putting η equal to the ratio T_0/T_p where T_0 and T_p are the threshold values for entry through the centre and through any other point P of the pupil. The η curves obtained in this way can be represented by the empirical formula $\eta = \eta_m 10^{-pr^2}$ used previously and Curve I of Fig. 5 shows the variation of p with the wave-length of the test patch. Comparing Fig. 5 with Fig. 4(a) it is clear that the absolute values of p are not very different in the two cases and the variations with

wave-length are similar. Thus for foveal vision the threshold observations merely confirm the measurements by brightness matching.

Very different results are obtained when the test patch is imaged on extrafoveal points. For a test patch of white light or of monochromatic light other than orange or red, the threshold value is almost independent of the position of the point of entry of the light in the pupil. Curve II of Fig. 5 gives the values of p obtained when the subject looked 5° to the side of the test patch. For wave-lengths less than 600 $m\mu$, p is not very different from zero. p equal to zero would correspond to a constant value of the relative luminous efficiency η. Other measurements, with white light, show that typical extrafoveal results are obtained when the test patch lies more than about 3° to the side of the direction of vision (Crawford [9]).

It appears therefore that, except when stimulated with red light, the sensitivity of the dark-adapted extrafoveal retina is practically non-directional. Two kinds of end organ—rods and cones—are present in the extrafoveal retina and, according to the duplicity theory, vision at low illuminations is served by the rods which are more sensitive than the cones in the dark-adapted eye. Thus we may say that in dark-adaptation the foveal cones have a pronounced directional sensitivity, while the rods are almost non-directional. The relative spectral sensitivity curve of the rods has its maximum value at about 510 $m\mu$. The maximum of the corresponding curve for the cones occurs at about 555 $m\mu$ or possibly at a rather greater wave-length. In passing through the spectrum from the blue end, the difference between rod and cone sensitivities diminishes until in the red end the cones have a sensitivity which is of the same order as that of the rods. This view is amply borne out by comparisons of the threshold values at the fovea, where there are no rods, and at near extrafoveal points, where there are both rods and cones. It is possible therefore that the appearance of directional sensitivity when the wave-length of the test patch exceeds 600 $m\mu$ (see Curve II, Fig. 5) may be due to the extrafoveal cones taking over perception of the test patch from the rods. Whether this explanation of the rise in the red of Curve II of Fig. 5 is correct or whether the rods have the peculiarity of being directional only when stimulated with red light cannot be decided at present.

So far, we have considered the dark-adapted eye only. By making the subject view an extended area of given brightness and colour, the retina can be brought to different conditions of adaptation and threshold measurements can be made in the same way as before. The adapting brightness remains on while the measure-

ments are being made and the flash of the test patch is an additional stimulus.

Consider first the case of a test patch imaged on the extrafoveal

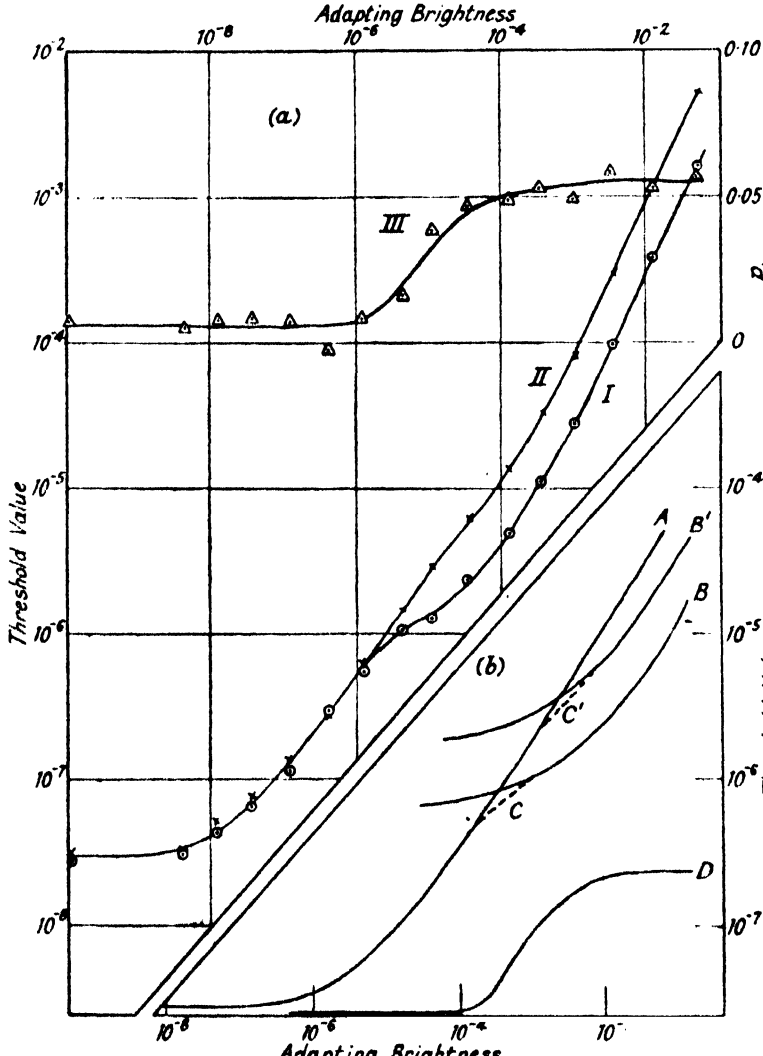

FIG. 6.

retina. Using light of wave-length 580 $m\mu$ both for the test patch and for the adapting field, Curves I and II of Fig. 6(a) show how the threshold value varies as the brightness of the adapting field is increased from zero. The light from the test patch entered

through the centre of the pupil for Curve I and through a point near the pupil edge for Curve II. The light from the adapting field entered through the centre of the pupil in both cases. Both the threshold value and the adapting brightness are expressed in energy units (ergs per sec. received by the eye from 1 square degree) and are plotted to logarithmic scales. The ratio of the threshold values for central and peripheral entry at any brightness level determines the magnitude of the directional sensitivity which is specified by the value of p (Curve III).

It is apparent from Curve III that up to a certain adapting brightness there is very little directional effect (p small). A transitional region follows and then at high brightnesses the directional sensitivity is approximately constant and has about the same value as that found for the fovea. The transitional region begins at an adapting brightness of about 10^{-6} energy units and at the same value the slope of Curve I begins to decrease. There is a point of inflection and finally, at high brightnesses, the slope of Curve I becomes constant. The explanation of the change of law of Curve I is provided by the duplicity theory. It is assumed that the rods acting alone would give a curve similar to Curve A of Fig. 6(b), while the cones acting alone would give the Curve B. When both rods and cones are in action the observed threshold will equal the lower of the separate thresholds. If the latter are equal or nearly so, the rods and cones may assist each other to some extent and the resultant threshold will be lower, as shown by the broken line C. The resultant curve built up in this way is assimilated to Curve I of Fig. 6(a), and there is little doubt, although the full reasoning cannot be given here, that the explanation based on the duplicity theory is substantially correct.

It is now easy to interpret the observed changes in the directional sensitivity. The rods are regarded as practically non-directional at all adapting brightnesses so that Curve A (Fig. 6(a)) applies equally for central and peripheral entry of the light. The cones are assumed to have a pronounced directional sensitivity whose value is independent of the adapting brightness. Thus the curve for peripheral entry, B′, is simply the curve for central entry, B, shifted upwards by a given amount. The resultant curve for peripheral entry is built up in the same way as before and from the difference of the resultant curves the variation of p (Curve D) is derived.

The above scheme proves equally useful in explaining the results obtained for other colours, including cases when test patch and adapting brightness have different wave-lengths. The example just

discussed refers to light of wave-length 580 $m\mu$. If the wave-length of the test patch is changed say to 500 $m\mu$, while the wave-length of the adapting brightness remains the same, the threshold curve of the rods (Curve A of Fig. 6(*b*)) moves bodily downwards because the rods are more sensitive to wave-length 500 $m\mu$ than to wave-length 580 $m\mu$ and the threshold value at any adapting brightness is diminished. At the same time the threshold curve of the cones (B) moves upwards because the cones are less sensitive to the new wave-length. Similarly, a change in the wave-length of the adapting brightness from 580 $m\mu$ to 500 $m\mu$, keeping the test patch wave-length constant, shifts the rod curve from right to left and the cone curve in the reverse direction. Experiment shows that the resultant threshold curves for different pairs of wave-lengths of test patch and adapting brightness can, in fact, be derived from rod and cone curves which shift about the diagram in the way just described. Moreover, the transition from the rod to the cone curve is accompanied by a characteristic increase in the directional sensitivity measured by the coefficient p.

Further evidence supporting the above views is provided by studies of the time course of dark adaptation. When the recovery of the eye from a high brightness is followed by measurements of the extrafoveal threshold for white or coloured light (other than red), the recovery curve falls into parts, an initial part attributable on the duplicity theory to the cones and a final part attributable to the rods (Hecht [11]). At the transition we should expect the directional sensitivity to drop from a high value characteristic of the cones to the very small value of the rods. Just such a change is observed experimentally (Crawford [9]).

Threshold Observations at the Fovea

There is a complication in the threshold measurements for extra-foveal vision, which has been ignored up to now. The threshold curve of the rods moves to different parts of the diagram of Fig. 6(*b*) without appreciable change of shape, but this is not true of the cone curve. The behaviour of the cone curve is, however, best studied by measurements of the foveal threshold which give the response of the cones unobscured by the rod response.

When the wave-lengths of test patch and adapting brightness are identical, the foveal threshold curve is a simple curve resembling Curve B of Fig. 6(*b*). The directional sensitivity has a high value appropriate to the cones and shows little if any systematic variation with wave-length. On the other hand, with a blue test patch and a green, yellow or red adapting brightness the threshold curve

falls into a low brightness and a high brightness range. This is well shown in Fig. 7, where Curves I and II refer to central and peripheral entry of the light from the test patch and Curve III gives the derived value of the directional sensitivity. In this case the wave-length of the test patch was 480 $m\mu$ and that of the adapting brightness 540 $m\mu$.

The form of foveal threshold curves like those of Fig. 7 is almost certainly to be explained with the aid of the trichromatic theory in the same way as the extrafoveal threshold curves are explained

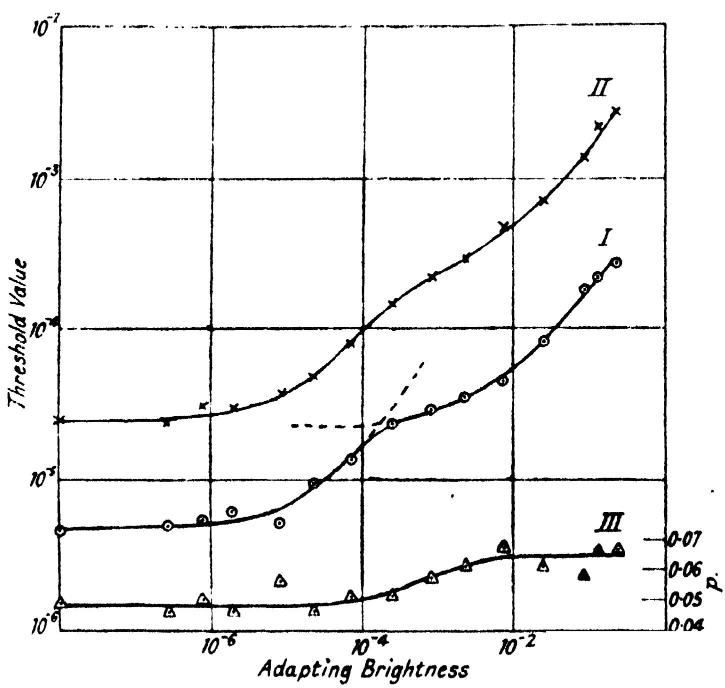

FIG. 7.

by the duplicity theory. The upper and lower parts of the threshold curve are attributed now to two types of cone. Each of these acting alone would, it is assumed, give a simple threshold curve as suggested by the broken line continuations of the two sections of Curve I in Fig. 7. By observing how the upper and lower sections of the curve shift in the diagram when the wave-lengths of test patch and adapting brightness are varied, approximate determinations can be made of the relative spectral sensitivities of the two types of cone which have been postulated. The maximum sensitivities occur at about 440 $m\mu$ and 540 $m\mu$ respectively so that we

may speak of these two types of cone as blue and green cones. The existence of a third type of cone—red cones—is also indicated by measurements of this kind although the separation of the green and red types by threshold measurements is difficult.

Reverting to Fig. 7, the variation in the directional sensitivity p may now be ascribed to a difference in the directional properties of the blue and green cones which are responsible for the upper or lower sections of the threshold curves. The direction and magnitude of the difference are about the same as those derived from the change in apparent colour described earlier on.

To sum up, the rods and the three types of cone each have their characteristic directional properties which must be accounted for in any acceptable explanation of the directional sensitivity of the retina. Pigment intrusion between the end-organs cannot explain all the effects observed without additional assumptions for which there is little justification. Differences in refractive index between the end organs and the surrounding medium, as suggested by Wright and Nelson, may provide the clue. The problem is certainly one whose solution should throw further light on the visual process.

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MOLECULES AND SURFACES

BEING AN APPRECIATION OF THE WORK OF IRVING LANGMUIR AND ITS INFLUENCE ON CHEMICAL THEORY

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British and American science are profoundly indebted to the Pilgrim Trust for the foundation of a series of lectures, which are to be delivered, under the joint auspices of the National Academy of Sciences and the Royal Society, in alternate years in London by an American, and in Washington, D.C., by a British man of science. The first of these lectures was given in London, on December 8, 1938, by Dr. Irving Langmuir of the General Electric Company's research department in Schenectady. As was natural, scientific bodies in this country were not slow to avail themselves of the exceptional opportunity of Dr. Langmuir's visit, so that, in less than a fortnight, London had the privilege of hearing him also deliver a Friday evening discourse at the Royal Institution, and the seventeenth Faraday lecture before the Chemical Society.

The choice of a lecturer was a most happy one. Dr. Langmuir's work and interests cover such a wide range, and have stimulated so much thought and research, both in pure and applied science, in this country as well as in America, that the theatre of the Royal Institution, where all three lectures were given, held audiences representative of a very large part of the progressive workers in physical, chemical, and biological sciences in these islands. They heard very lucid accounts of the research which has contributed so much, during the past twenty-five or thirty years, to our present detailed knowledge of the arrangement of molecules at surfaces, the nature and importance of the fields of force round organic molecules, and to the mechanism of thermionic emission.

A large part of his work has been concerned with surface phenomena ; indeed, almost all the foundation, and much of the detail also, in our present theories of the arrangement of atoms and molecules at surfaces is due to his consummate ability to combine the techniques of Physics and Chemistry in his attack on these

problems. The researches fall into two principal divisions: those on the reactions between simple gases and solid surfaces, principally metallic surfaces and those on the behaviour of molecules of organic compounds at solid and liquid surfaces. The earliest studies were on the reactions between heated metallic filaments, particularly tungsten, in gases at low pressures in electric lamp bulbs; and these have had extraordinarily far-reaching results both in the development of theory, and in practical applications. Starting from the observation that hydrogen causes an abnormally great loss of heat from a hot filament, too great to be accounted for by the thermal conductivity of ordinary gaseous hydrogen, it was found that the heated filament dissociates the hydrogen into atoms, a strongly endothermic process, and the free hydrogen atoms fly to the walls of the bulb. Eventually they re-combine, liberating the heat absorbed during dissociation, but the atomic hydrogen has a surprisingly long life and can perform all sorts of reactions impossible to molecular hydrogen.

Next, oxygen was studied. It was soon found to combine with the surface of the tungsten in an exceedingly stable layer, practically permanent at a temperature of 1,500° K., and not rapidly removed until considerably higher temperatures are applied. This layer prevented the dissociation of hydrogen (though above 1,500° K. it could be removed by hydrogen); and it also reduced the emission of electrons from the heated filament to a minute fraction of that from clean tungsten. Other work of this period (1909 to about 1916) dealt with the removal of traces of other gases by heated filaments, at low pressures, and the effect of gases on the life of the filaments.

The far-sighted direction of research, by Dr. W. R. Whitney in the firm's laboratories, towards a full understanding of the fate of all the *molecules* present in the lamp bulbs, rather than to a rapid empirical solution of the best conditions for obtaining long life of the filaments, soon bore fruit in the "half-watt" or "gas-filled" lamp. But it did much more for industry than merely produce a more efficient incandescent lamp. The firm had acquired an unrivalled knowledge of the properties of heated tungsten filaments, shortly to be put to use in the development of ever more efficient thermionic valves; the incidental discovery of a method of producing atomic hydrogen led at once to the invention of flames of atomic hydrogen, in which the hydrogen is dissociated in an arc, the atoms being carried to the surface of a metal requiring welding, so that the heat of recombination of the atoms is added, just where it is required, to the heat of combustion of the hydrogen. Another,

and more important, " by-product " of this research was the condensation pump for producing high vacua. The kinetic theory of gases had become, even at this early date, an everyday research tool in the hands of the firm's staff; it had been used originally for the purpose of calculating the rate at which molecules of gas hit the heated filaments, and it was but a step to apply it to the design of high-speed vacuum pumps operating by means of streams of mercury molecules, which collide with molecules of other gases present, propelling them to a pressure at which they can be removed by ordinary pumps.

It is worth pausing for a moment to consider how many industrial research laboratories would, even now with this example before them, permit part of their research staff to engage for several years continuously on work of such apparently abstruse academic character as the study of the properties of atomic hydrogen, and the precise fate of individual molecules moving in gases or condensed at surfaces. Yet the reward of this type of research, carried out thoroughly where the technical staff have the ability to apply its results, is far greater than that of the type of research ordinarily allowed in industry, and it has placed the American General Electric Company in a commanding technical position in their industry.

The principal advance in theory resulting from this work was the conception of the monatomic adsorbed layer of gases on metals, carbon, etc., in which the oxygen, or hydrogen, molecule is practically dissociated on the surface, each atom combining independently with the atoms of the underlying metal, by forces identical with those of primary valency. The surface forces of the metal are generally fully satisfied by the combination with this layer of gas, one atom thick. In the case of oxygen, however, where the adsorbed layer is extremely firmly held, J. K. Roberts has recently found that there are a few vacant spaces in the first layer, due to the fact that the pairs of atoms arrive at the surface at random, and do not fill up all the available vacant spaces, but leave a few single spaces unoccupied. These may be filled by molecules standing on end, one atom projecting above the first to form an incomplete second layer.

The reactivity of these " chemisorbed " monolayers differs very greatly from that of ordinary gases, and Langmuir laid the foundation of the modern studies on heterogeneous catalysis also, in this early work. Though he did not carry research very far into this field, the fate of oxygen molecules striking a surface of tungsten covered by a monatomic layer of oxygen was found to be very simple; they just combine with one surface tungsten atom, removing

it and its combined oxygen atom to form the volatile oxide WO_3. In 1921 there appeared studies on the catalysis of the combination of hydrogen and oxygen, and also of carbon monoxide and oxygen, on platinum surfaces, which showed clearly the importance of the adsorbed monatomic oxygen layer in the catalysis.

In more recent years the studies of atomic layers adsorbed on tungsten filaments have been continued with particular reference to the thermionic emission of electrons. Thorium, which may be present as an impurity in tungsten filaments, can make its way to the surface ; and when there, it enormously increases the electron emission. The alkali metals, of which cæsium has been most exhaustively studied, and barium themselves have a high emission and low work function, and they increase the emission of electrons from tungsten filaments to a very high value. Atoms of cæsium, striking a surface of heated tungsten, at once lose their electrons to the underlying tungsten metal, becoming monovalent positive ions themselves in the process ; if they evaporate, they do so only as positive ions ; but the electrostatic attraction, or "image" force, between the positive ions of cæsium and the tungsten metal are great enough to maintain a monatomic layer of cæsium on the surface of the metal up to fairly high temperatures, so that the emission of electrons from a cæsium-coated tungsten filament is greater by many powers of ten than that from a clean filament, and still more is it much greater than that from an oxygen-covered filament. Here, we have a clear case of an adsorbed layer being held by electrostatic forces.

The most powerful emitters of electrons are, however, surfaces of tungsten coated by a monatomic layer of cæsium on top of one of oxygen. The adhesion of the cæsium to the oxygen-coated tungsten is better than to the clean metallic tungsten, and the work function is less than that of the cæsium-coated tungsten. All this is in complete accord with what is known of the arrangement of the dipoles at the surface. Oxygen on tungsten has the electrons displaced so that the oxygen atom is negatively charged, the metal being positive ; the layer of dipoles so formed at the surface acts to retain the electrons in the metal, since the positive end of each dipole is inwards. Conversely, a cæsium ion at the surface is positive to the tungsten below, so that the dipoles at the surface have the positive end outwards and consequently help the electrons out.

The efficiency of the modern thermionic valve is in very large measure due to these researches ; but in fairness it should be said that other industrial research laboratories, notably that of the

General Electric Company in England, have also made very substantial contributions to the theory and practice of thermionic emission.

Searching, in 1916, for other natural phenomena akin to the monatomic adsorption of gases on metals, Langmuir found a fairly close parallel in the now well-known monomolecular films of organic substances on water. Fraülein Pockels, in 1891, showed how these films could be accurately studied with an exceedingly simple technique, and in 1899 Lord Rayleigh pointed out that their behaviour indicated that they were one molecule thick. Langmuir combined this view, that the films were one molecule thick, with the pictures of the probable shape and asymmetry of the molecules given by a literal interpretation of the organic constitutional formulæ, concluding that in most of the films which he studied the molecules are oriented perpendicularly, or nearly so, to the water, with that end of the molecule which, in simpler members of the same homologous series, confers solubility on the substance, directed towards the water. This conception, now a commonplace of chemical theory, was then regarded by many chemists as an almost impudent misuse of structural formulæ! There were some, even as late as 1920, who ridiculed the ideas of Langmuir and Harkins, saying that structural formulæ were never meant to be taken as literally as that! Yet the ideas have led to extraordinary developments in our knowledge of the properties of molecules and in understanding the theory of capillarity, besides helping to throw light on the constitution of some natural products. At that date, and indeed up till about 1923, the measurement of the cross-sectional area of molecules in these surface films was one of the best methods, sometimes the only one applicable, of ascertaining the size of the molecules; and some information as to shape was also given, especially when the molecules are enlarged near the end adhering to the water.

The adhesion of the end groups, in these films, to the water, runs very closely parallel with the solubilising properties of the same groups, in other organic molecules. The tendency of the long hydrocarbon chains to lie side by side is responsible for the peculiar structure of the crystals of long chain aliphatic organic compounds, which are built up essentially out of pairs of monomolecular layers, placed face to face with the water-soluble groups sandwiched between the hydrocarbon chains, which stand at a more or less steep angle to the plane of these layers. It was, indeed, possible in 1922 to make a very fair guess at the crystal structure of the long chain

aliphatic compounds, simply from consideration of the melting points of the esters of palmitic acid and other fatty acids, together with what the surface films revealed as to the forces between the different parts of the molecules, the principal forces being two, both of the Van der Waals type, a strong attraction between two water-soluble groups, or between a water-soluble group and water, and a less strong attraction between two hydrocarbon groups.

All the work on surface films, on solids as well as on gases, together with some information derived from the kinetic theory of gases, pointed to the attractive forces between molecules (apart from purely electrostatic "Coulomb" attractions between ions) being of very short range, not more than a very few Ångström units in range. Langmuir was thus led to his important "principle of independent surface action," which regards the forces at the surface of individual molecules as identical with the forces which give rise to surface tension in bulk liquids or solids. One can speak, without undue distortion of the meaning of the term, of the surface tension of a particular part of the surface of a molecule, meaning the surface energy associated with a given area of either the hydrocarbon, or the oxygenated part of a molecule such as a fatty acid. Since the surface forces extend but a short distance in comparison with the size even of one CH_2 group, the decrease in energy when the hydrocarbon chain of one molecule comes into contact, over its whole length, with the chain of another molecule, is the same as the decrease in total energy when two equal areas of a purely hydrocarbon liquid (a liquid paraffin) coalesce. Calculating the potential energy, in this way, from surface tension data, of long-chain fatty acids in various positions on the surface of water, Langmuir showed in 1925 that the position of minimum potential energy is with the chains packed closely side by side and the water-soluble ends of the molecules in the water, as experiment had already shown was the case. If the total lateral adhesion between the molecules was insufficient to keep them in contact against the disruptive effect of their thermal agitation, the position of minimum energy for an isolated molecule on a water surface is with the hydrocarbon chain lying flat, on the surface. This was in good accord with an earlier interpretation of "Traube's rule," namely that, in any homologous series, the addition of one CH_2 group to the molecule increases the tendency to be adsorbed from solution to such an extent that, in order to obtain equal lowering of surface tension, the concentration of the member of the series with n carbons must be three times that of the next higher member, with $n + 1$ carbons. Simple thermodynamical reasoning deduces from this rule that

the decrease in free energy, when a gramme molecule of the dissolved substance passes from the interior to the surface during adsorption, is about 600 calories for each CH_2 group, no matter how many such groups there may be in the molecule. Each CH_2 group having the same work of adsorption, each must be in the same position in relation to the surface as all the others; hence the chains in *isolated* molecules lie flat on the surface.

These monolayers on water surfaces are much the easiest type of surface film to investigate, and much more is known about the details of their structure than about other types of film. The principle of independent surface action, and the knowledge gained from these films as to the forces round molecules in their different parts, makes it possible to predict the structure of many other kinds of film, and also the surface structure of various organic compounds. Lubricating films on metals probably resemble monolayers on water fairly closely; so do the films of the various "collectors" used for rendering the surfaces of minerals hydrophobic, so that they are easily floated, like waxed needles on water, at the surface of air bubbles in the exceedingly important "froth-flotation" process used for the separation of minerals. It is now known that a layer only one molecule thick, of a substance containing at one end of its molecules a group with a definite affinity for the mineral surface (*e.g.* a xanthate group for slightly oxidised sulphide minerals, or a fatty acid for oxide minerals), as well as a hydrocarbon chain, orients itself on the mineral surface with the hydrocarbon chain outwards, thus giving to the surface an exterior layer of hydrocarbon groups, which (if more than a few methylene groups in thickness) makes the particle almost as difficult to wet as paraffin wax. The action of aluminium soaps in waterproofing textile fabrics is similar; the "business end" of the molecule affixes itself to the fibre, leaving the hydrocarbon end outwards, so that the surface is transformed from that of the strongly hydrophilic cellulose or wool, to one resembling paraffin wax, so far as the range of attraction of adhering water molecules is concerned.

Langmuir has not pursued his researches into biology, but there are already some important developments in this direction. Perhaps one of the most important is the growing realisation that in discovering the detailed molecular structure of these films, a beginning has been made in the science of the molecular anatomy of membranes and tissues. The importance of an understanding of constitutional organic chemistry is being brought home to the medical student as never before, because it is these organic molecules, with their orientation into membranes and micelles, which constitute

the bricks out of which the cell and its surfaces are built. We are now beginning to see how the fields of force round individual molecules actually do build up organised structures of some degree of complexity. In some recent studies of the effect of hæmolytic agents and snake venoms, on simple surface films, it is found that they penetrate and disrupt the cohesion of these films. Probably some such disruptive action on surface membranes is the cause of the destruction of the cells by these substances.

Protein molecules have been studied in surface films, in England and Holland as well as in America. They can exist, in solution, in a folded form with the hydrophilic groups outwards, or they can unfold on the surface, thus becoming, often, insoluble. On the surface, they probably form a network with many cross linkages. In one of his lectures, Langmuir compared the protein film on a water surface to a cloth, hanging here and there in festoons from the surface into the interior. It is very probable, from the work of E. N. Harvey and J. F. Danielli, that composite surfaces, consisting of fatty substances with protein adsorbed just below, are important constituents of cell membranes.

Knowledge gained from these films as to the fields of force round molecules is applicable to other problems besides those concerning surfaces directly. The peculiar distribution of attractive forces round molecules such as soaps, generically called, recently, the "paraffin chain salts," in which there is a long hydrocarbon chain with a very strongly water-attracting end group, this latter being electrolytically dissociated, results in the formation of those very interesting structures known as the "ionic micelles" in solutions of these substances. These are aggregates of the long chain ions, in which the hydrocarbon chains are packed together to form the interior (which according to G. S. Hartley appears to be a liquid), and the exterior is covered by the electrolytically dissociated, hydrophilic groups such as COONa, or SO_3H. These are one of the most interesting types of colloidal solution, and with the knowledge obtained from surface films, of the distribution of the field of force round the molecules of long chain fatty acids, the principal features of the structure of the ionic micelle, which had been studied earlier, by McBain, at once became clear.

It may seem strange to speak of the interior of a micelle, which has a diameter equal to twice the length of a long hydrocarbon chain, as a "liquid"; but in 1933 Langmuir showed that a rather puzzling type of monomolecular film on water has a chaotic arrangement in the upper four-fifths, or so, of the length of the molecules, which may properly be termed "liquid." For some years previ-

ously, these films had been known as "liquid expanded"; they have an area intermediate between that occupied by molecules standing upright on the surface, and by molecules lying flat, and Langmuir showed that the force with which they tend to spread over a water surface is closely similar to that exerted by a drop of a non-spreading oil on water, when there is dissolved in it a small amount of a substance (such as a fatty acid) which goes to the interface between the oil and the water, and forms an interfacial film there. Regarding the "liquid-expanded" monomolecular film as having an upper part, the hydrocarbon chains of the molecules, corresponding to the oil, and a lower end group, corresponding to the fatty acid in the interface between the oil and the water, the conclusion was drawn that the hydrocarbon chains are in constant chaotic motion, like the molecules in a liquid, subject only to the restraint that their lower ends are anchored to the water. Thus we have the amazing, yet very illuminating, conception of a liquid layer only some four-fifths of a molecule in thickness, and this is the only theory of these peculiar liquid-expanded films which appears anywhere near the truth.

One of Langmuir's latest contributions, made in conjunction with Miss Blodgett, is the discovery how to build up, on a solid surface, layers many molecules in thickness by simply dipping the prepared solid surface into water of suitably adjusted acidity, and removing it. A monomolecular film is maintained on the water surface, under constant compression by the use of a flexible thread behind which is a drop of oil possessing a definite tendency to spread. At each entrance to the water, part of the monolayer is transferred from the water to the solid surface; and under most conditions, at each removal from the water, another monolayer is transferred to the solid, the molecules this time being oriented the other way up. These "alternating" layers do not differ very much in structure from crystals of fatty acids, consisting of successive parallel leaflets of films, two molecules thick, the molecules in the two layers of the leaflet being oriented with their oxygenated groups pointing alternately towards, and away from, the solid surface.

These layers may be built up until they are several thousands of molecules thick, and their thickness measured accurately by optical means. In polarised light, they show very beautiful colour effects. The rather astonishing experiment, of measuring the thickness of built-up layers with an ordinary spherometer, is reasonably successful.

Under some conditions, layers are deposited only on the down-

ward path into the water, thus building up multiple films in which all the molecules point the same way. If a built-up film which has the outer layer of molecules oriented with the hydrocarbon end to the water is dipped into dilute thorium nitrate, the upper layer of molecules appears to be turned over, and the "conditioned" surface so obtained has considerable adsorptive powers for various organic substances. Proteins can be deposited in the form of built-up layers, as well as fatty acids and their bivalent salts.

There is a considerable degree of solidity in these films; it is possible, by adjustment of the acidity and of the amount of alkaline earth ions in the water, to build films containing barium or calcium stearate, and free fatty acid, in different proportions. By treatment with benzene, the free acid may be dissolved out of the films, changing the refractive index but not the thickness; if the film is now dipped in oil, the refractive index rises, as the oil penetrates into the spaces left where the free fatty acid was dissolved out. Thus a considerable proportion of the film can be removed without causing collapse of the whole structure.

Since about the year 1910, there has been a major revolution in chemistry. Those who received their chemical education in the years just before the war will remember that, although a certain amount of classical molecular theory was taught, there was little clear idea of the molecules as tangible individual objects. The counting of their number, and the measurement of their absolute dimensions, was probably regarded by most chemists as an amusing exercise for physicists who had nothing better to do; and few supposed that, even if we did know these magnitudes, the science of chemistry would be seriously affected. But now, in less than thirty years, not only the number and dimensions of molecules are well known, but also their shapes, the nature of their fields of force, their mechanical properties, and the way in which they build themselves up into crystals and other macroscopic formations are almost everyday matters; and even the nature and the strength of the "springs" which hold the atoms together in the molecule, and the importance of these interatomic forces for chemical reaction, are becoming understood. Many, of course, have contributed to the concentration of the research weapons of physics, chemistry, and mathematics, on the solution of these problems; but probably no single person has done more, in more different fields, than Dr. Langmuir; and industrial practice, no less than scientific theory, has made immense progress, as a direct result of the enlightened research policy of the firm of which he is so distinguished a member.

RECENT ADVANCES IN SCIENCE

ASTRONOMY. By A. HUNTER, Ph.D., F.R.A.S., Royal Observatory, Greenwich.

IN these days, when systematised research is the basis even of such a study as that of astronomy—a study which was once so personal a matter as to be described rather as an art than as a science—the former popular conception of the astronomer as an observer at the eyepiece of some great instrument, perpetually making wonderful discoveries, is apt to be somewhat wide of the mark. Major advances in astronomical knowledge are now, more often than not, the product of co-operation between dozens of observers at several observatories, and are made possible only by the laborious measurement of thousands of photographs secured over a period of many years. The emotions of the

> . . . . watcher of the skies,
> When a new planet swims into his ken

tend nowadays, therefore, to be experienced in a somewhat more attenuated form than perhaps Keats imagined. Occasionally, however, the conditions of the poem are fulfilled more or less literally, and an isolated astronomer reaches the headlines (at least in the scientific press) as a discoverer in the old-fashioned sense.

The past few years have been particularly rich in such discoveries in the solar system: the present report deals with some especially interesting minor planets, and two new planetary satellites.

THE INNER MINOR PLANETS.—It will be recollected that there exists a wide gap in the solar system between the orbits of Mars and Jupiter, where by Bode's law (and indeed by casual inspection of a scale diagram) a planet might be expected. In this gap revolve most of the asteroids, or minor planets, of which more than 1400 are now known, *i.e.* have had reliable orbits calculated for them. Hundreds more have been observed less extensively. Their mean distance corresponds closely to that predicted by Bode's law. Even the biggest is less than 500 miles in diameter, and is never visible to the naked eye; whilst the smallest known are believed to be a

mile or so across—mere travelling mountains—and require large apertures for their detection. These fainter objects are now being discovered at the rate of scores per annum by examining for trailed images long-exposure photographs, carefully guided on the stars in regions near the ecliptic. A trail is, of course, due to the asteroid's apparent motion amongst the stars during the two or three hours of exposure. An alternative method, which employs more economically what little light there is available from the fainter minor planets, consists of the inverse process—guiding the telescope not on the stars, but on a fictitious object of about the expected angular speed, and then searching the resulting photograph for small dots or short trails amongst the long trailed images of the stars. The interest of the work lies partly in the thrill of it (the study is a favourite one for amateurs), and partly in the bearing of the information obtained on theories of the solar system. The larger established observatories, however, usually take up the study for a more immediate purpose: that of determining the radius of the earth's orbit, the fundamental unit of astronomical distance. The method depends on this principle: a scale model of the solar system having been made from observations of the angular motions of the planets and the application of Kepler's laws, it is necessary to measure absolutely only one dimension in order to deduce all the rest, including the required radius of the earth's orbit. The asteroid Eros has proved most suitable for parallax measurements of this sort, since its orbit has such a high eccentricity that in favourable circumstances, *i.e.* when perihelion passage occurs at opposition, the asteroid may approach within 14×10^6 miles of the earth. Such an extreme case has not occurred since the discovery of Eros in 1898, but in 1901 the nearest approach was 30×10^6 miles, and in 1931 just over 16×10^6 miles. At both oppositions, many of the observatories of the world co-operated in making extensive series of observations: from the first was deduced a value for the solar parallax with a probable error of $\pm$ 1 part in 3000; from the second it is hoped that a still more accurate result will emerge when the extremely complicated reductions are completed in a few months' time.

Since this latest favourable opposition of Eros, the discovery of minor planets which may approach the earth still more closely has proceeded apace. An asteroid, since named Amor, was found in 1932 to have an orbit whose eccentricity is twice that of Eros, and whose closest approach in January 1932 brought it within 10^7 miles of the earth. The eccentricity of the orbit of Apollo, discovered in the same year, is 2·5 times that of Eros; and since

it has a comparable semi-axis, this orbit passes not merely within the Earth's orbit, but actually within that of Venus. It is fortunate that its inclination to the ecliptic prevents a nearer approach than 3×10^6 miles. The orbit of Adonis, discovered in 1936, also crosses those of the Earth and Venus, and in that year its least distance from the earth was only about a million miles.

The latest discovery is due to Dr. K. Reinmuth, who found, on each of two plates exposed at Heidelberg for two hours on the night of October 28, 1937, a trail whose length and density suggested that the object photographed was of about the 10th magnitude and was travelling at some 50 times the average angular speed of an ordinary asteroid. This remarkable body was provisionally called 1937 UB in accordance with the usual notation, but has since been named Hermes by its discoverer. Plates exposed on the next night and the one after, on the positions predicted for the object by extrapolating in both directions (the actual direction of motion not being known), failed to show any trace of the asteroid. This unexpected result was explained afterwards when the orbit was calculated from other observations: the angular motion had nearly trebled itself in 24 hours. Images were, however, subsequently detected on short-focus plates taken at Sonneberg on October 26, 27, 28 and 29. The positions given by these plates are unfortunately somewhat uncertain, on account of the small scale and the fact that on none of them was the asteroid even approximately centred; but they served to identify it on an October 25 plate taken at Oak Ridge, Massachusetts.

Preliminary computations from the European observations alone showed that the weight of the parallax determination was low, but later workers were able to combine the northern positions with those found from two Johannesburg plates, taken for another purpose on October 27. It was then found that the object's closest approach to the earth must have occurred on October 30. That no observations were secured at this time is hardly surprising: not only was the motion unpredicted at that time, but also the angular speed was so great that the asteroid swept more than a quarter of the way across the sky in 24 hours. At this unprecedented rate, even though the object probably nearly reached naked-eye brightness, photography would naturally be difficult except with big apertures. And once lost, of course, the asteroid was never reobserved: searching for a needle in a haystack would be simplicity itself compared with finding such an object, rapidly growing fainter as it receded, in an unknown part of the sky.

For the computation of an orbit, then, we have only five nights'

positions, two of which depend on off-axis images on small-scale plates. Finally, to add to the difficulties of the computers, the approach was so close that enormous perturbations by the earth's gravitational field are to be expected. Nevertheless, fair agreement is found among the various orbits published. Crommelin (*Journ. Brit. Astron. Assoc.*, **48**, 161, Feb. 1938) finds that the least distance from the earth at October 30·5 was about 485,000 miles. Hermes therefore approached us more closely than does any other celestial body except the Moon. The least distance between the orbits, *i.e.* the closest approach possible, is only 220,000 miles, actually less than the mean distance of the Moon. It is, however, most unlikely that the asteroid will return again in the same orbit —it is, in fact, doubtful whether its next approach, predicted for 1940 from the somewhat uncertain orbital period, will be observed at all. Improvement in the orbital elements will therefore probably depend on the finding of pre-discovery images on plates taken for other purposes.

The diameters of these tiny earth-grazing planets, unlike those of the larger asteroids, cannot be found from direct micrometer measurements, since their angular subtense is so small. They can, however, be inferred by using the known parallaxes and apparent magnitudes of the bodies, and assuming a plausible albedo, or reflecting power. In this way, a hypothetical diameter of a little less than a mile has been found for Hermes. In spite of its close approach, then, it will be useless for determination of the scale of the solar system because of its faintness : one appearance is not enough to fix the orbit with sufficient accuracy, and a second, as we have seen, is unlikely. Objects of this size are, however, of interest, if not of much significance, in decreasing still further the gap between the sizes of the minor planets and those of meteors. Astronomers of the future may yet find themselves defining a minor planet as a body of meteoric type observed outside the earth's atmosphere by reflected sunlight ; or a meteorite as a minor planet which has collided with the earth !

The real importance of these recent discoveries, however, lies in the way in which they have revolutionised our conception of the extent of the solar system over which minor planets are to be found. Until 1898 the asteroid zone was rigidly defined by the orbits of Mars and Jupiter ; in the last few years the inner limit has been pushed to Mercury, and the outer to Saturn. Before 1898 the nearest known approach of a minor planet to the earth still left them separated by some 50 millions of miles ; the discovery of Eros (1898) reduced this figure to 14, that of Amor (1932) to 10,

that of Apollo (1932) to 3, that of Adonis (1936) to 1¼, and that of Hermes (1937) to ¼.

It may also be pointed out that this extension of the minor planet zone has technically, at least, rendered unreal the distinction between superior and inferior planets. Apollo, Adonis and Hermes have properties pertaining to both classes: they are at times in inferior conjunction like an inferior planet, and at times in opposition or in quadrature, like a superior planet.

Jupiter X and XI.—The satellite system of Jupiter is as remarkable as the history of its exploration is interesting. The first four moons discovered by Galileo in 1610, with the newly-invented telescope, are bodies two or three thousand miles in diameter, and of the fifth or sixth stellar magnitude, which revolve at distances from the planet varying from 3 to 13 Jovian diameters. These major satellites are the only ones discernible with a small telescope, and are the bodies used by Roemer in his famous determination of the velocity of light. In the period 1892–1914 five others were discovered: No. V, only just over a diameter away from the parent body,[1] is a 13th magnitude object; Nos. VI and VII, 80 diameters away, are respectively 14th and 16th magnitude; No. VIII, 165 diameters away, is also 16th magnitude; No. IX is a little further out than VIII, but is only 19th magnitude. The diameters of these minor satellites are a little uncertain, but probably range between about 100 miles for V to 20 miles for IX. Satellites VIII and IX are of interest in that, like Phœbe, the outermost satellite of Saturn, and the satellites of Uranus and Neptune, their orbital motion is retrograde.

Two new satellites of Jupiter were discovered in July last by Dr. S. Nicholson on photographs taken with the 100-inch reflector at Mt. Wilson. Like Jupiter IX, also discovered by Nicholson, these bodies are so small that they are of only about the 19th magnitude. Jupiter X is just over 80 diameters from the planet, and its motion was at first erroneously thought to be retrograde. The retrograde motion of Jupiter XI, which is nearly 160 diameters away, has however been confirmed by the later observations.

The table on page 705 gives some particulars of the satellite system as we now know it, the order being of increasing distance from the planet. It will be seen that each of the new satellites belongs to one of the two sharply-defined groups into which the outer bodies are classed by their distances and their directions of motion.

The discoveries have brought up the number of known planetary

[1] Planetary satellites are given serial numbers in order of discovery, not of distance from the primary.

Satellite.	Mean Distance (10^6 mls.).	Period (days).	Motion.*	Diameter (miles).	Discovery.
V	0·11	0·50	D	~100	Barnard, 1892
I	0·26	1·77	D	2460	Galileo, 1610
II	0·42	3·55	D	2000	do.
III	0·66	7·16	D	3540	do.
IV	1·17	16·69	D	3350	do.
VI	7·11	250·6	D	~100	Perrine, 1904
VII	7·29	260·1	D	~40	do. 1905
X	7·31	260·5	D	~15	Nicholson, 1938
XI	14·0	693	R	~15	do.
VIII	14·6	739	R	~30	Melotte, 1908
IX	15·0	745	R	~20	Nicholson, 1914

* D = direct, R = retrograde.

satellites in the solar system to 28, of which 9 are of retrograde motion. It is noticeable that in the planetary systems comprising a large family of satellites (Jupiter and Saturn), the motion of the outer members is retrograde. This fact brings into prominence the whole question of the origin of these outer satellites. Solar perturbations are, of course, enormous at such distances: it has, in fact, been calculated that if Jupiter VIII should ever, at conjunction or opposition, get twice as far from its primary as it did in 1913—a supposition by no means wildly improbable—it would be captured by the Sun, and become an independent minor planet. Now we know (see the preceding section) that the asteroid orbits extend beyond Jupiter's; so it is equally conceivable that the reverse might occur, and a minor planet be captured by the combined attractions of the Sun and Jupiter to become a planetary satellite. Phœbe (Saturn IX) is in rather a different case. Assuming a circular orbit for Saturn and a negligible mass for Phœbe, it can be shown that the satellite cannot in present circumstances escape from the system. The factor of safety is such that correcting for the inaccuracy of the initial assumptions will not alter this conclusion. Conversely, then, the body has presumably not been captured relatively recently.

Stability is, as a matter of fact, helped by retrograde motion, and it can be shown that a satellite as far out as Jupiter XI, VIII, or IX will probably be unstable if the motion is direct, but not if it is retrograde. It may well be, then, that there is a continuous process of exchange going on; a sort of friendly rivalry between the Sun and Jupiter for these tiny secondaries, with the planet winning only if it can enlist a little fatherly help from the Sun, and losing even then if the resulting motion of the newly-captured

satellite is direct. The diameters of the outer satellites (see table above) are quite in accordance with this view.

It is to be hoped that amongst the many observational programmes with which the big American telescopes have to deal, time will be found to follow occasionally these outer satellites of Jupiter. Apart altogether from their value in giving a precise determination of the planet's mass, such observations may help to decide one of the fascinating problems outstanding in the solar system.

PHYSICS. By F. A. Vick, Ph.D., University College, London.

Secondary Electron Emission.—If the surface of any solid is bombarded with electrons whose speed is above a certain value, electrons in the solid may receive enough energy to emerge from the surface. These "secondary electrons" may be captured by other surfaces if the potential distribution is favourable, or they may return to the solid. The number of secondary electrons liberated is sometimes much greater and sometimes much less than the number of primary electrons striking the surface, depending upon the nature of the surface and the direction and velocity of the primary electrons. In all "vacuum tubes" in which electrons strike surfaces secondary emission is likely to occur, and in most cases it is a disturbing factor, for example in wireless valves. Sometimes, however, secondary emission is useful, as in the escape of electrons from the fluorescent screen of a cathode-ray oscillograph. Recently, too, high secondary emission has been applied to the amplification of small currents (in the "secondary electron multiplier"). It is thus important to be able to prepare surfaces emitting very few electrons, and also surfaces emitting as many as possible. For this to be done efficiently, it is necessary to know something about the mechanism of secondary emission and the relative importance of the various factors involved.

Consider first the energy distribution of secondary electrons liberated when a beam of primary electrons of definite energy strikes a target at a given angle of incidence (perpendicular to the surface, for simplicity). One type of apparatus employed to determine this distribution is illustrated in Fig. 1. The various electrodes, probably of molybdenum, are mounted in an evacuated glass enclosure and can be cleaned and baked in the usual way. The primary beam is formed by thermionic emission from cathode K (which may be a tungsten or nickel surface coated with alkaline earth oxides, indirectly heated by filament F), accelerated by the positive potential of A with respect to K. The potential of the cylinder C is adjusted

until the beam is narrow. All electrons liberated and scattered by the target T are collected by S if its potential is positive with respect to T. The dimensions of T are small enough for the field in its neighbourhood to be approximately radial. By magnetic attraction on a piece of soft iron attached to the rod B, the target can be withdrawn from the sphere in order that it may be cleaned by strong heating, or to allow layers of other material to be deposited on its surface.

If the potential of S is made V volts negative with respect to T, only those electrons leaving T with energy equal to or greater than eV will reach S (e being the charge of an electron). The current flowing between T and S is a measure of this number. The net current between K and T represents the difference between the number of primary electrons reaching T per sec. and the number leaving. If, on the average, each primary electron liberates more than one secondary, the net current will be opposite in sign to the primary current itself. Multiple reflection of electrons between target and sphere is negligible. Often the current, I_s, between T

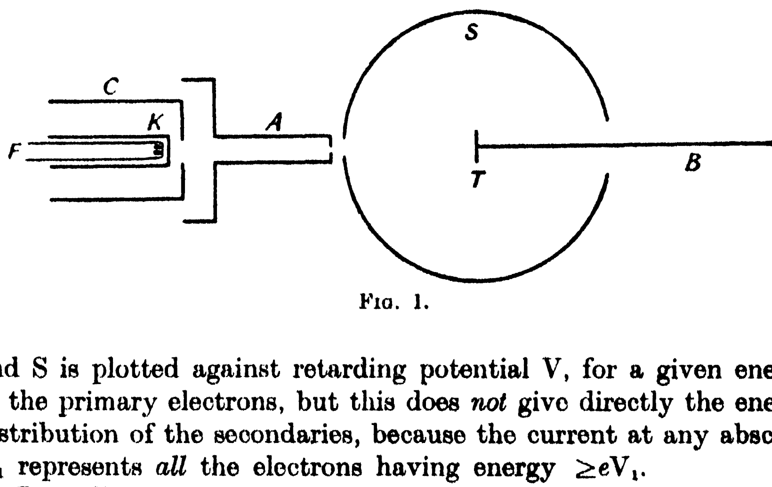

Fig. 1.

and S is plotted against retarding potential V, for a given energy of the primary electrons, but this does *not* give directly the energy distribution of the secondaries, because the current at any abscissa V_1 represents *all* the electrons having energy $\geq eV_1$.

Let dN be the number of electrons leaving T with energies between E and $E + dE$. $N = f(E)$ and $E = eV$. Then

$$I_s = c.\int_{E_1}^{\infty} f(E)dE \text{ and } \frac{dI_s}{dE} = c.f.(E)$$

$$\left(\frac{dI_s}{dE}\right)_{E_1} = c.N_1.$$

where c is a constant.

Therefore the shape of the energy distribution curve (which shows

the number of electrons leaving T at each energy eV) is obtained by plotting dI_s/dV against V.

The type of apparatus represented by Fig. 1 has been used by, among others, H. E. Farnsworth (*Phys. Rev.*, **25**, 41, 1925), H. Bruining and J. H. de Boer (*Physica*, **5**, 17, 1938). The method is not really suitable for very low values of V, that is for low energy secondaries, because the space charge of electrons round the target then results in a potential minimum between target and sphere. (*Cf.* Reimann, *Thermionic Emission* (1934), p. 43.)

In another method the secondary electrons pursue with velocity v a semicircular path of radius r in a uniform magnetic field strength H, according to the well-known relation

$$v = \mathrm{H}.\frac{e}{m}.r.$$

With constant H, a "magnetic spectrum" is formed on a photographic plate, from the blackening of which the velocity distribution can be determined. Alternatively, r is defined by a series of slits and the electrons are collected by a Faraday cage. The current to the cage at various values of H is measured. From the velocity distribution so obtained, the energy distribution is derived by dividing the ordinates by v and multiplying the abscissæ by v, since

$$\mathrm{E} = \tfrac{1}{2}mv^2,\ d\mathrm{E} = mv.dv$$

and if dn is the number with velocities between v and $v + dv$, $d\mathrm{N}$ is the number with energies between E and E + dE,

$$\frac{d\mathrm{N}}{d\mathrm{E}} = \frac{1}{mv}.\frac{dn}{dv}$$

(*cf.* Kollath, *Ann. der Physik*, **27**, 721, 1936). This energy distribution curve is for electrons leaving the target in the direction of the first slit, and to get the total distribution the process must be repeated for various orientations of the target. A good example of the use of the magnetic method is provided by L. J. Haworth (*Phys. Rev.*, **48**, 88, 1935).

An examination of typical energy distribution curves (*e.g.* Rudberg, *Phys. Rev.*, **50**, 138, 1936; Haworth, *loc. cit.*) shows the existence of two well-marked maxima. The first is at a low energy, between 2 and 15 volts, the distribution about it being approximately Maxwellian. The position of the second corresponds exactly to the energy of the primary beam in each case, and therefore represents primary electrons reflected without loss of energy. The position of the first maximum varies very little with change of primary energy over a wide range. The electrons included in this

region are not only secondaries (which, strictly, are those which were not in the primary beam but liberated from the solid itself), but also primaries which have been inelastically scattered from the solid, probably after having penetrated several atomic layers. In many cases, however, the number of scattered primaries is small compared with the number of secondaries liberated.

Of great importance is the variation with primary energy of the total number of secondaries, δ, liberated on an average by one primary, and its dependence on the nature of the surface bombarded. The apparatus of Fig. 1 can be used, the potential of the sphere, S, being maintained at a few volts (*e.g.* 10) positive with respect to

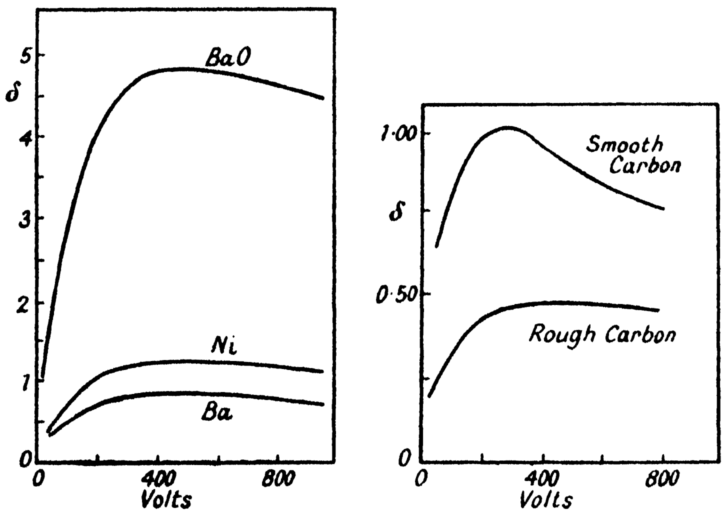

FIG. 2.

the target, while the potential difference between K and T is varied to give various primary energies. A few representative results are given in Fig. 2 (reproduced from Bruining, *Philips Tech. Rev.*, **3**, 80, 1938). It will be seen that δ reaches a maximum at primary energies corresponding to 200–600 volts. The occurrence of this maximum can be explained in the following general way. As the primary energy increases, more electrons in the solid are given sufficient energy to permit them to escape if their directions of motion near the surface are favourable. But at high velocities the primary electrons will penetrate further into the solid and secondary electrons freed from these depths will have more chance of being scattered and losing their energy before reaching the surface. Bruining has estimated the depth at which secondaries are freed in Ni,

with 500 volts primary energy, to be about 20 atomic layers (*Physica*, **3**, 1046, 1936). There is still a certain amount of disagreement over this figure.

It should be emphasised at this stage that thin films of oxide, adsorbed gas layers, grease, etc., on the surface of the target have a great influence on δ. Cleaning a metal surface by strong heating in a high vacuum nearly always reduces δ considerably, and until this is done the results will not be characteristic of the metal. Some curves illustrating the effect of outgassing are given by Farnsworth (*Phys. Rev.*, **20**, 358, 1922). Haworth (*Phys. Rev.*, **48**, 88, 1935) kept his target hot while taking measurements. If this procedure is adopted it may, of course, be necessary to correct for thermionic emission from the target (which merely adds on to the secondary emission). δ seems to be independent of the temperature of the target (Hyatt and Smith, *Phys. Rev.*, **32**, 929, 1928; Sixtus, *Ann. der Physik*, **3**, 1017, 1929; Treloar and Landon, *Proc. Phys. Soc.*, **50**, 625, Sept. 1938). Pure metals have quite low values of δ. Table I (adapted from Bruining, *Philips Tech. Rev.*, **3**, 80, 1938) shows some recent values, for a primary energy of 150 volts.

TABLE I

Metal.	δ	Work Function (volts).
Cs	0·55	1·81
Ba	0·63	2·11
Al	0·86	2·26
Li	0·45–0·55	2·28
Mg	0·90	2·42
Be	0·52	3·16
Mo	1·00	4·15
Cu	0·90	4·30
W	0·75	4·52
Fe	0·97	4·77
Ni	0·94	5·03

These values are lower than those reported for the same metals by earlier workers, for the reasons indicated.

Values of δ for oxidised or contaminated surfaces are given in Table II (Bruining, *loc. cit.*), for the same primary energy.

TABLE II

Surface.	δ
Li, sublimed in a poor vacuum	3·25
Cs, oxidised in dry oxygen	3–4
MgO	2–6
BaO	*ca* 3
Al + a thin layer of oxide	2·1

A glance at Table I will show that work function is not the only

factor upon which δ depends. This we would expect, because, for a given primary energy, δ will depend on (*a*) the number and energy of secondary electrons liberated in the material, and (*b*) the height of the potential barrier at the surface. (*a*) will depend upon factors analogous to ionisation potential in a gas, and (*b*) upon the work function. (Some factors influencing δ are discussed by Bruining in a series of papers commencing in the 1938 volume of *Physica*.)

For a given metal, the work function can be altered by the deposition on its surface of monomolecular layers of the alkali metals (*e.g.* Ba, Th). Experiments performed by Copeland (*Phys. Rev.*, **48**, 98, 1935) and Treloar (*Proc. Phys. Soc.*, **49**, 392, 1937) indicate that so long as the secondary electrons originate in the underlying metal and not in the adsorbed layer, δ increases as the number of adsorbed atoms rises from zero to a full monatomic layer, that is as the work function falls. The question is much more complicated when the electrons are liberated from the layer itself. We have already seen that an oxide layer increases δ considerably. It is therefore the oxide-coated surface or complex layers like Cs on Cs_2O on Ag (Penning and Kruithof, *Physica*, **2**, 793, 1935) which we shall use if we desire a high δ. In most practical applications it is necessary for δ to be constant, and many complex layers are unsuitable in this respect. MgO is one of the most stable surfaces with a reasonably high δ. For the application of such surfaces to the secondary emission multiplier see Zworykin, Morton and Malter, *Proc. Inst. Rad. Eng.*, **24**, 351, 1936.

Up to now we have considered surfaces which are reasonably smooth (with the possible exception of the oxides). If, however, the surface is rough or covered with particles of carbon or metal, secondary electrons leaving the solid proper may yet be caught in the "labyrinth." This will have the effect of reducing δ. It is for this reason that the anodes of many valves are coated with a black deposit of sputtered metal or of carbon (at the same time this process makes for better radiation of heat). Curves in Fig. 2 show the difference in δ for "rough" and "smooth" carbon. Complications may arise if during use Ba or Sr is evaporated from the oxide-coated cathode on to the anode. The effect of this is discussed by H. Bruining, J. H. de Boer and W. G. Burgers (*Physica*, **4**, 267, 1937).

Furthermore, δ at constant primary energy rises as the field tending to draw electrons from the surface is increased. According to Table I, Li, Be, or Cs would be good surfaces to use where secondary emission is not desired, but in practice there is usually sufficient residual gas to oxidise these surfaces slightly, and these increase δ as Fig. 2 shows.

This article is intended to set down main principles for the general reader, and cannot be comprehensive in the space available. No mention has been made of angular distribution of secondary electrons, or fine structure of the curves or detailed theory. An extensive bibliography up to December 1936 will be found at the end of a paper by Kollath (*Phys. Zeits.*, **38**, 202, 1937).

METEOROLOGY. By E. V. Newnham, B.Sc., Meteorological Office, London.

Katabatic Winds.—The severe frosts of the middle of May 1935, which over wide areas completely destroyed the foliage of many kinds of deciduous tree, did so much damage to the blossom of fruit trees that an impetus was given to the study of late spring frosts. At the East Malling Agricultural Research Station a programme of research work was drawn up which was to begin with the study of katabatic winds, *i.e.* with those winds that arise owing to the cooling of the ground by radiation under a clear sky at night. This cooling is communicated to the air nearest to the ground and results in that air becoming denser. When this happens on hilly ground a difference of density is set up between the air on the sloping hillside and the " free " atmosphere at the same level over lower ground, a state of affairs favourable for atmospheric connection. According to the idealised conception favoured by some text-books of meteorology the result is simply a downward flow of air in the valleys and an accumulation of the chilled air in the valley bottoms. This conception appears to be supported by the frequency with which the temperature reached during a clear and nearly calm night is lower in the valleys than on the hills. The study of katabatic winds initiated at East Malling arose from the fact that matters are not quite so simple as the above explanation implies. Fruit growers are familiar with the existence of " frost pockets," *i.e.* hollows especially liable to spring frost and which are therefore bad places for the planting of fruit trees, but have also often observed the damage on a particular night to be very variable over a short distance, *e.g.* all the trees in one orchard may be damaged while those in another at about the same level may be untouched. All who have studied the subject have been forced to recognise that it is not nearly so simple a subject as it appears to be at first sight. In the Quarterly Journal of the Royal Meteorological Society for October 1938, a summary of the work carried out at East Malling so far is given in a paper by C. E. Cornford entitled " Katabatic Winds and the Prevention of Frost Damage."

Before describing the results obtained it will be as well to say a little about the instrumental difficulties encountered and how they have been met. At first sight the measurement of air temperature may seem rather more easy than it usually is, seeing that the observations are made between sunset and sunrise and the thermometers therefore do not have to be protected against solar radiation. Actually this is not the case. If we were only concerned with cloudy and windy weather it would be possible to get a reasonably close approximation to air temperature by simply suspending the bare thermometer wherever a reading is required, but on a calm and nearly cloudless night the bulb of the thermometer quickly loses heat by radiation whereas the surrounding air does not. The result is that a freely exposed thermometer bulb becomes colder than the surrounding air. The difference in temperature, moreover, does not tend to a steady value, but is affected by every small change of the wind, even though the latter may be always so light as to come within the meteorologists' definition of a " calm " (*i.e.* less than 1 m.p.h.). The difference becomes large when practically all air movement ceases and at once diminishes when a slight draught brings new air into contact with the thermometer bulb. To meet this difficulty the bulbs of all the thermometers were fitted with cylindrical metal shields separated by a small annular air space and provided with small ventilation holes to allow air to pass between the thermometer bulb and the inner shield and between the two shields. The arrangement is virtually that of the Assmann psychrometer, but with a natural draught instead of a forced draught and is therefore dependent for its success on there being always some air movement, so that the air between the two shields is carried away before it has had time to be appreciably chilled by contact with the outer shield (which will have lost heat by radiation). Under these conditions the bulb of the thermometer is surrounded by a screen which is at a temperature very close to that of the free air, and the thermometer will show true air temperature. As no figures are given to show to what extent the arrangement proves successful, there remains some uncertainty as to whether the readings on the calmest occasions may not be a little too low.

For the measurement of wind a special form of " swinging plate " anemometer was devised that would be effective in very light winds (0–5 m.p.h.). It consisted of two light silk " plates " with their axes at right angles, the swinging movement of these plates, due to wind pressure, being transformed by a system of levers into up and down movements of a pen recording on a revolv-

ing drum. The plates were so set that one pen recorded the south to north and the other the west to east component of the wind, but as the anemometer was not compared with a standard the values of the velocity scale were not accurately known and could only be used for comparative purposes. The instrument was used in conjunction with a thermograph that recorded temperature on the same drum. Observations were made at a number of different places some planted with fruit trees and others uncultivated, and in the former case some observations were made of the effect of orchard heating.

Observations of temperature with orchard heaters in operation were made on five nights in the spring of 1936, with a view to finding suitable methods for estimating the effectiveness of the heaters. Two methods were employed :

(1) Minimum thermometers with radiation shields were placed on a mast at heights of 3 in., 10 ft. and 20 ft. in the middle of the heated area and 200 yd. away in the same orchard, and the recorded temperatures were compared.

(2) Comparisons were made between the readings of whirled unshielded thermometers at a height of 3 ft. at the middle or the leeward edge of the heated area and those of a similar thermometer on the windward edge of the heated area (whirled thermometers may be expected to give an even closer approximation to true air temperature than shielded thermometers).

The results obtained were rather disappointing. The heated area was only 1 acre in extent and contained 60 Harrington heaters, uniformly distributed. It appears that when the heating is applied to such a small area the effect is small and only detectable after various precautions have been taken. Thus, the temperature is so much affected by the nature of the covering of the ground that any difference observed may be due much more to that cause than to the heating. Not all the observed anomalies were, however, explained, *e.g.* why on the night of April 22–3 it was 1·5° F. colder at a height of 20 ft. in the heated area than at the same height outside.

Observations of katabatic winds were also made in places specially chosen so that the meteorological effects might be most marked. The first place selected was Bluebell Hill on the North Downs in Kent, with a gradient of 1 in 3. In a south-facing meadow on the hilltop the wind was from south and had a speed between 5 and 10 m.p.h., although the air was calm not far away in the Medway valley. Proceeding downhill the southerly wind diminished in force until a level was reached where no wind could be detected.

This calm zone was about 12 ft. wide. Below it was a very light wind blowing down the slope—the katabatic wind. This phenomenon of an uphill hilltop wind separated from the katabatic wind by a calm zone, which does not appear to have been noted by other observers, was found at other places during the summers of 1936 and 1937. The hilltop wind was found, moreover, in all seasons on nineteen clear nights when it was calm in the valleys, and on fifteen of these nights preserved the same direction as that of the wind of the day before.

The influence of woods on the katabatic wind was studied on Bluebell Hill. It was found, early in September 1937, that the katabatic wind was present both within and below a wood consisting of widely spaced beech trees about 30 ft. high and a dense undergrowth reaching down to the surface of the ground. The trees were too far apart to form an overhead canopy. Similar results were obtained in a different type of wood in another part of the country in July 1937. This is an interesting observation in view of the relative impenetrability to ordinary winds of woods like these that appeared "solid" to the eye. It may be due in part to the slow and steady character of the katabatic winds and perhaps also, one might add, to their stability, which makes it difficult for them to surmount tall obstacles. On the other hand a 3 ft. thick beech hedge, 7 ft. high, was observed on one occasion to be surmounted by a katabatic wind which had a speed of 1 m.p.h. in the open, and comparisons with anemometers showed that the wind did not penetrate the hedge.

The winds on the slope facing away from the hilltop wind were studied. On one occasion (March 22, 1937) when katabatic winds were blowing on the lower slopes of a hill facing the hilltop wind, it was found, by observations of smoke, that on the leeward side of the hill the air was calm for a depth of several hundred feet over the foot of the steepest part of the slope. This Cornford explains as being the result of the formation of an eddy by the hilltop wind, the return current of which would just counterbalance the katabatic wind. It was observed on several occasions that the katabatic wind blew no faster when the hilltop wind had the same direction than when the latter was from the opposite direction to that of the katabatic wind.

Variations of temperature within katabatic winds gave no simple relationship between temperature and altitude: sometimes the lowest and sometimes the highest places had the lowest temperatures. The influence of the nature of the vegetation covering the soil on temperature, to which reference has already

been made, is one disturbing factor, because the existence of a dense covering of grass or clover was found to produce a marked lowering of the temperature. This effect was so pronounced that in some regions a particular spot was the coldest on every occasion, and the whole series of thermometers was apt to show a distribution of temperature quite unrelated to elevation. The vegetation factor was eventually studied alone by selecting level land with a large variety of clover, in Kent. On several successive nights it was found that the minimum over grass or clover was from 5 to 6½° F. lower than the minimum over bare soil. There was a marked difference also between the minimum over tall dense grass and that over shorter and irregular grass, the former being the lower by about 3° F. It was also observed that the lowering of the minimum temperature is greater in the middle of a grass field than near the margins between it and bare or less densely covered ground. Experiments made at Birling with a line of thermometers set in the direction of the katabatic wind, so that air passed successively over all the thermometers in turn, showed that the air temperature fell about 2° F. when air passed from bare soil to wheat and returned almost to its original temperature when it emerged from over the wheat and again passed over bare soil. These observations were made in June 1937 with a katabatic wind blowing at a speed of 2–3 m.p.h. A very good example of the overshadowing of the effect of elevation by that of vegetation was obtained in July 1937 in a small valley at Leckford, Hants. The higher ground adjoining this valley was grass-covered while the valley bottom was bare. A line of 18 thermometers was placed in the valley bottom and others on the grass-covered slopes. The lowest temperatures were recorded over the grass-covered slopes; in some instances temperature was as much as 3·5° F. higher in the valley bottom, although records with anemometers showed that the air moved down from the colder to the warmer regions and thereby reduced the difference. When, however, differences of vegetation were not present, *i.e.* when both the high and low ground were both grass-covered or both bare, the lowest temperatures were generally found at the lowest levels.

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BIOCHEMISTRY.

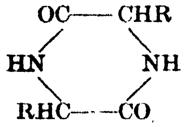
By W. O. KERMACK, D.Sc., LL.D., F.R.S.E., Research Laboratory, Royal College of Physicians, Edinburgh.

THE STRUCTURE OF PROTEINS.—Nothing is more urgently required by the biochemist than a more complete knowledge of protein structure. Substances of this class are so essential to living cells and perform so important functions that it is not surprising that in spite of their unpromising complexity they are the subject of ever-increasing interest. The great interest in the subject at the present time is shown by the number of books recently published on the subject and of general discussions which have taken place. We may instance, for example, the comprehensive volume edited by Carl L. A. Schmidt under the title of *Chemistry of the Amino Acids and Proteins*, Lloyd and Shore's *Chemistry of the Proteins* of which the second edition has recently been published, and the discussions held on protein chemistry at the British Association 1937 and at a meeting of the Royal Society in November 1938 (cf. *Nature*, 1938, **142**, 1023).

It is characteristic of the modern attack on the problem of protein structure that it is being carried out simultaneously on a large variety of fronts. The techniques of organic chemistry, of enzyme chemistry, of X-ray analysis and electron diffraction, the large resources of physical chemistry, viscosity measurements, dielectric constants, conductivity and Raman spectra are all being applied in their appropriate place. A small volume would be required to give even a condensed summary of this large mass of research. In the present article we can deal with only one or two of the more interesting advances and speculations. Reference may here be made to an able review recently published by T. W. Taylor (*Ann. Rep. Chem. Soc.*, 1937).

According to the classical view, a protein molecule consists of a large number of amino acids, bound together in long chains, chiefly by means of peptide linkages. Though much evidence has accumu-

lated in favour of this conception of protein structure, it has not been entirely unchallenged. Amongst the products of protein hydrolysis, there are found not only amino acids and polypeptides, but also diketopiperazines (I), formed from a dipeptide by loss of water.



(I)

R represents a side chain characteristic of the amino acid.

These diketopiperazine rings may either exist in the original protein molecule or be formed from the amino acids during the process of hydrolysis. Few probably would reject the possibility that a protein may contain a small number of preformed diketopiperazine rings. But the idea has been put forward that such rings play a much more important rôle, that they are the essential units of the protein molecule and that the latter is formed by the aggregation or polymerisation of numerous diketopiperazine rings. It was pointed out that no protease, that is an enzyme acting on a native protein, would hydrolyse any synthetic polypeptide, whilst no polypeptidase, the type of enzyme which acts on polypeptides, natural or synthetic, would attack a native protein. These facts certainly form a prima facie case against the view that the peptide chain exists in the native protein ; for enzymes which split native proteins, presumably do so by hydrolysing peptide links and should also act on the peptide links of suitable synthetic polypeptides. The difficulty became more acute when Ishiyama and Tazawa (cf. *J. of Biochem. Japan*, 1932–33, **17**, 285, and *Acta phytochim. Tokyo*, 1935, **8**, 331) reported that trypsine and pepsine could slowly hydrolyse suitable diketopiperazine derivatives. This result was of course a strong argument in favour of the existence of preformed diketopiperazine rings in the protein molecule.

Recent work, however, has removed these objections to the peptide theory of protein structure. First of all, very serious doubt has been cast on the results of the Japanese workers. Waldschmidt-Leitz and Gärtner (*Z. physiol. Chem.*, 1936, **24**, 221) and Greenstein (*J. Biol. Chem.*, 1936, **112**, 517) report that proteinases and even polypeptidases failed completely to hydrolyse diketopiperazines. Furthermore, Bergmann and Fruton have found that certain proteinases will act on particular polypeptides which they have prepared synthetically (*J. Biol. Chem.*, 1937, **118**, 405). Thus chymotrypsin acts on carbobenzoxyglycyl-*l*-tyrosylglycine-

amide with great rapidity. Other proteinases such as cathepsin, bromelin and papain, have also been shown to attack suitable synthetic polypeptides so that the enzymatic evidence is now strongly in favour of the existence of the peptide linkage in the protein.

Perhaps the most interesting and fundamental of Bergmann's many recent contributions to protein chemistry is that which he and Niemann have published in a series of papers on protein structure. The experimental side of this work consists in the determination of the amino acid content of a number of proteins, including gelatin, cattle hæmoglobin, egg albumin, serum fibrin and silk fibroin (Bergmann, *J. Biol. Chem.*, 1935, **110**, 471 ; Bergmann and Niemann, *ibid.*, 1936, **115**, 77 ; 1937, **118**, 301 ; 1938, **122**, 577). These determinations have been made with the maximum possible accuracy, and in order to attain this they have developed a number of new analytical methods which, however, cannot be discussed here.

It has of course been long appreciated that accurate analysis of a protein, as of other compounds, yields important information about the molecular structure, and in particular about the molecular weight. Thus if hæmoglobin contains 0·33 per cent. of iron (atomic weight 56), it follows that the molecular weight must be $\frac{56 \times 100}{0{\cdot}33} = 17{,}000$ or some exact multiple of this, as the hæmoglobin molecule must contain one, two, three or some other exact number of iron atoms. If it is further found that it contains 4 atoms of iron to 3 of cysteine sulphur to 12 of arginine, then the molecular weight must be about $4 \times 17{,}000 = 68{,}000$, or some exact multiple of this.

Bergmann and Niemann make two fundamental assumptions in developing their theory of protein structure. They first of all assume that the constituent amino acids are arranged in a simple linear series, that is, there is no branching of the polypeptide chain. A cyclic arrangement, however, is not excluded, and indeed would seem to avoid certain difficulties to be mentioned later. The second assumption is that the same amino acid occurs at regular intervals, that is to say, periodically along the chain. Thus, for example, every second acid might be glycine, every fourth one alanine and every sixteenth one tyrosine and so on. The number denoting the spacing, 2, 4, or 16 . . . is called the frequency and denoted by f.

One simple experimental test of the theory at once suggests itself. If the assumed frequencies be calculated from experimental data, they should always turn out to be whole numbers (within experimental error). In order to calculate the frequency of a

particular amino acid we require not only the weight W of that amino acid yielded by 100 g. of the protein and its molecular weight M, but also the average molecular weight of all the amino acids in the protein. The latter less 18, the molecular weight of water, gives the average molecular weight of the amino acid residues in the protein molecule. If this value is A, then 100/A is the number of amino acids in 100 g. of the protein. As this yields W g. of the acid in question, or W/M molecules, the frequency must be 100/A divided by W/M or 100M/WA.

If the complete analysis of the protein could be accurately carried out, the exact value of A could be calculated. However, in the best cases, only about 80 per cent. of the protein is accounted for by the analysis and all that can be done is to calculate A from this fraction. In certain instances a small correction justifiable on the basis of special considerations, may be introduced, but naturally the admission of more or less arbitrary modifications of this kind lessens the significance of the results. The experimental values F′ found for the various amino acids in a number of proteins are shown in Table I.

TABLE I.

Amino Acid.	Silk Fibroin.		Cattle Globin.		Blood Fibrin.	
	F′	F	F′	F	F′	F
Glycine	2·04	2	—	—	—	—
Alanine	4·01	4	—	—	—	—
Tyrosine	16·3	16	47·5	48	—	—
Arginine	218·1	216	48·6	48	17·4	18
Lysine	695·3	648	15·8	16	11·1	12
Histidine	2637·0	2592	18·1	18	47·6	48
Aspartic Acid	—	—	18·0	18	17·3	18
Glutamic Acid	—	—	36·4	36	8·0	8
Proline	—	—	47·4	48	17·3	18
Cysteine	—	—	190·5	192	62·0	64
Tryptophane	—	—	—	—	31·4	32
Methionine	—	—	—	—	44·1	48
Number of Residues	—	2592	—	576	—	576
Average Residue Weight (A)	—	84	—	115·5	—	130
Molecular Weight (Minimum)	—	217,728	—	66,528	—	74,880

$F' = \frac{100M}{WA}$ is the experimental value of the frequency F.

If we first of all consider those amino acids which have relatively low frequencies it will be seen that in almost every case the value of F′ is a whole number of the form 2^m3^n, m and n being either 0 or an integer. The more reliable the analytical method for the acid in question, the more exact in general is the agreement. Bergmann and Niemann therefore make a third assumption—namely, that all the frequencies are of this form. The observed values of the larger frequencies sometimes involve considerable experimental errors, but as will be seen from the table they do, in fact, lie in the neighbourhood of integers of the assumed type, which latter are therefore taken as the correct values of the frequencies in question.

If we regard the polypeptide chain as a closed cyclic one, the periodic hypothesis implies that the total number of amino acid residues must be an exact multiple of each frequency the molecule contains, that is to say, the number of amino acid residues must be either the lowest common multiple of all the frequencies or some simple multiple of this. Clearly the minimum number must be of the form 2^M3^N where M and N are integers. In cattle hæmoglobin, for example, the number was found to be 576 or 2^63^2. The molecular weight is then found by multiplying this number by the average residue weight A, in this case 115·5.

Thus the minimum molecular weight of cattle globin is found to be 66,500, a result in close agreement with that found by other methods. The minimum molecular weights of other proteins calculated by this method are given in the table.

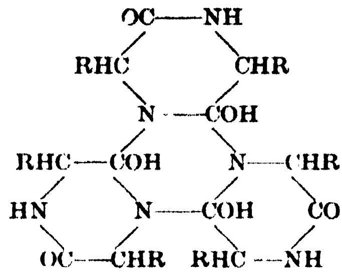
One important consequence of this theory is that it implies that the molecular weights of the proteins are not distributed at random but that the proteins fall into a series of classes, each consisting of those with the same number of amino acids and therefore of approximately equal molecular weight. Thus those of one class contain 288 amino acid residues, those of another 576 and so on, and the exact molecular weight will depend on the average residue weight for that particular protein. As the number of amino acids is always of the form 2^M3^N, it is to be expected that the molecular weights will appear in classes grouped round average values which bear whole number relations to each other. Now this is precisely the state of affairs found experimentally by Svedberg. A number of proteins have molecular weights of approximately 34,000, others 68,000, others about 140,000 and so on (*cf.* SCIENCE PROGRESS, 1933, **27**, 418). More recently a group has been found with molecular weight of about 18,000 (*cf.* Svedberg, *Nature*, 1937, **139**, 1051), and if 17,600 is taken as the unit, the vast majority of the 40 odd proteins examined are found to have weights in the neigh-

bourhood of an exact multiple of this unit. Furthermore, the multiples are found to be 1, 2, 4, 8, 16, 24, etc., all of which are numbers of the form 2^m3^n as required by Bergmann's theory. Moreover, 17,600 would itself correspond to 144 residues which is 2^43^2 so that Svedberg's experimental results are exactly what one would expect if Bergmann's theory were true.

All this is very pretty, but the theory is not without its difficulties. The molecule of silk fibroin for example is certainly linear and not cyclic. Now, the periodicity hypothesis applied to a linear chain means that the enzymatic or other regulating forces which control the synthesis of the molecule not only control the periodic spacing but manage to accommodate the end effects. If, for example, an acid with a frequency of 64 begins 13 from the beginning of the molecule it must finish $64 - 13 = 51$ from the end. It is not easy to see how this relationship can be ensured between the two ends of a molecule separated by perhaps thousands of amino acids and peptide links. It must, too, be remembered that from simple stoichiochemical considerations, such as were referred to at the beginning of this discussion, and Bergmann's accurate analysis of the proteins, approximate minimum molecular weights can be deduced without making use of Bergmann's assumptions. We would not, of course, in this way be able to deduce the exact number of amino acids—in a globin molecule there might be 560 or 580—but it has not yet been proved that the number is really exactly 576. Bergmann's assumptions fit the facts but cannot be looked upon as completely proven. This is especially the case in relation to the rarer amino acids in a molecule for which the frequency is large. If an acid, for example, is found with a frequency of 128 it would be difficult to be certain that this acid does always occur at exactly regular intervals and that minor irregularities are excluded. Still greater accuracy in analytical methods may be required in order to establish how far the theory applies in complete detail.

Another theory of protein structure which has attracted considerable attention during the past two or three years is that of the cyclol hypothesis proposed by Dr. D. Wrinch. The fundamental assumption is that the polypeptide chain is folded into a system of rings each containing six atoms and that each six membered ring is then completed by a slight rearrangement of the bonds and migration of a hydrogen atom. The process is illustrated in (II).

As will be seen, some of the rings are diazine rings with two nitrogen atoms and four carbons, others are triazine rings with three of



(II)

each kind. The system can readily be extended to form a monomolecular plain sheet, and it is believed that monomolecular films of protein consist of such cyclol sheets.

The most interesting application of the cyclol hypothesis is, however, to the native globular proteins. It is shown that the cyclol framework folds naturally into an approximately spherical structure, a kind of network or cage, and calculation shows that such a cage may contain 72, 288, 648 . . . $72.n^2$ amino acid residues. Now the second number of the series is 288 and it is therefore assumed that a cage molecule with 288 amino acids represents globular proteins such as Insulin with molecular weight in the neighbourhood of 35,000.

The experimental observations relating to crystalline insulin are compared with this cage molecule theory at considerable length in a paper by D. M. Wrinch (*Trans. Faraday Soc.*, 1937, **33**, 1368), and it is concluded that the theory co-ordinates a large number of facts and is consistent with the known data. In particular the size of the globular molecule, which is readily calculated on the theory, agrees well with the space taken up by the molecule in the crystal of insulin. An attempt has recently been made by D. Wrinch and I. Langmuir (*J. Amer. Chem. Soc.*, 1938, **60**, 2274) to obtain a much more decisive conclusion in favour of this theory from an analysis of the X-ray observations made by Crowfoot on crystalline insulin. The problem is so complex that various simplifying assumptions have to be introduced, but the general conclusion is that the X-ray intensities observed agree closely with those to be expected from a molecule of the shape and structure they assume. This they regard as a triumph for the cyclol hypothesis and a proof of the correctness of the cage molecule structure (*cf.* Wrinch and Langmuir, *Nature*, 1939, 49).

It seems, however, that the experts are not convinced. Bernal and Bragg (*Nature*, 1939, 74) have independently pointed out that it is impossible to locate the position of thousands of atoms in a

molecule from a few dozen observations and that many other interpretations might be consistent with the facts.

The two theories, those of Bergmann and Wrinch, are both stimulating and attractive, but in important respects they would seem to be inconsistent with each other. One postulates a linear arrangement of amino acids connected by peptide links, the other assumes a two- or three-dimensional configuration involving the characteristic diazine and triazine rings. Bergmann would doubtless agree that in the globular proteins his linear polypeptide is twisted into a three-dimensional configuration kept in position by relatively weak bonds which give way during de-naturation so that the protein collapses into more or less parallel bundles of peptide chains as suggested by X-ray observations (see Astbury and Lomax, *J.C.S.*, 1935, 846). On the other hand the cage skeleton suggested by Wrinch has one feature which may be of great significance in relation to Bergmann's results. It is a highly symmetrical structure having planes of symmetry, axes of symmetry and, what is very important, the molecule has trigonal or hexagonal symmetry about these axes. The various amino acids are characterised by their side chains, and it may be that what is important is that those side chains must be arranged so as to preserve elements of symmetry. To do this they must be introduced in groups of 2 or 3, 4, 6, 12, etc. A group of 7 identical side chains, for example, could not be introduced without disturbing the symmetry. Thus a three-dimensional network after the Wrinch pattern might form an explanation of Bergmann's frequency rule. On the other hand, it may be doubted whether the cyclol cage can be insisted on to the exclusion of all other possibilities. The existence, for example, of proline and oxyproline residues in insulin and most proteins makes an exclusive cyclol theory impossible, for these two amino acids contain a secondary but no primary amino group, so that the cyclising process is excluded. Then, too, the fact that the native proteins contain not only 288 amino acids but also approximately 144, 576, 864, etc., is troublesome. A less rigid theory of molecular architecture, perhaps laying much stress on hexagonal folding of the peptide chain and above all things emphasising symmetry considerations, but utilising many types of cross linkages other than the cyclol bond, may perhaps prove capable of accommodating more easily the experimental facts. Should this be so, the cyclol hypothesis will have been a most valuable contribution as a simplified model indicating the way.

GEOLOGY. By G. W. Tyrrell, A.R.C.Sc., D.Sc., F.R.S.E., The University, Glasgow.

Stratigraphical and Regional Geology.—W. J. McCallien describes "The Geology of the Rathmullan District, Co. Donegal" (*Proc. Roy. Irish Acad.*, XLIV, Sect. B, No. 5, 1937, 45–59), a Dalradian area in which the Crana Quartzite is correlated with the Crinan (Loch Awe) Grits of Argyllshire. The complicated structure of the quartzite group has been made out with the aid of the associated epidiorite sills.

The Anniversary Address of the President, Professor O. T. Jones, to the Geological Society in February 1938, was "On the Evolution of a Geosyncline" (*Quart. Journ. Geol. Soc.*, XCIV, 1938, *Proc.* lx-cx), and was based on the deposits accumulated in the British area during the Lower Palæozoic era. Three basins of deposition were developed simultaneously: the Welsh geosyncline which was continued into the Lake District, the Moffat geosyncline, and the Durness geosyncline with a possible extension into the Girvan district. Land masses occupied the sites of the Irish Sea and the region east of the Welsh geosyncline during parts of the era. All these areas are now covered by water, or by post-Palæozoic deposits; which appears to have been the usual fate of land masses that supplied sediments to the Lower Palæozoic basins of deposition in many parts of the world.

In his paper "On the Sliding or Slumping of Submarine Sediments in Denbighshire, North Wales, during the Ludlow Period," O. T. Jones (*Quart. Journ. Geol. Soc.*, XCIII, 1937, 241–283D) comes to the conclusion that "the disturbed beds are primarily the result of recurrent sliding or slumping of the mudstones on the sea floor during the Lower Ludlow period, and that they now occur as intercalations at several horizons in a great stratigraphical sequence of Lower Ludlow mudstones." This is, of course, in accord with now prevailing views on the origin of "disturbed beds" at many geological horizons throughout the world.

On the other hand, P. G. H. Boswell, in a paper entitled "The Tectonic Problems of an Area of Salopian Rocks in North-western Denbighshire" (*ibid.*, 284–321), adheres to his earlier interpretation of the structures of this area as of tectonic origin, although he does not dispute the occurrence of sliding and slumping at certain horizons and over strictly limited areas.

In a paper on "Contemporaneous Slumping and other Problems at Bray Series, Ordovician, and Lower Carboniferous Horizons in Co. Dublin," A. Lamont (*Proc. Roy. Irish Acad.*, XLV, Sect. B, No. 1, 1938, 1–32) describes submarine slidings of sediments which

appear to have been a surface expression of deep-seated tectonic adjustments. The Ordovician movements are connected with faulting between the Caradocian and Ashgillian. The Carboniferous landslides, which may have been set off by seismic agency, were intensified during the Nassau and Sudetic phases of orogenesis on the Continent.

Studies on Welsh Lower Palæozoic stratigraphy have been published in the following papers: S. H. Straw, "The Higher Ludlovian Rocks of the Builth District" (*Quart. Journ. Geol. Soc.*, XCIII, 1937, 406–56); J. R. Earp, "The Higher Silurian Rocks of the Kerry District, Montgomeryshire" (*ibid.*, XCIV, 1938, 125–60); C. A. Matley, "The Geology of the Country around Pwllheli, Llanbedrog and Madryn, South-west Carnarvonshire" (*ibid.*, 555–606); H. B. Whittington, "The Geology of the District around Llansantffraid ym Mechain, Montgomeryshire" (*ibid.*, 423–57).

The most striking result of E. M. L. Hendrik's work on "Rock Succession and Structure in South Cornwall: a Revision. With Notes on the Central European Facies and Variscian Folding there present" (*ibid.*, XCIII, 1937, 322–67) is that beds which have hitherto been represented on maps as "Grampound," "Manaccan," "Falmouth," and part of the Veryan (Ordovician) Series, really form a continuous suite, now called the Gramscatho Beds, and are not older than Middle Devonian on stratigraphical and palæontological evidence. Lateral movements which produced tectonic breccias are contemporaneous with an intrusion of serpentine like that of the Lizard. This intrusion and the pre-existing rocks are correlated with the serpentines and hornblende-schists of the Variscian folds in the Lower Carboniferous province of Central Europe.

It has long been known that the Carboniferous Limestone Series in the Midland Valley of Scotland is prone to vary greatly in thickness from place to place. Following the work of R. W. Dron and a suggestion due to the late B. N. Peach, J. E. Richey (*Summ. Prog. Geol. Surv. for 1935,—Part II*, 1937, 93–110) has shown that these variations take place in the neighbourhood of, and in connection with faults that traverse the underlying formations, and were still moving during the deposition of the Carboniferous Limestone Series.

Further papers on British and Irish Carboniferous rocks are the following: J. S. Turner, "The Faunal Succession in the Carboniferous Limestone near Cork" (*Proc. Roy. Irish Acad.*, XLIII, Sect. B, No. 13, 1937, 193–209); T. F. Sibly and S. H. Reynolds, "The Carboniferous Limestone of the Mitcheldean Area, Gloucester-

shire" (*Quart. Journ. Geol. Soc.*, XCIII, 1937, 23–51); L. R. Moore and A. E. Trueman, "The Coal Measures of Bristol and Somerset" (*ibid.*, 195–240); E. Greenly, "The Red Measures of the Menaian Region of Carnarvonshire" (*ibid.*, XCIV, 1938, 331–46).

The two following papers deal with aspects of Carboniferous igneous history in the Midland Valley of Scotland: G. A. Cumming, "The Structural and Volcanic Geology of the Elie–St. Monance District, Fife" (*Trans. Edin. Geol. Soc.*, XIII, Pt. III, 1936, 340–66); C. G. Dixon, "The Geology of the Fintry, Gargunnock, and Touch Hills" (*Geol. Mag.*, LXXV, 1938, 425–32).

G. M. Lees and P. T. Cox describe "The Geological Basis of the present Search for Oil in Great Britain by the D'Arcy Exploration Co., Ltd." (*Quart. Journ. Geol. Soc.*, XCIII, 1937, 156–94). The prospects cover four separate and largely unrelated sets of geological conditions, namely, those of the Lower Carboniferous Oil Shale Series in Scotland, the Permian in North-Eastern Yorkshire, the Carboniferous of the Midlands and eastern England, and the Jurassic–Lower Cretaceous of southern England. Many interesting new stratigraphical data have been gathered in the course of these investigations, but unfortunately little oil as yet.

P. G. H. Boswell has summarised "The Geology of the New Mersey Tunnel" (*Proc. Liverpool Geol. Soc.*, XVII, Pt. II, 1937, 160–91) from experience gained during its construction. The fault which had been predicted by Morton in the Middle Bunter Sandstone was duly met with, as also the buried glacial channel of the Mersey predicted by Mellard Reade. "Never before has a buried glacial channel been explored so thoroughly as that of the River Mersey."

The following are recent papers on the Pleistocene and Glacial history of Britain: L. J. Wills, "The Pleistocene History of the West Midlands" (*British Association, Pres. Addresses, Nottingham, 1937*, 71–94); S. E. Hollingworth, "The Recognition and Correlation of High-level Erosion Surfaces in Britain: a Statistical Study" (*Quart. Journ. Geol. Soc.*, XCIV, 1938, 55–84); S. W. Wooldridge, "The Glaciation of the London Basin and the Evolution of the Lower Thames Drainage System" (*ibid.*, 627–67).

In his paper "Über Sedimentgesteine in der Leptitformation Mittel-Schwedens—Die sogennante 'Larsboserie,'" S. Hjelmqvist (*Sver. Geol. Undersök. Årsbok*, **32**, No. 3, 1938, 5–39) describes the Larsbo Series as consisting of quartzites and mica-quartzites on the periphery of its outcrop, but passing through schistose greywacke and mica-schist to an innermost core of oligoclase-gneiss. The series belongs to a geosynclinal, and is regarded as a Flysch forma-

tion built of the erosion products of mountain ranges due to the folding of the Leptite formation.

S. Føyn's memoir on "The Eo-Cambrian Series of the Tana District, Northern Norway" (*Norsk Geol. Tidsskr.*, **17**, 1937, 65–164) deals with the stratigraphy and petrology of a series of sandstones, siltstones and shales, with two horizons of tillite indicating a local glaciation, which belong to the earliest Cambrian. This formation was formerly thought to be post-Cambrian, but Holtedahl's later view that it is equivalent to the Eo-Cambrian Sparagmite is confirmed by Føyn's work.

The paper by N. Tilmann *et alia*, "Contributions to the Geology of the Rhenish Schiefergebirge" (*Proc. Geol. Assoc.*, XLIX, 1938, 1–48. With Report of Excursion, *ibid.*, 225–60) is one of the valuable excursion guide series of the Geologists' Association. The region is "distinguished by a complete development of the later Palæozoic rocks of Devonian and Carboniferous age, with numerous facies changes, while the earlier Palæozoics (the Cambrian and Silurian) only appear in certain cores of large anticlines now laid bare."

Two further papers on Continental stratigraphy and regional geology are the following: K. H. Scheumann, "Zur Frage nach dem Vorkommen von Kulm in der Nimptscher Kristallinzone (Sächsen)" (*Min. u. Petr. Mitt.* 49, 1937, 216–40); R. J. H. Patijn, "Geologische Onderzockingen in de Oostelijke Betische Cordilleran" (*Univ. Amsterdam. Inaug. Diss.* 1937, 130 pp.).

A valuable summary of "The Geology of the Oilfield Belt of Iran and Iraq" is given by G. M. Lees in *The Science of Petroleum* (Oxford Univ. Press, 1938, 140–8). The subject is really the geology of the Zagros ranges of south-western Persia, a sector of the Alpine-Himalayan System. There is a remarkable uniformity in the structural and depositional conditions of the Zagros; and a notable feature is the dominance of limestones and marlstones from Permian to Palæogene times. The main reservoir rock is the porous Asmari Limestone of Lower Miocene and Upper Oligocene age.

Three interesting papers on the geology of the Indian Empire have been published by the Indian Science Congress Association, all by D. N. Wadia: "Progress of Geology and Geography in India during the past Twenty-five Years" (*Indian Sci. Congr. Assoc., Calcutta*, 1938, "Progress of Science in India during the past Twenty-five Years," 86–132); "An Outline of the Geological History of India" (*ibid.*, "An Outline of the Field Sciences of India," 43–68); "The Structure of the Himalayas and of the North Indian Foreland" (*Pres. Address, Sect. of Geology*, 25th Indian Science Congress,

Calcutta, 1938, 28 pp.). In the last-named paper Dr. Wadia concludes : " The plan of the great edifice of the Himalayas is discernible only in the haziest outline yet. We cannot be so bold as to say that the Himalayas are built on the plan of the Alps, nor even that their architecture is individual. No doubt several tectonic features are common, and the Alpine–Himalayan axis of earth-folding originated in one common and continuous impulse. But the proportions are so vastly different, that the very magnitude of the earth-waves raised in the case of the Himalayas gave them comparatively simpler tectonics. . . ."

A memoir of great value to Indian geology is Sir L. L. Fermor's " An Attempt at the Correlation of the Ancient Schistose Formations of Peninsular India." Part I.—General Discussion (*Mem. Geol. Surv. India*, LXX, Pt. I, 1936, 1–51) ; Part II.—Accounts of Provinces, No. 1. Non-Charnockitic Region—Iron-ore Provinces (*ibid.* 53–217). As this memoir is unfinished it is premature to attempt its appraisement just now.

Writing on " The Distribution of the Regional Isostatic Anomalies in the Malayan Archipelago," R. W. van Bemmelen (*De Ingen. in Ned.-Indie. IV. Mijnb. en Geol.* " *De Mijningenieur*," Jaarg. V, No. 4, 1938, 61–7) shows that the main belt of negative anomalies is directly connected with the surface feature of the Soenda mountain arcs, whereas the positive zones are not directly connected with the surface relief. This feature cannot be easily explained by the orthodox compression theory of folding, but finds a more consistent explanation on the theory of disturbance of hydrostatic equilibrium in the sub-crust by magmatic differentiation (Undation theory).

The same author also discusses " Examples of Gravitational Tectogenesis from Central Java " (*ibid.*, Jaarg. IV, No. 4, 1937, 55–65). The object of this paper is to demonstrate that the bicausal interpretation of orogenesis (primary vertical movements or undations due to magmatic differentiation, followed by secondary gravitational reactions) provides a sound scheme for the observed tectonic events in a part of Central Java.

" The Structure of the Arabian Peninsula " is discussed by L. Picard (*Geol. Dept. Hebrew Univ. Jerusalem*, Ser. I, Bull. 3, 1937, 9 pp.). The main element is the basement of ancient rocks (Suess's " Arabian Shield "). Around this massif, after a belt of plateau country, there comes zone after zone of folding, both of Jura type and true Alpine type.

The object of K. S. Sandford's paper " Observations on the Geology of Northern Central Africa " (*Quart. Journ. Geol. Soc.*, XCIII, 1937, 534–80) is to sketch the geological history of the vast

continental region between the Belgian Congo and Cyrenaica, and between the Nile and the French Sahara and Sudan. In his summary Sandford shows that the geological history of this region comprises four continental periods alternating with three marine periods, ranging in age from the Archæan to the Quaternary.

Two papers on Northern Rhodesian geology by F. Dixey are the following: "The Geology of Part of the Upper Luangwa Valley, North-eastern Rhodesia" (*Quart. Journ. Geol. Soc.*, XCIII, 1937, 52–76), and "The pre-Karroo Landscape of the Lake Nyasa Region, and a Comparison of the Karroo Structural Directions with those of the Rift Valley" (*ibid.*, 77–93). The first deals mainly with rocks of the Karroo System. The second describes some interesting features in the relations of the Karroo sediments to the land surface of that time, and shows that certain elements of the ancient surface still play an important part in the modern landscape.

The active Geological Survey of Southern Rhodesia has recently published two new memoirs: J. C. Ferguson and (the late) T. H. Wilson, "The Geology of the Country around the Jumbo Mine, Mazoe District" (*S. Rhodesia: Geol. Surv. Bull.* No. 33, 1937, 137 pp.); A. E. Phaup and F. O. S. Dobell, "The Geology of the Lower Umfuli Gold Belt, Hartley and Lomagundi Districts" (*ibid.*, Bull. No. 34, 1938, 150 pp.).

F. B. Wade has published a valuable "Stratigraphical Classification and Table of Tanganyika Territory" (*Tanganyika Territory: Dept. of Lands and Mines, Geol. Div.*, Bull. No. 9, 1937, 60 pp.).

A number of papers on the geology of various parts of the Canadian Shield have lately appeared. The late W. H. Collins gave a conspectus of the "Sudbury Series" (*Bull. Geol. Soc. Amer.*, **47**, 1936, 1675–90), based on his own long-continued work, and on that of other investigators. This formation consists exclusively of clastic materials, conglomerate, quartzite, greywacke and argillite. As the deposition of these sediments followed closely on the waning Keewatin volcanism, they are composed mainly of volcanic materials.

Another paper by W. H. Collins, "Timiskaming Sub-Province" (*ibid.*, **48**, 1937, 1427–58), was intended for a chapter in a projected book on the geological history of the Canadian Shield by Collins and several collaborators, and is now edited by C. K. Leith who contributes an introductory note. The paper summarises the geological history of the Timiskaming sub-Province, which extends northward from Lake Huron to Hudson's Bay. It includes a restudy of the classical original Huronian area.

F. J. Pettijohn's study of "Early Pre-Cambrian Geology and

Correlational Problems of the Northern Province of the Lake Superior Region" (*ibid.*, 153–202) follows much the same lines as the preceding. He poses two alternative hypotheses for the early Pre-Cambrian: one postulates two periods of volcanism separated by a period of continental sedimentation; the other, three periods, the lower two comprising both sedimentary and volcanic episodes, the uppermost being largely sedimentary. It is concluded that "it seems necessary to abandon the terms Coutchiching, Keewatin and Timiskaming for the strata in this area."

The same author describes an "Early Pre-Cambrian Varved Slate in North-western Ontario" (*ibid.*, **47**, 1936, 621–8). It occurs in a "Timiskaming-type" series (Abram Series) consisting mainly of conglomerates of terrestrial origin, with which are associated greywackes, siltstones, argillites, iron formations and tuffs. The argillites are finely banded, and may be "varved." The author concludes that an annual control of sedimentation was probably responsible for the banding, but cannot be certain that the control was glacial.

In a second paper on the subject C. Schuchert endeavours again to unravel the complicated succession and structure of the "Cambrian and Ordovician of North-western Vermont" (*ibid.*, **48**, 1937, 1001–78). The area embraces part of the well-known Taconic Mountains, in which tardy recognition of large-scale overthrusting by Keith led to the settlement of the long-continued "Taconic Controversy."

The following memoirs deal mainly with the stratigraphy and petrology of Pre-Cambrian areas in the Adirondacks of New York state: A. F. Buddington, "Geology of the Santa Clara Quadrangle, New York" (*New York State Mus. Bull.* No. 309, 1937, 56 pp.); R. S. Cannon, "Geology of the Piseco Lake Quadrangle, New York" (*ibid.*, No. 312, 1937, 107 pp.).

The following memoirs deal mainly with the stratigraphy of Lower Palæozoic regions in Eastern Canada: A. O. Hayes and B. F. Howell, "Geology of Saint John, New Brunswick" (*Geol. Soc. Amer. Special Papers* No. 5, 1937, 146 pp.); W. H. Twenhofel, Geology and Palæontology of the Mingan Islands, Quebec" (*ibid.*, No. 11, 1938, 132 pp.); W. H. Twenhofel and R. R. Shrock, "Silurian Strata of Notre Dame Bay, Newfoundland" (*Bull. Geol. Soc. Amer.*, **48**, 1937, 1743–72); G. R. Heyl, "Silurian Strata of White Bay, Newfoundland" (*ibid.*, 1773–84).

The following are two memoirs dealing with the geology of areas in the western United States: A. D. Howard, "History of the Grand Canyon of the Yellowstone" (*Geol. Soc. Amer. Special Papers*

No. 6, 1937, 159 pp.); F. E. Turner, "Stratigraphy and Mollusca of the Eocene of Western Oregon" (*ibid.*, No. 10, 1938, 130 pp.).

The revival of mining interest in the great Comstock Lode has led to re-study of the "Geology of the Silver City District and the Southern Portion of the Comstock Lode, Nevada," by V. P. Gianella (*Univ. of Nevada Bull.* XXX, 1936, 1–108). The rocks range from Triassic to Quaternary, and the structure of the region is complex. The Tertiary formations comprise a thick series of volcanic rocks separable into Eocene, Miocene and Pliocene groups. The mineral veins were formed at about middle Miocene times.

C. Deiss has studied, re-measured, and accurately zoned the "Cambrian Formations and Sections in Part of the Cordilleran Trough" (*Bull. Geol. Soc. Amer.*, **49**, 1938, 1067–1168). The work seems to have resulted in a complete re-arrangement of the Cambrian successions in Nevada, Utah and Wyoming.

The Uinta Mountains of Utah and Colorado are of interest to geologists because of their unusual erosional features, and also because they form the largest east-west trending range in the Western Hemisphere, in strong contrast to the north-south trends of the Rocky Mountains System. J. D. Forrester, in a memoir on the "Structure of the Uinta Mountains" (*ibid.*, **48**, 1937, 631–66) discusses the origin and peculiar orientation of the range.

The memoir by E. T. Hodge on the "Geology of the Lower Columbia River" (*ibid.*, **49**, 1938, 831–930) aims at describing the general geology as a basis for showing how the river came to have its present course through the Cascade Mountains. The volcanic history, entirely Tertiary, is complex, an early and a late andesitic series, with the Columbia Basalts between them, interdigitating with several continental sedimentary formations.

The Geological Society of America has recently published four long and detailed memoirs on Mexican geology: L. B. Kellum *et alia*, "Evolution of the Coahuila Peninsula, Mexico" (*Bull. Geol. Soc. Amer.*, **47**, 1936, 969–1176); R. W. Imlay, "Geology of the Middle Part of the Sierra de Parras, Coahuila, Mexico" (*ibid.*, **48**, 1937, 587–630); R. W. Imlay, "Stratigraphy and Palæontology of the Upper Cretaceous Beds along the Eastern Side of Laguna de Mayran, Coahuila, Mexico" (*ibid.*, 1785–1872); T. S. Jones, "Geology of Sierra de la Peña and Palæontology of the Indidura Formation, Coahuila, Mexico" (*ibid.*, **49**, 1938, 69–150).

Professor K. Sapper, the "Father of Central American Geology," began his life-work in this difficult region in 1888. In a final memoir entitled "Mittelamerika" (*Handb. d. Reg. Geol.*, VIII, Abth. 4*a*, 1937, 160 pp.), he summarises the geology of Central

America from his own observations combined with those of other workers. C. Schuchert has written an appreciative review of Professor Sapper's farewell volume (*Amer. Journ. Sci.*, XXXIII, 1937, 394–5).

In a valuable memoir entitled "Grosstektonische Probleme des mittelamerikanischen Raumes," R. A. Sonder (*Zeitschr. f. Vulk.*, XVII, 1936, 1–33) attempts an original reconstruction of the geological lineaments, including the main tectonic lines, the mountain structures, and distribution of the volcanoes, within and bounding the Central American region.

J. Keidel and H. J. Harrington record the discovery of "Lower Carboniferous Tillites in the Pre-Cordillera of San Juan, Western Argentina" (*Geol. Mag.*, LXXV, 1938, 103–29). The series contains several glacial horizons interbedded with sediments carrying Lower Carboniferous brachiopods and lamellibranchs.

According to C. E. Wegmann in a paper "Zum Baubilde von Grönland" (*Mitt. Naturforsch. Ges. Schaffhausen*, XIII, No. 3, 1937, 15–23), the main structural elements of Greenland are (1) the Pre-Cambrian Shield; (2) the late Pre-Cambrian and early Palæozoic deposits which occur (*a*) as undisturbed plateaux in the north-west, and (*b*) as folded zones in the north and east; and (3) the younger subsidences, some of which lie within the Caledonian folded zones as "Molasse" basins.

In his memoir on "Geological Investigations in Southern Greenland. Part I—On the Structural Divisions of Southern Greenland" the same author (*Medd. on Grönland*, **113**, No. 2, 1938, 148 pp.) divides the formations into two main cycles, the Ketilidian and the Gardar. The Ketilidian began with a geosynclinal series, consisting of a lower sedimentary, and an upper volcanic group, which were folded and highly granitised by the rise of the Julianehaab Granite. After a considerable hiatus, this was followed by the Gardar, beginning with a sandstone group, and followed by a great thickness of porphyritic lavas. These were finally injected by intrusions of essexite, alkali-syenites and alkali-granites, including "Rapakiwi" granites.

L. Hawkes, in a paper on "The Age of the Rocks and Topography of Middle Northern Iceland" (*Geol. Mag.*, LXXV, 1938, 289–96), has re-examined the evidence on which Pjeturss claimed that glacial beds of Early Quaternary age occurred in this region. He finds that these deposits have been misinterpreted, that they are of fluviatile and pyroclastic origin, and are no longer to be regarded as of a late geological date. The plateau and the deep fjords of the region are therefore inferred to date from the cessation

of the early Tertiary volcanicity, and not from Glacial times as stated by Pjeturss.

INVERTEBRATE PALÆONTOLOGY. By H. DIGHTON THOMAS, M.A., Ph.D., F.G.S., British Museum (Natural History).

AN important fauna is dealt with by C. E. Resser in "Cambrian System (Restricted) of the Southern Appalachians," *Geol. Soc. Amer. Spec. Papers*, XV, 1938, pp. 1–140. It is naturally mainly concerned with the brachiopods and trilobites, and new genera and species are described.

A re-study by C. Lochman of the Cambrian faunas from Western Newfoundland shows that not only is the Upper Cambrian represented but also the late Middle Cambrian (*Journ. Paleont.*, XII, 1938, pp. 461–77). Several new species are present, as well as one new genus of trilobites and a new genus of brachiopods.

In the course of a reconnaissance in eastern Yunnan, China, a series of shales were discovered which yield a rich fauna. Its examination by T. H. Yin shows that this "Devonian Fauna of the Pochiao Shale of Eastern Yunnan" (*Bull. Geol. Soc. China*, XVIII, (1), 1938, pp. 33–66) is the same as the *Spirifer tonkinensis* fauna of Indochina, and leads the author to consider it as equivalent to the *cultrijugatus* zone of Central Europe.

J. Shirley shows that "The Fauna of the Baton River Beds (Devonian), New Zealand" is more advanced than those of the Silurian shelly facies (*Quart. Journ. Geol. Soc. London*, XCIV, 1938, pp. 459–506). Brachiopods predominate, but trilobites, lamellibranchs, corals and bryozoa also occur. The first include species identical with, or closely allied to, those of the calcareous, Bohemian facies of the Lower Devonian of Europe and Asia, a few species of Rhenish affinities, and at least two forms identical with species from the Lower Devonian of North America. There is no suggestion of any connection with the austral Lower Devonian of South Africa and South America.

The "New Devonian Fossils from Calhoun County, Illinois," described by G. A. Cooper and P. E. Cloud (*Journ. Paleont.*, XII, 1938, pp. 444–60), consist of forms unlike those from any other Illinois locality, but with close affinities with the upper part of the Tully Limestone of New York. The fauna is mainly of brachiopods, of which 15 species are new, but other groups are also represented.

"La Faune des Couches de passage du Dinantien au Namurien dans le synclinorium de Dinant" is described by F. Demanet in *Mém. Mus. Roy. Hist. Nat. Belg.*, LXXXIV, 1938, pp. 1–201. Brachiopoda and mollusca constitute the bulk of the fauna, but

radiolaria, polyzoa, and some other groups also are represented. As a result of the analysis of this fauna the author is led to support the view that the German sub-zones IIIγ, IIIβ, and IIIα are the equivalents of the English sub-zones D_3–P_2, D_2–P_1, and D_1 respectively.

A number of authors have contributed to the report "On a Second Collection of Fossils and Rocks from Kenya made by Miss M. McKinnon Wood," *Monog. Geol. Dept. Hunterian Mus. Glasgow Univ.*, V, 1938. Dr. J. Weir (pp. 12–14) confirms the previous record of *Palæoanodonta fischeri* in the area, and also figures specimens of the same genus from Burma. He also deals (pp. 17–81) with the Jurassic brachiopods and molluscs and some Neogene molluscs. Its rich lamellibranch fauna, of European type, confirms the Upper Bajocian age of part of the Kambe Series, the rest of which is Bathonian. A few echinoids are described by Miss E. D. Currie, some of which are new; while the late Professor J. W. Gregory describes the corals. The Neogene foraminifera are dealt with by C. D. Ovey, who produces evidence to merge *Operculinella* and *Operculinoides* into *Operculina*, and *Operculina complanata* (Defrance) into *O. venosa* (Fichtel and Moll).

Vol. XCI (for 1937), 1938, of *Monog. Palæont. Soc. London*, contains Part IV of "The Ammonites of the English Corallian Beds," by W. J. Arkell, who deals with those of the *plicatilis* zone; Part XI of "The British Cambrian Trilobites," by P. Lake; and Part I of "The Carboniferous Rugose Corals of Scotland," by D. Hill. The last author is able to recognise three recurrent facies faunas. Her analysis of the Clisiophyllid corals shows that many "genera" and "species" are unnecessary.

"The Upper Palæozoic Foraminifera of the Samara Bend and the Trans-Volga Region" are described by D. Rauser-Cernoussova in *Trav. Inst. Geol. Acad. Sci. U.R.S.S.*, VII, 1938, pp. 69–167. They were collected from the sequence of the whole of the Carboniferous and the lower part of the Lower Permian, are mainly fusulinids (39 out of 43 species), and include many new forms. The species are shown to be restricted to definite horizons, and a number of faunas are recognised throughout the sequence under discussion, although the work does not admit of any solution of the problem of the stratigraphic division between the Upper Carboniferous and the Lower Permian.

The same author discusses "The Stratigraphical Importance of the group *Pseudofusulina uralica* (Krotow)," *ibid.*, pp. 169–96. The material is from the *Schwagerina* horizon of the region of the Urals, and includes new forms. Its analysis shows that where

the fullest sequence of that horizon is developed three palæontological stages can be distinguished : (1) a lower stage with *Triticites* and *Fusulinella usvae* Dutk., (2) a middle stage with *Pseudofusulina uralica* Krotow and *Schwagerina princeps* Ehrenberg, and (3) a higher one with *Pseudofusulina lutugini* Schellwien. The species occur both in the limestone facies of the " Upper Carboniferous " and in the " Artinskian " deposits, so that the author considers those horizons to be synchronous.

Some impure limestone nodules in the Balclatchie Shales at Laggan Burn, near Girvan, Ayrshire, have yielded specimens of *Cryptograptus tricornis* in such a good state of preservation that the development of the proximal end of the species has been described by O. M. B. Bulman (*Geol. Mag.*, LXXV, 1938, pp. 539–543). Two thin, diverging chitinous rods represent the sicula, and are covered at the apex by a thin, apparently structureless membrane. Theca 1^1 and th 1^2 (which seems to bud early from it) grow first downwards to the level of the sicular aperture, and then turn till they are inclined slightly upwards, and open on opposite sides a little above that aperture. The origin of th 2^1 and 2^2 is a distinctive feature, and quite unlike that of any yet described Diplograptid. They develop from, and at an early stage of growth of, th 1^1 and th 1^2, respectively, and each, in growing spirally round the sicula, changes its direction of growth in the vertical plane markedly. They open above th 1^1 and th 1^2 on the reverse and obverse sides, respectively, and, in addition, in the proximal part, at least, the two stipes composing the biserial rhabdosome lie side by side instead of in the normal back to back orientation. This structure of the proximal end in the genotype species of *Cryptograptus* justifies the separation of the genus, possibly along with *Nanograptus*, in the family Cryptograptidae. The peculiar development of *Oncograptus* may indicate a relationship to *Cryptograptus*.

For a full and up-to-date account of all aspects of the dendroid and true graptolites, reference should be made to the excellent summary given by the same author in " Graptolithina," *Handbuch der Paläozoologie*, IID, 1938, pp. D1–D92.

E. A. Ripper, in " Notes on the Middle Palæozoic Stromatoporoid Faunas of Victoria " (*Proc. Roy. Soc. Victoria*, L, (2), new ser., 1938, pp. 221–43), discusses the known species of *Actinostroma*, *Clathrodictyon*, *Syringostroma*, and *Stromatopora*, and groups them according to their skeletal structure. This, together with their stratigraphical distribution, indicates that the most important progressive, evolutional changes, are the thickening of the laminæ and increasing definition of the pillars in *Actinostroma*, the increasing

reticulation of the mesh in *Syringostroma* and *Stromatopora*, and the straightening of the laminæ and the separation of the radial pillars as distinct skeletal elements in *Clathrodictyon*. With these as criteria, Miss Ripper concludes that the Victorian faunas are probably of Middle Devonian age, with the exception of the Yeringian faunas of the Loyola Limestone, which has Silurian affinities, and of the Lilydale Limestone, which contains a high percentage of forms characteristic of the Lower Devonian.

So far little attention has been paid to the coral fauna of the Devonian of Bohemia, especially to that of the Middle Devonian; but F. Prantl partly remedies this in his study of "Some Laccophyllidæ from the Middle Devonian of Bohemia," *Ann. Mag. Nat. Hist.*, (11), II, 1938, pp. 18–41. The family is represented by only three genera, but by numerous species which occur in abundance.

The "Upper Middle Devonian Rugose Corals of the Nevada Limestone," described by E. C. Stumm (*Journ. Paleont.*, XII, 1938, pp. 478–85), come from levels between 500 feet and 2400 feet above the base of that limestone. In the lower 1000 feet of the succession dealt with in the paper, there is a successive series of coral reefs, but in the higher part only one coral zone is as yet known. The corals show a marked resemblance to those of Hamilton age in eastern North America and the Mississippi Valley, although there is a total absence of the heliophylloid forms so characteristic of the typical Hamilton rugose coral fauna.

D. Hill and L. B. Smyth write "On the Identity of *Monilopora* Nicholson and Etheridge, 1879, with *Cladochonus* McCoy, 1847," in *Proc. Roy. Irish Acad.*, XLV, B, (6), 1938, pp. 125–38, after making a study of the types. The corals usually occur as fragments, either as free, erect, zigzag branches usually referred to *Cladochonus*, or as reptant coils usually recorded as *Monilopora*. The former are the distal portions of colonies whose proximal ends are reptant rings. The authors discuss the systematic position of *Cladochonus*, and conclude that it probably belongs to a separate sub-order of the Zoantharia. It possesses no tabulæ, while the sclerenchyme is reticulate, a condition not known in any other genus.

M. Mirchink, in "Corals from the Jurassic Beds of the Environs of Koktebel in the Crimea," *Bull. Soc. Nat. Moscow, Sect. Géol.*, n.s., XLV, 1937, pp. 62–80, identifies 29 species, of which 15 occur in the Swiss Sequanian, thus supporting a Sequanian age for the Crimean coralliferous rocks considered.

"Celenterati, Echinodermi e Brachiopodi del Cretaceo medio-superiore dello Zululand" are the subject of a report by E. Montanaro-Gallitelli and Z. Lang in *Palæont. ital.*, XXXVII, 1937,

pp. 193–210. Though the corals are few in number they are important because of their rarity in the South African Cretaceous. The fauna includes one new species of *Hemiaster* and a new species of *Terebratula*.

An important contribution to our knowledge of early faunas is the study of "Ozarkian and Canadian Brachiopoda" made by E. O. Ulrich and G. A. Cooper, *Geol. Soc. Amer. Spec. Papers*, XIII, 1938, pp. 1–323. Homœomorphy in shell shape is shown to be a common feature, though the internal characters are sufficiently distinct and persistent to be of critical value. The shell structure is usually too poorly preserved to be of use. Tentative suggestions are made as to evolution in the group. Thus, *Eoorthis* may be related to *Orthis, sensu stricto*, although it is possible that *Nanorthis*, the earliest of the Orthidæ, gave rise to *Archaeorthis* and to *Orthis*.

The "New Caradocian Brachiopods from the Berwyn Hills, North Wales," which are described by H. B. Whittington in *Ann. Mag. Nat. Hist.*, (11), II, 1938, pp. 241–59, are all preserved as casts, so that direct observation on shell structure is precluded, but the internal structures can mostly be interpreted. They include new species and varieties, and are often of value as zonal indices, not only within the area considered, but also frequently over a wider geographical range.

J. K. S. St. Joseph deals with "The Pentameracea of the Oslo Region, being a Description of the Kiær Collection of Pentamerids," in *Norsk Geol. Foren.*, XVII (1937), 1938, pp. 225–336. The Pentamerids are abundant in the Norwegian Silurian, although few genera and species are represented. The material, however, is well preserved so that the characters of the interiors, as well as generally of the exteriors, can be studied by serial-sectioning, transfers, and the construction of enlarged scale-models. The pseudodeltidium is composed of shell-substance continuous with that of the thin layer on the inside of the spondylium, while there is no deltidium composed of fused deltidial plates. The spondylium duplex characterises the ventral valves of the Pentamerids. It is composed of a pair of plates, which are in contact ventrally, where they form a duplex septum attached to the valve-wall, while dorsally they separate to build the spoon-shaped spondylium. The length of the septum and the length and shape of the spondylium vary considerably in different genera. The dentition is very degenerate, and the function of articulation must have devolved almost wholly on the muscles. Typically the cardinalia consist of a pair of long crural plates (arising almost at right angles to the valve floor), beyond which there may project the brachial-processes. The crural

plates frequently have a three-fold division into inner-plates, bases of the brachial-processes, and outer-plates. The cardinalia of *Clorinda*, *Holorhynchus* and *Stricklandia* depart markedly from those of the typical genera, and indicate that the last-named genus did not give rise to *Pentamerus*, as has been suggested.

D. Nalivkin monographs the " Brachiopoda of the Upper and Middle Devonian and Lower Carboniferous of North-Eastern Kazakhotan " in *Trans. Central Geol. Prosp. Inst.*, XCIX, 1937, pp. 1–200. The fauna shows that, in the area dealt with, stages from the Givetian up to the Viséan may be recognised. A great many new species are described, particularly among the Productidæ and the Spiriferidæ.

A. H. Sutton examines the " Taxonomy of Mississippian Productidæ," *Journ. Paleont.*, XII, 1938, pp. 537–69, and, using, as criteria, both external and internal characters, recognises 2 subfamilies, 16 genera, 74 species, and 2 varieties. His valuable review also includes an attempt to establish the course of evolution within the family.

B. Licharew writes " Sur les Brachiopodes du Carbonifère Supérieur du bassin du Donetz " in *Journ. Geol. Acad. Sci. Ukrain. S.S.R.*, V, (3), 1938, pp. 73–139. The spiriferids and productids from Avilova correspond to the horizons C3*a* and C3*b* of Samara, while the brachiopods of the lower part of the series with *Araucarites* have an Upper Carboniferous character, and do not appear to be contemporary with the *Pseudoschwagerina* horizon of the Russian Platform. Some of the productids are new.

M. Mirchink describes Upper Permian brachiopods in " Materials for the Study of the Pinega Zechstein Brachiopoda," *Trav. Inst. Geol. Acad. Sci. U.R.S.S.*, VII, 1938, pp. 313–43. The productids are described in detail. They bear practically no similarity to those of the south-eastern part of the U.S.S.R., nor with Western Europe, but with the Lower Permian fauna of the arctic region. The author concludes that there was a close connection of the Pinega Zechstein basin with the open arctic Upper Permian Sea.

H. M. Muir-Wood discusses a little-known group of British fossils in " Notes on British Eocene and Pliocene Terebratulas," *Ann. Mag. Nat. Hist.*, (11), II, 1938, pp. 154–81, and shows that several species can be recognised among the latter, which include a number of giant forms. The separation is effected by the character of the interiors. The British Tertiary Terebratulas are readily distinguished from Recent species of *Gryphus* and *Liothyrella* by the development of inner and outer hinge-plates separated by crural bases in the fossil forms.

F. Prantl describes "Lower Turonian Bryozoa from Předboj (Bohemia)" in *Rozpr. Stát. Geol. Ustav. Ceskoslov. Repub.*, VIII, 1938, pp. 1–71. The Cyclostomata predominate considerably over the Cheilostomata. Thirty-seven species are identified, of which the larger number are forms with an erect, dendroid, ramous, or pedunculated zoarium. This is to be correlated with rapid sedimentation of the terrigenous material, a conclusion supported by the rarity of the occurrence of unilamellar, incrusting forms with the ramous forms. Among the species which occur are many which are also present in the Cretaceous of Western France and Saxony; several of those in the former area are characteristic of the French Cenomanian, although at Předboj, where that horizon is missing, they are to be found in the Lower Turonian.

The "Late Paleozoic Pelecypods: Pectinacea" are described in a valuable memoir by N. D. Newell, *State Geol. Surv. Kansas*, X, 1937, pp. 1–123. He has found that four characters, which are discussed in detail, can be employed consistently in their classification, namely, (1) the Musculature, in which two types are distinguished; (2) the Hinge, in which four types can be recognised; (3) the Ornamentation; and (4) the Shell microstructure. The author considers that the pteriid ligament is a natural derivative of the arcid ligament, and that *Pernopecten* is ancestral to the modern *Amussiums* and not to the modern pectinoids, which are probably descended from the *Aviculopectens*. After dealing with the systematics of the American species, Dr. Newell gives a very useful list which revises the generic status of most of the extra-American, late Palæozoic species.

"The Lower Triassic Pelecypoda of the Ussuriland," which are described by L. Kiparisova (*Trav. Inst. Geol. Acad. Sci. U.R.S.S.*, VII, 1938, pp. 197–311), belong to 10 families—of the 52 species described, 19 are new. The fauna is identical with, or allied to, that of the Lower Triassic of the Mediterranean Province, and seems to indicate that the whole of the Lower Triassic is developed in the area considered.

F. S. MacNeil describes "Species and Genera of Tertiary Noetinæ" in *U.S. Geol. Surv. Prof. Paper*, CLXXX A, 1938, pp. 1–49. A re-definition of the sub-family is given, based on the critical morphology and developmental trends of the type genus, and, as a result, this includes some forms not previously regarded as Noetinæ and excludes certain genera previously referred to it. Two groups can be distinguished by the character of the sculpture, ligament, and crenulations: the one characteristic of the Atlantic, and the other of the Pacific. In the former, which reached its maximum

in the Pliocene, geographic speciation and later hybridisation both seem to have occurred. The latter is represented now by only one restricted species, though it was particularly abundant in the Miocene.

W. P. Woodring discusses the "Lower Pliocene Mollusks and Echinoids from the Los Angeles Basin, California, and their inferred Environment," in *U.S. Geol. Surv. Prof. Paper*, CXC, 1938, pp. 1–67, and deals with 26 forms of molluscs and one echinoid. The fossils are assigned to three depth-range groups on the assumption that they represent essentially the same depth-range as the modern species of the Eastern Pacific to which they are closely allied. Those of deep-water facies are widely distributed throughout the basin, but those of shallow-water facies are so far known only near the northern and western margins. The occurrence of mixed facies-faunas and of land fossils in the northern part of the basin suggests proximity to land in that region.

In "Stratigraphy and Mollusca of the Eocene of Western Oregon," *Geol. Soc. Amer. Spec. Papers*, X, 1938, pp. 1–130, F. E. Turner deals with a large suite of lamellibranchs and gastropods. If the Eugene formation at the top of the sequence be excluded, then two distinct marine molluscan faunas can be recognised. The lower, which occurs in the Tyee and Umpqua formations, has much in common with that of the Capay stage of California, while the upper fauna, found in the Coaledo and Spencer formations, has numerous species characteristic of the Tejon formation of California, and the Cowlitz formation of Washington.

The Javan Pliocene deposits have recently received much attention, and among other results are those of C. H. Oostingh who has described "Die Mollusken des Pliocäns von Süd-Bantam in Java. 1. Abschnitt (Gastropoda I)," *De Ingenieur in Nederland-Indie*, IV, (2), 1938, pp. 17–60, and (7), 1938, pp. 105–15. The author deals with 98 species and sub-species, and compares this gastropod fauna with that of Benkoelen in Sumatra.

In "Zur Kenntnis der arktischen Belemniten von König-Karls-Land," *Zentralbl. Min., Geol., Paläont.*, B, 1938, pp. 19–28, E. Stolley discusses the identity and occurrence of the belemnites of that region, and concludes that they point to a high Upper Jurassic or Lower Neocomian age for the deposits.

The "Devonian Ammonoids of America" have been monographed by A. K. Miller, *Geol. Soc. Amer. Spec. Papers*, XIV, 1938, pp. 1–262. They were widely distributed, and closely similar, if not identical, species occur in both the eastern and western hemispheres. The author is of the opinion that all the ammonoids

developed from *Eobactrites*, that the clymenids became extinct at the close of the Devonian, and that the later ammonoids developed from the extra-siphonate goniatites and not from the clymenids. He holds, in fact, that all the later ammonoids developed from the Anarcestidæ and not the Agoniatitidæ.

A. K. Miller and C. A. Moore write on "Cephalopods from the Carboniferous Morrow Group of Northern Arkansas and Oklahoma," in *Journ. Paleont.*, XII, 1938, pp. 341–54. Several genera and species are described in detail, and, as a result, the authors conclude that, though the Morrow cephalopods are related to both Mississippian and Pennsylvanian forms, they are more like the latter than the former.

BOTANY. By Professor E. J. Salisbury, D.Sc., F.R.S., University College, London.

The subject of oxidation-reduction potentials of soils has been studied by a number of investigators since Gillespie published his observations in 1920 in *Soil Science*. The principles involved were summarised in 1934 by Brown (*Soil Science*, **44**, 65–76, 1934) and in the October number of the same Journal (**46**, 323–30, 1938) Puri and Sarup report investigations in which the relations between soil reaction and oxidation-reduction potentials were studied in single-base soils. The relationship was found to be linear for all soils, irrespective of the alkali used for titration. They conclude that the relation between pH and Eh is so perfect that the measurement of pH should suffice for all practical purposes. If these fundamental conclusions are valid, and many field determinations appear to support them, one must conclude that the instances where discrepancies appear are the outcome of either faulty experimentation, or interpretation, of the results obtained.

The root systems of some desert plants have been studied by M. Evernari (*Jour. Linn. Soc. Bot.*, LI, 383, 1938), who finds that the succulent desert perennials, such as *Suæda asphaltica* and *Zygophyllum dumosum*, have very superficial but widely spreading roots, those of the latter, for instance, extending up to 400 cm. from the axis. *Retama Retam* though possessing a main root penetrating to a depth of about a metre, has superficial horizontal roots produced at a depth of about 5 to 10 cm., which extend radially for some 3 to 5 metres. The roots of the annuals penetrate only 5 to 7 cm. and do not exhibit lateral extension.

The root systems of Heath plants is the subject of a paper by G. H. Heath and L. C. Luckwell (*Journal of Ecology*, XXVI, 331, 1938). The shrubby species represented by *Calluna vulgaris*, *Erica*

cinerea, *E. tetralix*, and *Vaccinium myrtillus*, have maximum rooting depths ranging from 17 to 23 cm., though most of the roots occupy the first 10 cm. *Ulex Gallii* has a maximum root penetration of 76 cm. with an average of 15 cm. The perennial herbs, such as *Galium saxatile*, *Potentilla erecta*, and *Polygala serpyllacea*, have a shallow root system occupying an average depth of from 2 to 8 cm. only, though with a maximum penetration of from 10 to 17 cm. *Polygala serpyllacea* showed a definite negative correlation between water content of the soil and depth of penetration. In soil with an average water content of 74 per cent. the maximum depth to which the roots extended was 16·5 cm., whilst in a soil with a water content of 256 per cent. the maximum penetration was only 10 cm., although the lateral spread was greater in the latter than the former. The Monocotyledonous herbs, such as *Narthecium ossifragum*, *Scirpus cæspitosus*, and *Nardus stricta*, had root systems extending to depths of from 25 to 28 cm.

The photosynthesis of Strand and Dune plants is the subject of a paper by Alfons Beiler (*Jahrb. f. Wiss. Bot.*, **87**, 1938). His results indicate a much higher rate of photosynthetic activity for unit surface in the dune succulents, such as *Cakile maritima* and *Salsola kali*, than for dune species such as *Eryngium maritimum*, *Elymus arenarius* and *Psamma arenaria*.

In a series of papers in the *Annals of the Natal Museum*, IX, 1938, Prof. Stephenson *et al.* describe the plant and animal communities of the intertidal zone in South Africa. Both flora and fauna exhibit a striking association of Cape species and others of tropical coral reefs. Denudation experiments indicated a definite algal succession in which the pioneers are probably diatoms, whilst the equilibrium finally reached was similar to that of the community before denudation.

L. Leven records the occurrence in Allium roots, treated with growth-promoting substances, of large cells in the cortex, some of which contain double the normal somatic number of chromosomes. Although cell division takes place in these large cortical cells the chromosome doubling appears to take place in the so-called resting stage of the nucleus. Further away from the root apex than the region where the cells were found containing 32 in place of the usual 16 somatic chromosomes, cells were observed in which the number of chromosomes was 64 (*Hereditas*, XXV, 87, 1938).

In the same journal (*Hereditas*, XXV, 33) A. Gustafsson deals with the subject of polyploidy in the Blackberries. It appears that the southern *Rubus ulmifolius* is diploid, but nevertheless consists of a number of closely related forms. In this genus there

appears to be no correlation between the taxonomic value of species and their chromosome numbers. It is apparently only in the subgenus Eubatus that apomictic species occur and all these apomictic species are either triploid, tetraploid, or pentaploid. Amongst the investigated species of this section the tetraploids preponderate to the extent of constituting 82 per cent. The *Rubi Corylifolii* are tetraploid, pentaploid, hexaploid, superhexaploid, and heptaploid. The author calls attention to the fact that several eastern species exhibit higher chromosome numbers in the more western part of their range, a fact attributed to hybridisation with western types. It may, however, be pointed out in connection with the last-named feature that climatic elimination in the more severe climatic extremes is not improbable, since Schlosser has shown that tetraploid plants of some species are less frost-hardy than diploid individuals of the same species.

PLANT PHYSIOLOGY. By PROFESSOR WALTER STILES, Sc.D., F.R.S., The University, Birmingham.

PLANT HORMONES.—During the last two years much activity has been shown in the investigation of the action of auxin and other plant hormones. Since Kögl and Haagen-Smit isolated and determined the chemical constitution of auxin a, auxin b and heteroauxin a number of substances of related constitution have been found to possess similar physiological properties. Now J. B. Koepfli, K. V. Thimann and F. W. Went ("Phytohormones: structure and physiological activity. I," *Journ. Biol. Chem.*, **122,** 763–80, 1938) have investigated the question of the relationship between chemical constitution and auxin-like activity, and conclude that such substances must possess the following characters, namely, a ring system, a double bond in the ring, a side chain, a carboxyl group, or one readily convertible to a carboxyl, in this side chain separated by at least one carbon atom from the ring, and a definite space relationship between the ring and the carboxyl group so that the distance between them falls within a certain limited range.

While an auxin is essential for the growth of the coleoptile and while auxins may affect roots, a quite different substance is necessary for the growth of roots. Thus P. R. White ("Seasonal fluctuations in the growth rates of excised tomato root tips," *Plant Physiol.*, **12,** 183–90, 1937) has succeeded in growing excised tomato roots for four years in an artificial nutrient medium containing a small quantity of yeast extract in addition to sucrose and inorganic salts, while J. Bonner and F. Addicott ("Cultivation in vitro of excised pea roots," *Bot. Gaz.*, **99,** 144–70, 1937) have similarly

grown pea roots on such a medium for four months. Presumably a constituent of the yeast extract functions as a growth hormone for the roots, and the latter workers report that crystalline vitamin B_1 is capable of partially replacing the yeast extract though it will not permit optimal growth for very long. However, with the addition of sixteen pure crystalline amino-acids to the vitamin B_1 the excised roots grew as well as with the yeast extract and Bonner and Addicott therefore regard vitamin B_1 as a plant hormone effective for root growth. While P. R. White (" Amino-acids in the nutrition of excised tomato roots," *Plant Physiol.*, **12**, 793–802, 1937) also found the addition of amino-acids to the culture medium exercised a beneficial effect on the growth of excised tomato roots, this is disputed by W. J. Robbins and M. B. Schmidt (" Growth of excised roots of the tomato," *Bot. Gaz.*, **99**, 671–728, 1938), who agree, however, that vitamin B_1 or its intermediate thiazole is essential for the growth of tomato roots, and that this is the constituent of yeast extract responsible for the effect of the latter on root growth. They observed an effect with a quantity of vitamin B_1 as small as 10^{-9} mg., corresponding to a concentration in the nutrient solution of 1 in 4×10^{13}.

It has been reported by W. Davies, G. A. Atkins and P. C. B. Hudson (" The effect of ascorbic acid and certain indole derivatives on the regeneration and germination of plants," *Ann. Bot.*, N.S., **1**, 329–52, 1937) that vitamin C (ascorbic acid) accelerates the growth of willow branches and, in low concentrations, furthers the germination and subsequent growth of oats, mustard and cress while it has a retarding action in high concentration. W. G. Clark (" Ascorbic acid in the Avena coleoptile," *Bot. Gaz.*, **99**, 116–24, 1937) has found ascorbic acid present in the oat coleoptile in considerable quantity, it being synthesised there from a precursor in the seed, the vitamin itself not being present in the germinating seed. Clark tested the activity of ascorbic acid as a growth hormone with the *Avena* coleoptile, but it was found not to be a cell elongation hormone like auxin nor to further the action of auxin in the *Avena* coleoptile. According to J. and D. Bonner (" Ascorbic acid and the growth of plant embryos," *Proc. Nat. Acad. Sci.*, **24**, 70–5, 1938), plants respond to the addition of ascorbic acid if they do not synthesise this substance in adequate amount. Thus these authors cultivated on artificial media the embryos of a number of varieties of pea and found the ascorbic acid content varied considerably in the different varieties. The response to the addition of ascorbic acid was roughly inversely as the content of this substance, whence it may be concluded that lack of response indicates

that the embryo already contains sufficient ascorbic acid for its own requirements.

An examination of what is known as the "pea test" for auxin has been made by J. van Overbeek and F. W. Went ("Mechanism and quantitative application of the pea test," *Bot. Gaz.*, **99**, 22–41, 1937). The stem of a pea seedling is split longitudinally and immersed in a solution of auxin, when the two halves curve inwards. According to these authors this curvature results from the more rapid growth of the epidermal surface as compared with the wounded surface, this differential growth being itself due to the fact that auxin can enter through the epidermis, but not through the wounded cells. The curvature is said to be proportional to the logarithm of the auxin concentration and so the test can be used for the quantitative determination of auxin. This theory of the origin of the curvature is called in question by K. V. Thimann and C. L. Schneider ("Differential growth in plant tissues," *Amer. Journ. Bot.*, **25**, 627–41, 1938), who show in several ways that auxin can enter plant tissues through cut surfaces at least as readily as through the epidermis, and that, as would indeed be expected, the initial rate of entry may be greater through a cut surface.

C. L. Schneider and F. W. Went ("A photokymograph for the analysis of the Avena test," *Bot. Gaz.*, **99**, 470–96, 1938) describe an automatic photographic recorder for examining the process of the curvature when the decapitated oat coleoptile is subjected to the excentric application of auxin to the top of the decapitated stump. By means of this apparatus it was shown that the curvature begins about 20 minutes after the application of the auxin and continues at a constant rate for about an hour, after which the curvature diminishes or even becomes reversed. With increase of auxin concentration inside the coleoptile, sensitivity to externally applied auxin diminishes. The interval allowed to occur between decapitation and application of auxin affects the sensitivity of the coleoptile to auxin. Thus for all concentrations of auxin up to the concentration producing the maximum curvature, increasing this interval up to 30 minutes brought about a rapid increase in sensitivity, a more gradual increase up to 200 to 240 minutes, and then, with further increase in the period, a gradual reduction in sensitivity. If the period is longer than 100 minutes a second decapitation immediately before applying the auxin produces a slight reduction in the reaction with low concentrations of auxin, but doubles the reaction with maximum angle concentration. The second decapitation results in more uniform reactions. From the results of this investigation the authors propose a modified *Avena*

test for auxin which should give greater precision. This involves a period of 3 to 4 hours between decapitation and application of the auxin, a second decapitation 20 to 40 minutes before the application of auxin, and photographs of the coleoptile 90 minutes after this application.

Among recent papers dealing with the effects of growth-promoting substances on plants may be mentioned one by J. W. Mitchell and W. E. Martin (" Effect of indolylacetic acid on growth and chemical composition of etiolated bean plants," *Bot. Gaz.*, **99**, 171–83, 1937), wherein it is recorded that application of 3 per cent. β-indolylacetic acid lanolin mixture to the first internode of etiolated bean seedlings induced " gall " formation at the point of application, the development of roots in the " galls," and in many cases the formation of dense rows of roots along the entire length of the hypocotyls. However, the first and second internodes and the leaves of treated plants increased less in size than those of untreated plants. Transport of food material from the cotyledons, and water intake, were both retarded by the treatment, and materials were only conducted upwards as far as the point of application of the indolylacetic acid.

In another paper, J. W. Mitchell and C. L. Hamner (" Stimulating effect of beta (3) indolylacetic acid on synthesis of solid matter by bean plants," *Bot. Gaz.*, **99**, 569–83, 1938) record the results of treating the cut surface of decapitated bean stems with β-indolylacetic acid lanolin preparations. When these latter contained more than 0·00185 per cent. indolylacetic acid the development of axillary buds was retarded, tumours and roots were produced at the end of the stem and there was increase in both fresh and dry weight of the parts of the plant near the point of application of the auxin as compared with untreated controls. With lower concentrations of the auxin, the inhibitory action on axillary bud development was less, as was also tumour formation, while there was no abnormal root development.

Working with stem cuttings of Eureka lemon and Delicious apple treated at the apex or base with indolylacetic acid, W. C. Cooper (" Hormones and root formation," *Bot. Gaz.*, **99**, 599–614, 1938) found that although there was little difference in the amount of auxin which could be recovered from treated lemon and apple cuttings, yet the former produced roots while the latter did not, and it was therefore concluded that the apple cuttings do not contain certain substances necessary for root formation. The author is of the opinion that in the lemon the leaves supply one substance necessary for the differentiation of the root primordia and another for the

outgrowth of the latter. Indolylacetic acid induces the rapid transport of the former substance to the base of the cutting, while the latter is transported slowly. It is concluded that the action of indolylacetic acid in root formation lies in its mobilisation of root-forming substances occurring naturally in the plant.

B. F. Harrison (" Histological responses of Iresine lindenii to indolylacetic acid," *Bot. Gaz.*, **99**, 301–38, 1937) finds that the rooting of stem cuttings of *Iresine lindenii* is accelerated by brief treatment with weak aqueous solutions of indolylacetic acid. Application of 3 per cent. indolylacetic acid in lanolin to the first and third internodes results in the formation of tumours and numerous adventitious roots at the place of application of the auxin. A detailed account is given illustrated by numerous photomicrographs of the histology of the tissues affected by treatment with auxin.

K. V. Thimann (" On the nature of inhibitions caused by auxin," *Amer. Journ. Bot.*, **24**, 407–12, 1937), from measurements of different parts of treated and untreated plants, finds that the inhibition of axillary bud development in *Pisum* and of root development in *Avena* as a result of treatment with auxin is not accompanied by any increase in growth elsewhere in the plant and in each case involves an actual loss in the total dry weight of the plant. Very low concentrations of auxin further root elongation and it is suggested that in all cases, with roots, stems and buds, growth is furthered by relatively low, and retarded by relatively high, concentrations of auxin, so that there should be an optimum value of auxin concentration for growth in each case.

Finally reference may be made to the effect of auxins in producing parthenocarpic fruits. This effect was obtained by F. G. Gustafson (" Inducement of fruit development by growth-promoting chemicals," *Proc. Nat. Acad. Sci.*, **22**, 628–36, 1936; " Induced parthenocarpy," *Bot. Gaz.*, **99**, 840–4, 1938) by applying a mixture of lanolin and indolylacetic acid or related substance to the stigma or cut surface of the style of tomato, *Petunia*, *Salpiglossus*, pepper and egg-plants, while P. Hagemann (" Über durch β-indolessigsäure ausgelöste Parthenocarpie der Gladiole," *Gartenbauwiss.*, **11**, 144–50, 1937) obtained parthenocarpic fruits of *Gladiolus* by a similar treatment. F. E. Gardner and P. C. Marth (" Parthenocarpic fruits induced by spraying with growth-promoting compounds," *Bot. Gaz.*, **99**, 184–95, 1937) obtained parthenocarpic fruits of the American holly (*Ilex opaca*) by spraying the open flowers with a solution of indolylacetic acid or similar substance. Histological examination of these parthenocarpic fruits by F. E. Gardner and E. J. Kraus (" Histological comparison of fruits

developing parthenocarpically and following pollination," *Bot. Gaz.*, **99**, 355–76, 1937) shows that development proceeds in the natural way without tumours or formation of root primordia, and with swelling of the ovaries through cell division and increase in the size of the cells. There is, however, no development of endosperm and no trace whatever of an embryo.

ZOOLOGY. By EMERITUS PROFESSOR W. GARSTANG, M.A., D.Sc.; E. B. FORD, M.A., B.Sc.; H. K. PUSEY, M.A., and B. W. TUCKER, M.A., The University, Oxford.

OPISTHOBRANCHIATE MOLLUSCA.—There has been a revival of interest in these highly specialised molluscs. H. H. Brown (*Trans. Roy. Soc. Edin.*, **58**, 1934, 179–210) began with a general study of the burrowing Tectibranch *Philine aperta*. In the adult stage their mode of ploughing the mud, their tubular slime-tracks, the prehensile working of the bilobed radula (1–0–1) which is appropriately compared to a Petersen "grab," and that of the calcified gizzard-plates for grinding up the food collected, as well as the processes of fertilisation and egg-laying, are all tersely and clearly described. A striking feature of the gut is the constriction from it of a gastric pouch into which the ducts of the digestive gland open. Valvular flaps and ridges of the wall guide the lighter food into the stomach and the heavier sand and mud past the gastric aperture into the intestine. The burrowing modifications include remarkable changes in the character and position of the sense-organs.

N. Millott has studied a small sponge-eating Dorid. His species is probably the familiar *Jorunna johnstoni* of Alder and Hancock, Bergh, and Eliot, but a change in the M.B.A. Fauna List, accompanied by a misprint, has misled him into calling it first *J. tormentosa* (!), and then *J. tomentosa* (Cuv.). His main paper (*Phil. Trans.*, B, **228**, 1937, 173–218) gives a good histological account of the alimentary canal, and the chief features of the feeding process. Canalisation of the buccal epithelium suggests that the chitinous lining is thickened, if not originally formed, by a process of secretion. Digestion is entirely intracellular, and limited to the cells of the digestive gland. These are held to give rise to free phagocytes, which assist in the work. Fore-gut and hind-gut are similarly ciliated, ridged, and grooved. Particles scraped up by the radula pass to the digestive gland without selection. The residues of digestion, after return to the stomach, are consolidated into fæcal pellets in the cæcum and intestine. No salivary glands were found, but gland-cells throughout the gut pour a copious mucilage into its lumen. There are some obscurities in the author's account of the working of the

radula, and it is surely a mistake to attribute "mastication" to this organ. The author might have been more explicit as to the animal's treatment of the "numerous projecting sponge-spicules" which enter the œsophagus with its food (p. 187). All we are told of their later history is that in the fæces "fragments of sponge-spicules may *sometimes* be seen" (p. 211). Do more, then, go in than come out? It is enlightening to follow the animal's reactions to iron saccharate, but after all the problem is how *Jorunna* disposes of spiculiferous sponges.

A. Graham (*Trans. Roy. Soc. Edin.*, **59**, 1938, 267–307) fully describes the alimentary canal and processes of digestion in four Coelenterate-feeding Æolids. *Eolidina alderi* shows a few peculiarities, but in general the structure is remarkably uniform. The jaws are cutting blades; the narrow, unfolded, radula *rakes* the cut pieces of the prey into the œsophagus. Excepting the œsophagus of *Facelina*, œsophagus, stomach, and the ducts of the digestive gland are lined by a ciliated epithelium, which is packed with peculiar "vacuoles," elliptical, with an internal partition. Graham shows that this type of epithelium is characteristic of all Coelenterate-feeding Æolids, and absent in the others. What he does not mention is that in the species in which he figures them the so-called vacuoles have a suggestive resemblance to distorted nematocysts, and in each case correspond closely in size to those ingested—large in *Eolidina* which feeds on the Actinian *Heliactis bellis*, and small in *Cratena* which feeds on Hydroids. Is it possible that a second chapter is opening up in the remarkable history of this group—that they not only take over the weapons of their prey, but inoculate themselves against reprisals? Although nematocysts are not digestible by Æolids, there is a possibility that their walls may be rendered more permeable, and their contents less poisonous by prolonged exposure to the digestive juices. That the 'vacuoles' serve a prophylactic function is probable from their distribution, and from the fact that they are absent from the œsophagus of *Facelina*, which, like the buccal cavity, is protected by thick cuticle.

Except for small "salivary" glands, the alimentary canal completely lacks the mucous cells so abundant in Dorids, but the prey, before being devoured, is well lubricated from the pedal gland. The digestive gland, as is well known, is differentiated into branching "ducts," which open into the stomach, and terminal "tubules," which are lodged in the cerata. Secretion of enzymes is restricted to the tubules. Their epithelium consists of two kinds of cell only—digestive and lime-holding, the former with several phases. They produce proteolytic enzymes, some of which are

shed into the lumen and pass to the ducts and stomach, where digestion, and finally absorption, take place. Ingestion of solids is limited to the digestive cells themselves, which become loaded with muscle-fragments, nematocysts, etc., after a meal of their natural food. Undigested residues are eventually expelled into the lumen, whence they pass back to the stomach and out by the anus. No absorption takes place in the intestine, but amœbocytes are numerous. Fæces are loose and dispersed by cilia.

The author concludes with a discussion of the evolution of the cnidosac and sundry questions involved in the Æolid's utilisation of the nematocysts of its prey. The lime-cells may have been a significant factor in the evolution of Æolids, since they probably control the hydrogen ion concentration in the gut, which must be kept sufficiently low to prevent premature explosions. Zooxanthellæ are also ingested with their food in certain cases (*e.g. Eolidina*). They remain undigested, and accumulate in the digestive cells.

Plumage and Hormones.—Nowikow (*Zool. Anz.*, **115**, 1936, 193–9, and *Acta Zool.*, **18**, 1937, 447–8) has extended his results in Passerines (*cf.* Science Progress, **31**, April 1937, 713) to the crossbill and chaffinch, which show the same absence of hormonic control as the sparrow and bullfinch. On the other hand, it is claimed by Witschi (*Anat. Rec.*, **67**, suppl. 1, 1936, 83) that such control does operate in the weaver-bird *Pyromelana franciscana*—an experimental result which is surprising in view of the admittedly close relationship between Fringillidæ and Ploceidæ (*cf.* below, p. 754).

J. P. Chu (*Trans. Roy. Soc. Edin.*, **59**, 1938, 533–6) has traced the sequence of plumage types from hatching to maturity, and the effects on plumage of hypo- and hyper-thyroidism in the male Brown Leghorn fowl. The sequence entails a decrease in melanin pigmentation and a tendency to develop fringes to the feathers, in which the barbs lack barbules. He confirms the view, in large measure, that this sequence reflects a progressive decrease in thyroid activity. Thyroid administration in stages 2 and 3 of juvenile plumage induced feathering similar to that of stage 1, while plumage of a juvenile type was produced in adults. No change, however, was made in the first phase of juvenile feathering, or in the first definitive plumage of the chick, so that it is uncertain whether any thyroid control is involved in the transition between these two. The experiments lend no support to the view of Torrey and Horning that thyroid-feeding induces hen-feathering.

Teeth of Monotremes.—H. L. H. H. Green (*Phil. Trans.*, B, **3**, 1937, 367–420) has made a detailed study of the development

and morphology of the teeth of *Ornithorhynchus*. The full dental formula is

$$i\tfrac{1}{5}\ c\tfrac{1}{1}\ pm\tfrac{2}{2}\ m\tfrac{3}{3},$$

but a number of these developing teeth fail to come to maturity, the usual formula, as is well known, being

$$\tfrac{3}{3}(= pm\tfrac{1}{0}m\tfrac{2}{3}).$$

It is evident that the teeth are very degenerate, so that it is difficult to derive phylogenetic conclusions from them. It seems clear, however, that they cannot be convincingly reduced to the tritubercular tuberculo-sectorial pattern. It is therefore probable that Monotremes diverged from the rest of the Mammalia before this pattern was acquired. They certainly give no support to the rather startling suggestion of Gregory (*Proc. Amer. Phil. Soc.*, **73,** 1934, 262) that Monotremes are specialised derivatives from phalangeroid Marsupials; nor is there any evidence of any fusion of enamel organs, as might be expected of so archaic a type on Bolk's theory of "dimery," according to which each mammalian tooth is the product of a fusion of two reptilian teeth.

VERTEBRATE EMBRYOLOGY.—Details of endoderm formation in birds have long been obscure, though most authors are agreed that this layer migrates forwards under the ectoblast from some source behind. In the unincubated egg of the fowl and other birds W. Jacobson (*Jour. Morph.*, **62,** 1938, two papers) has described and figured the formation of endoderm by invagination from a posterior median endoderm-plate in the ectoblast within the *area pellucida*, just in front of the *area opaca*. Cells pass in singly or in small groups at first, but later the whole surface of a small area bends in, forming an archenteric canal which opens vertically downwards into the subgerminal cavity (blastocoel). Surface cells roll inwards and downwards into the canal and migrate radially in all directions, mostly forwards, to fuse peripherally with the inner edges of the "yolk-endoderm," *i.e.* the deep layer of the *area opaca*. A complete endoderm layer is thus formed by the fifth hour of incubation.

Other findings are in the field of chemical embryology. Jacobson (*loc. cit.*) shows (as Woerdermann, 1933, did for Amphibia) that in all invaginating cells, whether producing endoderm or chordamesoderm, the glycogen content falls as the cells pass in from the ectoblast. Early cell movements can thus be analysed. The many-celled morula becomes the single-layered blastula by upward migration of the deeper cells. At the periphery these deeper cells remain as the yolk-endoderm of the *area opaca*, and are increased by the products of vertically orientated mitoses, *i.e.* by local proliferation

without loss of glycogen and not by invagination. He also shows that, as epiblast cells move in to the primitive streak and node, their lipoid (probably sterol) content rises, while it falls in the mesoderm, but not in the notochord, as they again pass laterally. There is thus a mediolateral falling gradient of lipoid content in the embryo. This is probably correlated with the organising powers of the axial mesoderm and notochord.

GENETICS.—R. A. Fisher has now reported further on his extensive studies on Dominance in Poultry (*Proc. Roy. Soc.*, *B*, **125**, 1938, 25–48). His present paper deals with the factors for Feathered feet, Rose comb, Internal pigment, and Pile. The results obtained from this work are in harmony with his theory of dominance modification by selection, and provide a further noteworthy demonstration of its operation. Th. Dobzhansky and M. L. Queal have conducted an important investigation on the genetics of isolated wild populations of *Drosophila pseudo-obscura* (*Genetics*, **23**, 1938, 239–51 and 463–84). For this work they selected colonies inhabiting mountain forests in the Death Valley, California, and concentrated on testing for genes in the third chromosome. They find that, in the wild population, 11·9 per cent. of these chromosomes carry genes which have recessive lethal effects, and another 3·1 per cent. possess semi-lethals. There is no sharp distinction between these two classes, so that no less than 15 per cent. of the third chromosomes of the wild population carry recessive factors with destructive effects. Furthermore, defining "normal" viability as that possessed by flies with two third-chromosomes of different origin, 39 per cent. of these chromosomes contain factors which reduce viability, and 2 per cent. contain factors which raise it. It is rather surprising to notice that as many as 3·5 per cent. of the third chromosomes of the wild population carry factors with visible external recessive effects. These frequencies remained approximately constant in populations from ten different localities.

M. A. Nagai and G. L. Locher have now been able to demonstrate the production in *Drosophila* of mutations by fast neutrons (*Genetics*, **23**, 1938, 179–89). The lethal mutation rate of such flies proved to be 2·6 times that of the corresponding gamma-ray controls. Under the conditions of the experiment, slow neutrons did not significantly raise the mutation-rate above that of the gamma-ray controls. A valuable advance in comparative genetics has been made by G. Gottschewski and C. C. Tan (*Genetics*, **23**, 1938, 221–38), who have demonstrated numerous homologies between the eye-colour genes of *Drosophila melanogaster* and *D. pseudo-obscura*. Their results have been obtained by transplanting optic discs from

one species to the other. M. E. Hoover has studied the cytology and genetics of inversions in *Drosophila melanogaster*, using the salivary glands in the former section of the work. The correspondence between the cytological and genetic phenomena is satisfactory throughout (*Z. indukt. Abstamm.- u. VererbLehre*, **74**, 1938, 420–34).

F. A. E. Crew and S. S. Munro have made a much-needed survey of gynandromorphism and lateral asymmetry in birds (*Proc. Roy. Soc. Edinb.*, **58**, 1938, 114–34). They have added four new instances of each. They conclude that lateral gynandromorphism in birds is always the result of an abnormal chromosome distribution, and that this may be classified as falling within three distinct types. (1) The plumage is under direct genetic control and so follows the aberrant chromosome distribution, as in the sparrow and finch type. (2) The sexually dimorphic plumage is subject to hormone regulation, as in the fowl. Consequently, the bilateral plumage type is practically impossible. (3) A nearly normal sex type of plumage occurs on one side, and intersexual plumage on the other, this occurring in the pheasant.

A. D. Buchanan Smith, O. J. Robinson, and D. M. Bryant have provided a thorough survey of the genetics of the pig (*Bibliogr. Genet.*, **12**, 1938, 1–160).

PHYSICAL ANTHROPOLOGY.

By the late L. H. DUDLEY BUXTON, M.A., D.Sc., Exeter College, Oxford.

PITHECANTHROPUS ERECTUS, the "Ape-Man" of Java, was found nearly forty years ago. Since that time extensive searches have been made in that island for further traces of this important Hominid. The excavations have produced much interesting material, but until recently no example of the same type as the original Trinil skull-cap has been found. Dr. G. H. R. Koenigswald, however, in Vol. XLI (1938) of the *Proceedings of the Koninklijke Nederlandsche Akademie van Wetenschappen te Amsterdam* reports the finding at Bapang of a second fragment. The face and the base of the skull have been lost, but there remain thirty pieces, which fortunately fit together and form the greater part of the brain case, with the exception of the right half of the frontal bone and part of the right side of the back of the skull; we have therefore more material to work on than in the Trinil calvarium, with which it shows a close resemblance. The new fragment has a similar supraorbital region, and the same flat profile of the forehead, and—a very important point—a similar low calvarial height. It is about the same length, but the breadth is greater. Dr. Koenigswald believes that this difference is only apparent, as he thinks in the light of this new evidence that the

broadest part of the Trinil calvarium is missing. Of special importance is the presence of the temporal region which is missing in the earlier specimen. The depth of mandibular fossa, a marked line of demarcation between man and the apes, shows that *Pithecanthropus* must be a hominid. The mandibular fossa forms the bearing surface, as it were, for the jaw on the cranium, and in spite of the great weight of the jaw is shallow in the apes, but deep in man. Dr. Koenigswald suggests that the depth of the fossa may be associated with speech. This is not the place for a discussion of a technical point, but it may be noted that, while it would be nice to be able to suggest on the evidence of the temporo-mandibular joint that the ape-man of Java could emit Cassandra-like utterances about the future of Man, Arthur Thomson showed long ago the difficulty of associating speech with anatomical features. Until we know about the teeth and the form of the lower jaw we are hardly in a position to speculate about how that jaw was used.

The position of the earhole (*porus acousticus externus*) has created some interest in recent years. This new fossil—the Trinil specimen was too broken—shows that it was in the position beneath the root of the zygoma which it occupies in Man and in young, but not adult, apes. Immediately behind the mandibular fossa lies a process known as the mastoid process, on to whose external surface is inserted the sternocleidomastoid muscle, which plays such an important part in the movements of the head and is a conspicuous feature of the surface anatomy of the neck, nearly always stressed by artists in their drawings of this region. In Man, especially in males, this muscle is large and is associated with a large mastoid process. In the apes, on the other hand, the process is small. Dr. Koenigswald suggests that the small size of the process in the new Java fossil may indicate that he had not yet reached a fully upright position. Finally, attention is drawn to the fact that both Java fossils have very thick bones. This character is common to them, to *Sinanthropus*, " Peking Man," and to all the more primitive forms of man, in varying degrees and is definitely a hominid rather than ape-like character. The new evidence provided by this fossil suggests that *Pithecanthropus* is more primitive than *Sinanthropus* and the most primitive hominid known. It may be said definitely to controvert the old theory which had already been generally abandoned that *Pithecanthropus* was not a hominid at all, but merely a gigantic gibbon.

Sinanthropus, apart from references in other literature, is rapidly acquiring a special literature of its own. The most recent contribution to this study is that by Dr. Weidenreich on " The ramification

of the middle meningeal artery " (*Palæontologica Sinica*, New Series D, III, 1938). Anthropologists have paid attention especially to the skeletal parts of man and immediate relations. In the case of fossil man such a limitation has been imposed upon them by the nature of the material. All we know about the soft part of fossil man is limited to the attachments for muscles and ligaments and the courses of some blood vessels and nerves. On the other hand, where the bones form cavities we can at least estimate the shape of the soft parts which were formerly housed inside the cavity. Considerable attention, for example, has been paid to a study of the light thrown by endocranial casts on the shape of the brain, but for the most parts the blood vessels, whose form is clearly visible on such casts, have been practically neglected.

Previous workers have shown that in recent man there are two main groups. In one the anterior branch is largest and is a direct continuation of the main trunk. In the other there are two branches, sometimes of the same size, but more usually the anterior branch is the largest. In *Sinanthropus* it is possible to study the principles of ramification in eleven cases. It is noticeable that the variability is not less than in modern man, but, in spite of numerous variations in detail, with but one exception the general character of these variations differs from those of modern man.

In *Sinanthropus* the artery is divided into two or even three branches, and the ramifications of all the branches, rather abundant in modern man, are markedly poor in *Sinanthropus*. In the latter the observed conditions resemble the anthropoids, and especially the gorilla, where the superior temporal ramus represents the main stem and the anterior branch or branches are only small side vessels. Neanderthal man comes rather close to the second type of man noted above, and is thus shown once more to be more advanced than *Sinanthropus*. The Java skull, on the other hand, is similar to the more advanced type of *Sinanthropus*, but the Piltdown skull is related to the more advanced types of modern man. The Swanscombe fragment, which has been previously described in these columns, also shows a distribution of blood vessels like that of modern man, very similar to that of an Eskimo examined by Dr. Weidenreich. It must not, however, be supposed that it is any argument in favour of the Swanscombe skull being Eskimo-like. On the contrary in modern man the ramifications can be shown to be independent both of the size of the skull and of the race to which the skull belongs, but it may be considered to be a definitely human character. Generally, however, the first type may be considered to be more advanced. It may well be asked what light such minute anatomical

details throw on the problem of the development of man. At least two points come out clearly. If the pattern of the blood vessels on the inner surface of the cranial vault are studied in detail we do get a separation between what are generally considered to be hominids and those fossils which are generally considered to be specimens of Man. *Pithecanthropus* and *Sinanthropus* belong to the former category, whereas the Swanscombe and Neanderthal skulls all fall into the latter. Secondly, although there has been considerable difference of opinion about the position of the Piltdown skull, opinion has rather tended to see in this fragment a hominid, not a man. If Dr. Weidenreich's conclusions can be shown to be of certain phylogenetic importance, then Piltdown must be re-examined in the light of new evidence.

Turning to modern man, anthropologists have argued for many years about the causes of platymeria and platycnemia, the flattening of the thigh and shin bones. It has generally been suggested that these conditions, which are usually associated with one another, are due to some form of posture or gait, such as the habit of squatting on the haunches, a method of sitting down common to all primitive peoples. It has further been suggested that the flattening is due to hypertrophy of certain muscles or at least to an extension of their area of attachment. One of the classical explanations was the habit of walking up, but, as modern civilized man, who is only rarely either platymeric or platycnemic and normally has rounded thigh bones, probably walks up and down hill not less than and probably a good deal more than primitive man, this explanation seemed unlikely, even though it had the blessing of Broca, the father of modern anthropology. Buxton, in the *Journal of Anatomy*, LXXIII, 1938, has called attention to the fact that if racial averages are taken there is a marked correlation between flattening in the bones of the arm with flattening of the bones of the lower limb. He suggests then that some explanation, other than gait, must be given. If a given surface is required for the attachment of certain muscles, by a simple exercise of the Calculus it will be seen that a circular form will require most bony substance, whereas a flattened form will result in an economy of material without any loss of stability, for the bones have to support the body as well as to act as levers when it is set in motion. The author suggests that this shortage in bony material is due to some deficiency in the diet of primitive peoples, but he is not able to suggest what exactly this deficiency is. There is yet another difficulty to which he draws attention, namely that, though there is high correlation between the average flattening of the bones of the upper and lower limbs, in the only series of complete

skeletons he was able to examine this correlation did not appear. They were in any case not particularly flattened and it may be that the need for economy did not extend to the whole skeleton equally. In any case we have here an attempt to solve what has for many years been a text-book matter and familiar to everyone who begins the study of physical anthropology.

Turning to studies of recent man, Gerhardt von Bonin and G. M. Morant in *Biometrika*, XXX, 1938, discuss the Indian races of the United States in a survey of previously published measurements. The authors state that their work is a modest contribution towards the solution of the following problems about the American Indians. First, are they a homogeneous people ? secondly, do they show divergencies similar to those found among the inhabitants of other continents ? They used the method of the coefficient of racial likeness and conclude that in general, as elsewhere, there are resemblances between neighbouring peoples, but the Algonkin groups, in spite of close linguistic affinity, show remarkable physical differences. Generally, in fact, on the evidence of the coefficient of racial likeness, there are more marked divergencies between the various Indian tribes in the United States than there are between comparable groups in other parts of the world. Between America and Asia there is a relationship between the Indians of San Francisco Bay and the Ainu and Japanese. The Chukchi of northern Kamchatka also show resemblances with the American Indians, notably those of the centre of the United States, but unfortunately with the present evidence available there are no Asiatic series which can be linked with the Chukchi. There are also close bonds between the western Eskimo and types in the United States. The most important points, however, that emerge are that, though there is very great diversity among the American Indians, so great that until there is much more material available a clear study of their interrelationships cannot be made, within the groups themselves the variation is of the same order as that found in a long series of Egyptian skulls taken from a single cemetery at Gizeh, dating from the twenty-sixth to thirtieth dynasties and usually taken as a standard for variation in a relatively unmixed human group. The great variation is then between tribes. It is very unfortunate that no data is available for statistical purposes from either Canada or Mexico, as the western Indians of the former country are generally considered to be closely related to tribes on the mainland of Asia, while the cultural relationships of the Mexican Indians with those of the United States raise all sorts of interesting points which it would be valuable to compare with evidence provided by physique.

NOTES

"The Medical Press and Circular" (1839–1939) [P. J.]

The last Irish editor, Dr. Roulette, has written an interesting account of the first hundred years of this well-known journal and the times through which it has passed, showing the influence it has had in the development of medical journalism, and in bringing about a more united state of the profession generally.

In 1839, at a time when there were only two other medical journals published in the British Isles, the *Dublin Medical Press* was started by Arthur Jacob, a professor of Anatomy and Physiology in the School of the Royal College of Surgeons in Ireland, "to diffuse useful knowledge and to afford others the opportunity of doing so; to preserve the respectability of the professional character; to rouse the slumbering energies of the Irish practitioners; to instil honour-principles and foster kind feelings in the breast of the student and to protect the institutions of the country against the attacks of those interested in their destruction." Jacob held his professorship for 41 years. He practised as an ophthalmic surgeon. He is best known for his discovery of the layer of rods and cones in the retina, for long known as "Jacob's membrane."

In the founding of the *Dublin Medical Press* he was associated with Maunsell, a professor of midwifery in the School of the Royal College of Surgeons. Like Jacobs he also became more interested in medical politics and journalism and conducted the *Dublin Evening Mail* up to the time of his death in 1819. While the motive of the *Dublin Medical Press* was predominantly medico-political, it was from the beginning customary to print in each number a professional paper by some writer of standing, and much space was given to letters on clinical cases and reports of scientific meetings, not only of Dublin societies but also of those of London, Paris and other countries, and was regarded as a journal which "promises to be the medium for conveying to the public abstracts of all that is interesting and new, not alone in medical science, but in every other branch of scientific inquiry."

But the chief aim of the journal was Carmichael's scheme of

medical reform—"to unite the medical profession of Ireland into a corporation upon such principles as shall constitute them into one National Faculty." Such unification of the profession as aimed at by the reformers failed, but the movement expressed a spirit in the profession in Ireland and the United Kingdom, and a number of provincial centres were organising, while the foundations of the British Medical Association were being laid as a comprehensive voluntary association through which all branches could express themselves, and prepared the way for the passing of the Medical Act in 1858.

The energy of the reformers deserves all praise, but they were intolerant of all opposition and imported personal attacks into their advocacy. Unfortunately such methods were common in those days to all kinds of discussions. It is hard to believe how such abuse could ever have been regarded as helping towards conviction.

From its foundation down to our own time, among the main interests of the *Dublin Medical Press* has been the Poor Law medical service of Ireland. Under the 1838 Act the controlling authority was the board of the Poor Law Commissioners, the interpretation of whose powers became the subject of much controversy, which was influenced by the personal hostility of the *Dublin Medical Press* to the Commissioner Mr. Nicholls and his assistant Mr. Phelan, an apothecary. The question at issue was not the desirability of adequate supervision and control, but the choice of a proper controlling body. The existing Poor Law Act was unpopular. It was an extension of the English Poor Law system and had been imposed on Ireland without any attention given to the social and economic differences between the two countries.

In 1844 the medical Press became interested in a Bill for the "Better regulation of Medical Practice throughout the United Kingdom"—a forerunner of the Medical Act of 1858. The chief point of controversy was the relative standing of apothecaries and the medical profession. The *Dublin Medical Press* opposed the apothecaries, the *Lancet* strongly supported them. In the 1844 Bill the apothecaries were excluded from professional status—this Bill was withdrawn and the following year an amended Bill recognised them.

The *Dublin Medical Press* was rarely free from controversy. It took active part in the discussion of public questions affecting the interests of the profession and when it was not engaged in these controversies (for its discussion was almost always controversial) there were smaller matters in which it had lesser controversies.

It was concerned in squabbles more or less personal in cliques and groups which after 100 years have lost much of their interest.

The offer of 6*d*. per man per month, including medicine and travelling expenses, for attendance on officers and men of the Royal Irish Constabulary not unnaturally raised discontent in the profession and this was not lessened by the remark of the Inspector-General that " the honour of serving the police was in itself nearly a sufficient reward for the services rendered." Though later reformed, the authorities of the present police force until recently have put the appointments up to Dutch auction and given them to the lowest bidder.

But though much given to controversy the *Dublin Medical Press* gave considerable space to recording advances in medicine and surgery. In the issue of January 6, 1847, Mr. J. MacDonnell, surgeon to Richmond Hospital, described how on January 1 of that year he " put to the test the surprising discovery of Dr. Jackson and Dr. Morton that the inhalation of the vapour of rectified sulphuric ether is capable of rendering a patient undergoing a surgical operation perfectly insensible to pain." He confessed himself " sanguine respecting the safety, the great utility and the manageableness of this singular agent."

To the student of social and epidemiological history of Ireland the most important event of the nineteenth century was the great famine of 1847, due to failure of the potato crop. The census commissioners of 1851 calculated that from direct and indirect results of the famine, starvation, disease and emigration the population of the country lost in the course of a few years 2½ millions or nearly one-third of the whole.

The fever hospitals were inadequate to deal with even a fraction of the numbers requiring hospital care and the " temporary fever sheds " did little more than crowd the fever-stricken patients in insanitary ill-ventilated shelters without any hospital equipment. The *Dublin Medical Press* criticised the Government authorities for their incompetence and for their ingratitude to the medical men who laboured night and day at the hourly risk of their lives and received 5*s*. a day.

Dr. Roulette has produced an interesting and useful account of a great journal which after years of controversy has passed from the troubled waters of 100 years ago into the calm of modern time. May he long remain to carry on in peace the useful functions of the important corporation which he controls.

"**Typhoid Fever on the Witwatersrand**"[1] (P. J.)

In the South African Union typhoid fever is endemic—with periodic epidemic character. Approximately 5,000 cases are notified annually, but this figure falls short of the actual morbidity rate—owing to the inadequate notification in the large rural areas occupied by the Bantu population. It has been estimated that 2 per cent. of the Bantu are chronic typhoid carriers.

W. Lewin has made an exhaustive study of typhoid in South Africa. He considers four chief aspects—*viz.* bacteriology, serological diagnosis, specific prophylaxis and specific treatment.

The bacteriological aspects included cultural characters, morphology, agglutinability, biochemical reactions, Vi antigen content of strains from acute cases and chronic carriers; the chief concern being to determine whether there is any relation between biological characters of the organism and the clinical manifestation of the disease.

242 strains were investigated, 229 from acute cases and 13 chronic carriers. In 182 cases the organism was isolated from the blood, withdrawn by veni-puncture into a sterile syringe and inoculated into ox bile or nutrient broth or both, 2 to 3 c.c. of blood into 10 c.c. of bile and 5 c.c. into 100 c.c. of broth. After 18–24 hours' incubation at 37° C., plates of MacConkey's bile salt litmus agar were inoculated from the bile and broth cultures and after a further 18–24 hours' incubation of these plates, the non-lactose fermenting colonies were picked off and subcultured on to agar slopes which were incubated at 37° C. for 12–18 hours.

Biochemical reactions of these cultures were examined and agglutination tests with antityphoid serum were performed. As soon as the organism was identified as *B. typhosus*, subcultures were made on to ascitic agar, on which medium the strains of bacilli were maintained throughout the investigation.

In all cases there was good growth on agar—more abundant on ascitic agar. The strains were plated on agar and the resulting colonies appeared "smooth"; no typical rough colonies were seen in any of the strains shortly after isolation. In morphology, staining and cultural characters the strains conformed to standard descriptions. Variations in size from cocco-bacilli to long forms 8 μ in length were common. Motility varied in different strains, the majority were actively motile and no non-motile forms were met with.

A 24-hours' broth culture of the strains was tested for agglu-

[1] By W. Lewin, M.D., B.Ch. Publication No. XLI of the South African Institute for Medical Research, Johannesburg.

tinability with serum obtained from a rabbit inoculated intravenously with *B. typhosus* H.901. In addition, a saline suspension of a 24-hour agar growth of each strain was similarly tested with the same antiserum. The organisms in the broth cultures of all the strains were agglutinated in a serum dilution of 1 in 500 after 2 hours' incubation at 52° C. The saline suspensions did not give the same constant results—21 of the 241 strains showed no agglutination after 2 hours' incubation at this temperature. Further incubation for 22 hours caused agglutination to occur in these 21 strains. This delayed agglutination could not be ascribed to the absence of H agglutinogen in the organisms as the bacilli were motile in all cases. It appears that for the identification of the organism, agglutination tests should be performed with broth cultures, and not with saline suspensions.

Felix and Pitt have found that live smooth strains of *B. typhosus*, if agglutinable with pure smooth O antiserum, kill mice if injected intraperitoneally in small doses. Highly agglutinable strains only kill in correspondingly larger doses. This phenomenon is due to an antigen which Felix and Pitt have termed the Vi antigen. Strains which contain a considerable proportion of this antigen were termed " virulent " and those in which the factor was absent were termed " non-virulent," and those strains which contained this antigen, but not in high degree, were termed " intermediate " strains. The 242 locally isolated strains of *B. typhosus* were examined for their Vi antigen content, the object being to determine the ratio of " virulent," " intermediate," and " non-virulent " strains of *B. typhosus* found on Witwatersrand, and whether there is any correlation between the virulence of a strain and the clinical course of the disease ; also to determine the nature of the strains isolated from the chronic carriers of typhoid fever.

Adopting Kaufman's three forms of *B. typhosus*, classed according to their virulence :

(1) V form, Vi antigen fully developed and therefore O inagglutinable.

(2) W form, no Vi antigen and well agglutinated by O antiserum.

(3) The V-W form, of intermediate virulence, has Vi antigen but is well agglutinated by an O antiserum, being a mixture of V and W forms.

Examination of 229 strains showed 7·86 per cent. of V forms, 90·39 per cent. V-W forms and 1·75 per cent. of W forms. This conforms with the results of other workers. It was also concluded that the identification of the form of the infecting organism is of

no prognostic value. In man the reaction of the patient is of more importance as regards the clinical result than the form of the invading organism.

H and O agglutination is unreliable as a diagnostic measure in persons previously infected with typhoid vaccine.

From experiments on the protection—prophylactic—action of different types of antityphoid serum, it is concluded that sera rich in Vi and O antibodies give as high a degree of protection against infection with live Vi-containing typhoid bacilli as does a serum rich in Vi but poor in O antibodies.

A definite protective action is also afforded against a lethal dose of such bacilli by a serum rich in O antibodies but lacking in Vi; the degree of protection in this case is less than that afforded by the former two types of serum. Further, it is concluded that a serum rich in O antibodies affords better protection against a fatal dose of live non-Vi-containing typhoid bacilli than a serum not so rich in these O antibodies, even if the latter serum contains Vi antibodies. There also appears to be a direct relationship between the quantity of O antibodies in a non-Vi-containing serum and its protecting power against infection with live bacilli.

Therapeutic vaccination—combined serum—vaccine therapy is considered, but the results are not encouraging.

The vaccine used was Grasset's typhoid endotoxoid vaccine, prepared at the South African Institute of Medical Research. Of 33 patients (7 Europeans and 26 non-Europeans) who were given vaccine—among the 26 non-Europeans there was an early drop in temperature in 7 cases, and in the 7 European cases the drop in temperature could be ascribed to the effects of the vaccine in only one case.

Miscellanea.

The New Year Honours list included the following names well known in scientific circles: *O.M.:* Sir James Jeans. *G.B.E.:* Sir Frank Smith, Secretary of the Department of Scientific and Industrial Research. *Knights:* Prof. W. W. Jameson, dean and professor of public health, London School of Hygiene and Tropical Medicine; Prof. R. Robinson, Waynflete professor of chemistry in the University of Oxford; Prof. R. G. Stapledon, professor of agricultural botany, University College of Wales. *C.M.G.:* Mr. J. N. Oliphant, director of the Imperial Forestry Institute, Oxford. *C.I.E.:* Dr. W. Burns, agricultural expert to the Government of India in the Imperial Council of Agricultural Research Dept.; Lieut.-Colonel G. Covell, director of the Malaria Survey of India;

Mr. H. B. Dunnicliff, chief chemist, Central Revenues Chemical Service, Lahore. *C.B.E.:* Mr. W. F. Brown, chief mineral adviser to the Commissioners of Crown Lands; Dr. A. S. Griffith, bacteriologist, Medical Research Council. *O.B.E.:* Prof. W. E. Agar, professor of zoology, University of Melbourne; Dr. H. R. Angell, senior pathologist of plant industry, Council for Scientific and Industrial Research, Commonwealth of Australia; Prof. C. J. Hawkes, professor of engineering, King's College, Newcastle; Mr. W. F. Higgins, secretary of the National Physical Laboratory.

Sir Frank Smith has retired from his post as Secretary to the Committee of the Privy Council for Scientific and Industrial Research and has been appointed adviser on research and development to the Anglo-Iranian Oil Co. H.M. The King has approved the appointment of Prof. E. V. Appleton, D.Sc., LL.D., F.R.S., to succeed Sir Frank Smith.

Prof. G. I. Finch has been created Commander of the Order of King Leopold II for his services in Belgium while Fondation Francqui professor in the University of Brussels during the year 1937–38.

The gold medal of the Royal Astronomical Society has been awarded to M. Bernard Lyot of the Meudon Observatory, France, for his work on the solar corona.

Other recent awards have included the following: Murchison medal of the Geological Society to Dr. H. Jeffreys; Longstaff medal of the Chemical Society to Prof. I. M. Heilbron; Duddell medal of the Physical Society to Mr. R. W. Paul; the medal of the Institute of Metals to Sir Harold Carpenter.

Prof. Sir Robert Robinson has accepted nomination as President of the Chemical Society for the period 1939–41. The Council of the Society has nominated Prof. G. M. Bennett to succeed Dr. H. J. T. Ellingham as honorary secretary and Mr. F. P. Dunn to succeed the late Mr. Emile Mond as treasurer.

Dr. R. L. Smith-Rose has been appointed superintendent of the Radio Department of the National Physical Laboratory.

The following is a list of the presidents of the several Sections of the British Association for the meeting at Dundee this year: Section A (Mathematics and Physics), Mr. R. S. Whipple; Section B (Chemistry), Prof. E. K. Rideal; Section C (Geology), Prof. H. H. Read; Section D (Zoology), Prof. J. Ritchie; Section E (Geography), Mr. A. Stevens; Section F (Economics), Prof. H. O. Meredith; Section G (Engineering), Mr. H. E. Wimperis; Section H (Anthropology), Prof. W. E. Le Gros Clark; Section I (Physio-

logy), Prof. D. Burns ; Section J (Psychology), Mr. R. J. Bartlett ; Section K (Botany), Prof. D. Thoday ; Section L (Education), Dr. A. P. M. Fleming ; Section M (Agriculture), Sir Thomas Middleton.

We have noted with very great regret the announcements of the death of the following scientific workers during the past quarter : Prof. George Barger, F.R.S., regius professor of chemistry in the University of Glasgow ; Prof. J. W. Bews, botanist and principal of Natal University College ; André Blondel, electrical engineer ; Dr. Calvin B. Bridges, geneticist, of the California Institute of Technology ; Dr. F. P. Burt, reader in stoichio-chemistry in the University of Manchester ; Sir Thomas Callender, managing director of Callender's Cable Co. ; Prof. H. A. Cummins, C.M.G., emeritus professor of botany, University College, Cork ; Dr. C. J. Gahan, keeper in the Department of Entomology, British Museum ; Dr. F. W. Goodbody, lecturer in medical chemistry, University College, London ; Prof. E. H. Hall, Harvard University, discoverer of the Hall effect ; Prof. W. McDougall, F.R.S., psychologist ; Mr. Emile Mond, chemist ; Mr. M. A. Phillips, guide lecturer in the British Museum (Natural History) ; Prof. A. W. Porter, F.R.S., physicist ; Prof. H. H. Woollard, F.R.S., anatomist ; Prof. Georges Urbain of the Sorbonne, chemist.

National Bureau of Standards Handbook H.23 issued by the Superintendent of Washington, D.C. (10 cents), contains an important account of the precautions which should be taken for the protection of persons habitually using radium, *e.g.* technicians, nurses and messengers. A chart prepared by G. Failla, physicist to the Memorial Hospital, New York, shows the minimum safe distance for continuous working (7 hours daily) with different masses of radium entirely surrounded by various thicknesses of lead. For example, if 1 mgm. of radium is enclosed by 1 cm. of lead the safe distance is just under 20 cm. ; for 1 gm. 15 cm. of lead would be needed for the same working distance.

The National Physical Laboratory has issued a number of pamphlets containing details of the tests carried out in its several departments together with the reduced scales of fees which came into operation on January 1. The pamphlets are primarily designed to inform manufacturers as to what the Laboratory can do, the form in which test specimens should be prepared and the manner

in which they should be packed and despatched ; but there is much detail of general interest.

The pamphlet relating to the work of the Heat Division is concerned with thermometric tests, hygrometers, viscometers and the determination of the usual thermal constants—transition temperatures, specific heats, latent heats, conductivities and coefficients of expansion. The conditions for the issue of certificates of performance for liquid-in-glass thermometers are given in detail. For Class A mercury thermometers these include scales of maximum permissible errors, limits for the variation of the correction along the stem and limits for the temporary depression of the ice point. It is stipulated that corrections at consecutive test points shall not differ by more than five times the limit of accuracy of the test, *e.g.* by not more than 0·01° C. if the thermometer is tested to + 0·002° C. The ice point is checked at the commencement of the test and again 24 hours after its conclusion. The difference must not exceed the accuracy with which the ice point is observed. Thus if the initial ice point was 0·00 ± 0·01° C. the final one must lie within the limits ± 0·02 ± 0·01° C. The fees for class A tests vary with the accuracy required, the range and the number of test points. A − 20° C. to + 100° C. thermometer is tested at seven temperatures to an accuracy of ± 0·1° C. for 4*s*. and to ± 0·01° C. for 15*s*.

The conditions fulfilled by clinical thermometers bearing the N.P.L. mark are described. The graduations must not be in error by more than 0·2° F. below 106° F. and the mercury column must not drop by more than this amount when the thermometer is allowed to cool for observation. The resetting must satisfy a centrifuge test and the scale must allow the readings to be taken easily. The alleged rapidity of action (½ min., 1 min., etc.) is not checked since this varies when the same thermometer is used for different individuals. The tests cost the maker 3*d*., including free collection and delivery in the London area if not less than 1,000 thermometers are presented at one time !

Determination of the resistance of a four-lead platinum resistance thermometer at the ice, steam and sulphur points costs £2 10*s*. with another £2 for a measurement at the oxygen point. With this type of thermometer the accuracy at the ice and steam points does not exceed ± 0·02° C. The usual fee for a specific heat measurement between 0° C. and 100° C. is £4 and the same charge is made for determining the thermal conductivity of a metal in the range 50° C. to 200° C. A determination of the conductivity

between 200° C. and 800° C. costs £8. For such conductivity measurements a guard ring method is used. One end of the specimen (a rod *c.* 15 in. long and 1, 1·5 or 2 in. diameter) is maintained at a suitable high temperature while the other is cooled by a flow calorimeter which measures the heat conducted. Radial flow is prevented by a guard tube, similarity of temperature along rod and tube being checked by thermo-elements attached to the former at three equidistant points.

Other pamphlets issued by the Laboratory deal with the work of the Radiology and of the Acoustics Divisions. One or two details in each only can be noted here. Search for lost radium containers costs about £5 per day in the London area ; inspection of an X-ray or radium department from £5 to £15 plus the personal expenses of the inspecting staff ; tests on a high vacuum gauge are carried out for about £5. The Acoustics Division is prepared to report on plans for auditoriums, conference halls, etc. ; to inspect completed buildings for the purpose of effecting improvements and, of course, to carry out routine transmission and absorption tests on suitable material.

We have also received from the Laboratory a copy of the *Abstracts of Papers* published in 1937 (H.M. Stationery Office, 1*s.* net), and Vol. XXIV of the *Collected Researches* (£1 2*s.* 6*d.* net). The former contains abstracts of 141 papers classified under suitable headings, an author and a subject index and a preface by the new director, Prof. C. G. Darwin. Here it is pointed out that the main purpose of the Laboratory is to assist British industry, that opportunities for advising British firms on technical matters is welcomed, and that no charge is made unless experimental work is carried out at the request of the firm.

The *Collected Researches* are concerned mainly with measurements relating to fundamental standards and is the last volume of its kind which will be published. It contains Stott's paper on pivots and jewels used in measuring instruments, Vigoureux's determinations of the electromotive force of the Weston cell, the ampere and the ohm, and Sears and Barrell's paper on the wavelengths of light.

The *Report of the Department of Scientific and Industrial Research* for the year 1937–38 (H.M. Stationery Office, 3*s.* net) contains a summary of the work carried out by the various Research Boards and Research Associations supervised by the Department. One new research association was formed during the year—the British Coal Utilisation Research Association. It will have a minimum

income of £18,000 a year. The Wool Research Association has had a difficult time, but certain sections of the industry have increased their subscriptions and a possible income of £29,000 a year may be available during the next quinquennium. By contrast the Cotton Research Association, with an income of £87,000 a year, continues to expand. Its laboratories and workrooms cover over three acres and are equipped with all the apparatus and machinery needed to deal with problems encountered in every process of the industry from the raw material to the finished cloth.

Grants to students in training, senior research awards and grants for the development of special investigations totalled £26,400. Eighty applications for maintenance allowances for students-in-training were granted and 105 refused, the distribution between the various branches of science being much as before.

It is interesting to note that the Wool Association's chlorination process for the preparation of wool resistant to shrinkage has been put on a production basis and that the goods manufactured from the wool are to be sold under the trade mark *Woolindras.*

We have received from Messrs. Adam Hilger Ltd. copies of their publications S.B. 107/9, S.B. 156/6 and S.B. 265 dealing respectively with spectrographic outfits for metallurgical and general chemical analysis, outfits for absorption spectrophotometry and with the arrangement and equipment of a complete spectrochemical laboratory. The details provided are sufficient to enable a prospective user to learn what can be done, how it is done and what it will cost. The quartz spectrographs cover the range 2000 A. to 8000 A. and, in the largest, the length of the spectrum between these limits is 67 cm.

The General Chemical and Pharmacological Co., Ltd., Judex Works, Sudbury, Middlesex, have sent us a pamphlet dealing with their analytical reagents. There will in future be given on the bottles containing them the actual values of the impurities in the batch of which the material is a portion. Moreover, this analysis will be based not only on the analysis of the works laboratory, but also on an analytical certificate issued by competent independent analysts. The advantage to the user of knowing the actual values of the impurities in the reagent instead of a schedule of maximum values of impurities is obvious, and the manufacturers of Judex analytical reagents are to be congratulated on their new venture.

ESSAY REVIEWS

THE DISPLACEMENT OF MAXWELL. By W. WILSON, F.R.S., Hildred Carlile Professor of Physics in the University of London. Being a Review of **Electromagnetics : A Discussion of Fundamentals,** by ALFRED O'RAHILLY, Professor of Mathematical Physics, University College, Cork. [Pp. xii + 884, with 73 figures.] (London, New York, Toronto : Longmans, Green & Co. ; Cork : Cork University Press, 1938. 42*s*. net.)

MUCH of this book is historical. The author claims that in it the history of electromagnetic theory is re-written and we find in the titles of many of the chapters such names as Poisson, Maxwell, Ampère, Neumann, Liénard, Poynting, Voigt, Weber, Ritz and others. He professes to disagree with most of what is commonly accepted to-day about electromagnetic theory. He maintains " that Maxwell's views are really off the main line of development " and he is almost contemptuous of Einstein's special theory of relativity which he describes as " Einstein's use of Voigt's transformation." He regards his work, quite mistakenly in the opinion of the present reviewer, as " an essay in constructive criticism." There is, of course, a semblance of constructiveness in the suggestion that an " appropriate theoretical sub-structure for [electromagnetic] phenomena " is a law of force between moving charges which must involve the element of propagation in time. Professor O'Rahilly has no new sub-structure of his own to put in place of the Maxwell-Lorentz-Einstein foundation. He seems to hesitate between a formula derived by Liénard and a " universally ignored alternative formula proposed by Ritz." Of the former it may be pointed out that it is a consequence of the accepted classical theory, as Professor O'Rahilly admits, while the latter, being based on the " initial assumption . . . that each electrified point emits, at each instant and in all directions, fictitious infinitely small particles . . ." seems exposed to criticism even more devastating than that directed by Professor O'Rahilly against the imaginary clocks and observers which he seems to regard as an essential part of Einstein's special theory of relativity.

Most of the author's criticism is directed against the expositions

of electromagnetic theory by numerous writers, ranging from great authorities like Einstein, Planck, Drude and Sommerfeld, down to the writers of obscure text-books. Apart from this what will strike the reader most forcibly in the perusal of this work is the attack on the notion of "field," on Maxwell's displacement and on Einstein's relativity. "Any alleged explanation based on 'fields,'" we are told, "is ultimately reducible to metaphors." Of Maxwell's displacement he says: "The term 'displacement' was most unfortunate; it seems to have had a hypnotic effect on Maxwell himself, . . ." In another place we read ". . . it [*i.e.* $\dot{E}/4\pi$] is not a current at all, it does not represent a flow of electricity." Finally on pp. 522 and 523 we read "We have already shown the impossibility (it ought to be obvious!) of regarding $\dot{E}/4\pi$ as a current, except by a quibble on the word 'electricity.'" Yet strangely enough we read at the end of the chapter devoted to Maxwell and his displacement current: "It does not at all follow from this chorus of despair [he is referring to expositions by various writers of the nature of Maxwell's displacement] either (1) that Maxwell's innovation was wrong or (2) that it cannot be explained and derived without contradicting the doublet theory of dielectrics and without resisting logical incorporation into an electron theory. . . . There is then all the more urgency for relating it to the rest of electrical theory without perpetrating paradoxes about a current which is not a current." Can it be that Professor O'Rahilly is all the time at heart a follower of Maxwell and Einstein and that most of his book is a "mere logomachy"?

Clerk Maxwell, one of the greatest of all who have contributed to the advancement of physical science, introduced the displacement current as a hypothesis and thereby succeeded in unifying the two provinces of optics and electromagnetism. His electromagnetic theory brought about a co-ordination of two vast collections of experimental results, and the ability of a theory to achieve this kind of thing is, according to Professor O'Rahilly himself, the sole test of it (p. 641). Indeed, it did more than this. It predicted the existence and character of electromagnetic waves and it has been the inspiration and guide of much or most of the later work—in macroscopic physical science at all events. It is quite easy to justify the view that $K\dot{E}/4\pi$ is a current density—even in free space. In fact, we usually, and indeed almost invariably, measure currents of electricity by the line integral $\oint(\mathrm{H}dl)$—apart from a constant numerical factor—and a field intensity, notwithstanding its "metaphorical" character, is something that can be measured.

All galvanometric measurements are based, directly or indirectly, on the identification, apart from a constant numerical factor, of current strength and this line integral and actual observation proves that it does not vanish round suitably oriented loops in regions through which broadcasting waves are travelling. This is sufficient evidence that electric currents flow through such loops. These are Maxwell's displacement currents. We can easily associate them with the actual passage of electricity. We have only to integrate $K\dot{E}/4\pi$ over an area A perpendicular to E and a period of time to obtain $KAE/4\pi$ $\left(\text{or } \frac{KAV}{4\pi d}\right)$ which is the charge acquired by condenser plates perpendicular to E.

Relativity, which may be said to have its immediate origin in Maxwell's theory, is also severely treated by Professor O'Rahilly. He comments on it as follows: "Seizing upon an elementary and innocuous transformation given by Voigt in 1887, they [the mathematicians] have, under the leadership of Einstein (1905), erected a weird structure labelled 'relativity.' It involves the introduction of *dates* into physical laws, the proceeding being skilfully concealed by the invention of imaginary 'clocks'" (p. 851), and further on (p. 852) we read: "The real popularity of the new ideas began when Minkowski (1908) adopted the analytical dodge metaphorically called the use of four dimensions. The mathematicians got their chance, and the semi-educated developed their natural gullibility."

These passages are characteristic of the author's style and critical method. There is quite frequently a very serious misapprehension of the plain meaning of some description or theory accompanied by the ascription of unworthy motives. Einstein's theory is apparently vitiated by "dates" which he has "skilfully concealed." Minkowski uses an "analytical dodge." The mathematicians "got their chance." The "analytical dodge" of Minkowski, to which reference has been made, is simply the adoption of $x^2 + y^2 + z^2 - c^2t^2$ as an invariant; x, y and z being the components, in a rectangular co-ordinate system of a displacement and t the time interval during which it occurs. By "invariance" is meant, of course, that $x^2 + y^2 + z^2 - c^2t^2$ is equal to $x'^2 + y'^2 + z'^2 - c^2t'^2$ where x', y', etc., refer to any other rectangular co-ordinate system which is moving with a constant velocity relatively to the former one. Since, if we like, we may write $w^2 = -c^2t^2$ and $w'^2 = -c^2t'^2$, we may also speak of space-time as a four dimensional continuum and call the invariance the theorem of Pythagoras. The important thing about this is that it is *physically* significant and its immense co-ordinating value

(the "sole test of a physical theory" according to Professor O'Rahilly) is beyond dispute.

The book is interesting and much of the historical part is valuable, for example, the account of the work of Voigt and his discovery of the transformation which is now generally attributed to H. A. Lorentz. There is also much healthy criticism of loose expositions of physical theory by individual writers. The reader will be impressed by the great erudition and competence of the writer; but he will probably close the book with a sigh of regret that its author did not approach his task in a broader and more open-minded spirit.

THE LAWS OF HEREDITY AND THE COURSE OF EVOLUTION. By E. W. MacBride, M.A., D.Sc., F.R.S. Being a Review of **More Difficulties of the Evolution Theory,** by Douglas Dewar, F.Z.S. [Pp. xii + 215, with 6 plates.] (London: Thynne & Co., Ltd., 1938. 8*s*. 6*d*. net.)

The occasion of this article is the appearance of a remarkable book entitled *More Difficulties of the Evolution Theory*, in which the author attacks the whole idea of evolution and supports a doctrine of the origin of species by special miraculous acts of the Creator, such as was held by our great-grandfathers and is supposedly taught in the Book of Genesis. It would require a long essay on comparative embryology to expose all the errors of Mr. Dewar's book, and for this not even the most generous of editors has space. One or two examples of Mr. Dewar's arguments may however be cited: thus he maintains that the human "os coccyx" is an independent creation, though in the embryo it appears as a short tail which is regarded by every comparative anatomist as evidence that the human skeleton has been evolved from that of a simian ancestor. In the plains of North America are found the bones of numerous horse-like animals which show, step by step, the change from a four-toed ancestor to the one-toed horse, a series which Huxley regarded as the final proof of evolution. According to Mr. Dewar, however, all these varied types wandered in from the north, and there is no proof that one evolved from another. "In my opinion," he writes, "the unfossiliferous nature of the pre-Cambrian rocks is fatal to the theory of evolution." If we reflect that rocks of all ages back to the Cambrian may persist as loose clays in which even the structure of a jelly-fish may be preserved, and that all pre-Cambrian rocks are of indurated material in which no delicate structure could survive, the value of this argument of Mr. Dewar's must be apparent!

The book is dedicated to Dr. Fleischmann, Professor of Zoology at Erlangen, who for thirty-seven years has opposed the doctrine of evolution. It may be noted that Erlangen is a Roman Catholic university, and Dr. Fleischmann's interpretation of his own excellent descriptive researches may well have been affected by his intellectual environment. In any case the attitude of the Church towards evolutionary science has broadened since Fleischmann's day, and as a result of personal experience I have reason to believe that, granted the initial act of creation, few enlightened Churchmen would now quarrel with the doctrine of subsequent evolution of species.

Mr. Dewar attributes to his opponents an atheistic materialism such as was current towards the end of the nineteenth century when the influence of Huxley and Tyndall was at its height. Theological discussions are out of place in a scientific journal, but I may be permitted to remark that pure atheism is a difficult creed to maintain, for it means the development of everything by chance out of a mob of particles as to the origin of which the atheist has no explanation. It is, of course, well known that the leading physicists have abandoned the atheistic position: Professor von Planck cannot be accused of orthodoxy, yet he has said, "The fact that we can predict and to a certain extent control the course of future events would remain an insoluble mystery were there not a fundamental agreement between our minds and the structure of the Universe." In other words, there is behind phenomena a Mind essentially similar to but infinitely greater than our own. Some modern physicists go even further and assert not only that the "Laws of Nature" are the manifestations of an Almighty Mind but that at certain points in the stream of development direct explosive intervention took place, altering the gradual process and giving it a new start. As Lord Kelvin pointed out long ago, there is evidence of three such interventions, the first to effect the primal aggregation of energy in the Universe, the second to generate Life and the third to produce Man.

I once gently chided Sir James Jeans on his use of the word "creation" in an essay, and he replied: "It is all very well; Science takes you back a certain distance and then brings you up against a blank wall."

Evolution properly means "unfolding," as the bud of a flower gradually unfolds its petals, and in the eighteenth century the word was used in this sense. Thus Mother Eve was supposed to have had within her not only the germs of her immediate offspring but within these germs those of the subsequent generation, and so

on, down to the present day. Lamarck, the first evolutionist in the modern sense, described the gradual modification of species as one generation succeeded another, and called his theory "Transformism": the word "Evolution" was used to describe this process by Lyell in his *Principles of Geology*, and this use is now universal.

Evolution is the result of the nature of animals and plants and of the manner in which they transmit their characteristics to following generations. Like tends to beget like, but parents and offspring are never identically similar, and evolution results from the summation of their differences, the nature of which therefore merits close examination. Two kinds of these differences are familiar to us: there are first what are called "fluctuating variations"; if we look at the leaves of a tree we can trace a continuous series from the smallest to the largest, and the same is true of animals. Darwin assumed that these minute variations occurred in all directions and were heritable: the first great blow to be struck at Darwinism was the proof by the pure-line investigations that these small variations are not heritable. Thus Johannsen took the smallest and lightest bean in a crop produced by one plant, sowed it and self-fertilised the flowers of the plant which sprang from it; he then compared the crop which it produced with that resulting from similar treatment of the heaviest bean in the original crop. In each case the individual beans differed slightly amongst themselves in size and weight, but the crop produced by the lighter of the two selected beans was no less heavy than that of its fellow. Agar examined the females of the little fresh-water crustacean, Simocephalus, which produce eggs that develop without fertilisation in a brood-pouch between the body of the mother and its projecting shell; breeding from the progeny of a single female he found that the longest larvæ produced descendants that were no longer than those of their shortest sisters. Finally there are the experiments of Jennings (in America) on the "Slipper Animalcule," the Protist Paramecium, which reproduces itself by transverse division. He selected a single individual and put it in a bowl of hay infusion; when it had so multiplied as to overcrowd the bowl he took from the latter a single individual and placed it in a new bowl. In this manner Jennings was able to carry on a population derived from a single individual for hundreds of generations: when he started a new line by selecting the smallest individual in a bowl he found that the animalcules so produced were just as large as those obtained by selecting the largest individual and breeding from it.

No three organisms could be more divergent from one another in the scale of life than a flowering plant, a Crustacean and a Protist, and it is clear therefore that the rule of life deduced from these three sets of experiments is of universal application.

But there exist variations of a quite different nature, known as " sports " ; these crop up in every farm-yard and garden, and are strongly inherited. Darwin never properly discriminated between these and " fluctuating variations." The fashionable view has been that these sports are the material of evolution and of the formation of new species, but the most recent investigations show that this view is untenable. Mohr, who was described by the late Dr. Bateson as the foremost " drosophilist " in Europe, has shown that sports are the outward and visible signs of a weakness of constitution and that the more marked are the peculiarities of the sport the greater is the weakness. The cause of sports is evidently to be found in the soil (plants), stables and sties (animals) in which they are bred ; when sports are transferred to natural conditions where they are free from the attacks of their enemies they go wild—*i.e.* they resume the habits and structure of the wild ancestor. One of the best instances of this is the transformation of the New Forest pigs imported into New Zealand about 1790 by Captain Cook, who distributed them to the Maoris in order to wean the latter from cannibalism—they were in the habit of eating their superfluous young girls. The Maoris, however, knew nothing of husbandry, for in New Zealand there were no mammals, only a few species of reptiles and amphibia. The pigs escaped into the bush and in forty years developed all the features of the original wild boar ; they became a menace to the settlers and expert in cutting out isolated sheep from the flocks, surrounding and devouring them. Eventually the Government had to institute a campaign against them, but has not even yet succeeded in exterminating these feral pigs from the country.

If, then, neither fluctuating variations nor sports can produce evolution, what remains ? Can it be as Lamarck asserted, that new habits adopted under pressure of new circumstances and continued for a long time become heritable and leave their marks on the structure of their possessors ? The newest investigations not only support this hypothesis but afford definite proof of it, and the Lamarckian view is no longer merely a plausible theory, but has become an established fact.

I shall give one instance of the researches which have established Lamarckism—*viz.* a series of experiments carried out by my late pupil, Miss Dorothy Sladden, who investigated the tastes of par-

thenogenetic stick-insects, the normal food of which is, in England, the leaves of the privet. By forcing the insects to eat ivy by means of periods of starvation, between which they were rescued from complete famine by a meal of privet, Miss Sladden showed that most of them required five or six such periods before they would accept ivy—some indeed took it only at the ninth or tenth presentation. The eggs of all the insects which accepted ivy at the same presentation were collected and allowed to produce a new generation which was treated similarly to the first. After four or five generations 100 per cent. of the progeny of all classes accepted ivy at the first presentation.

Experiments no less convincing have been carried out by Professor W. Macdougall on the heritability of habits artificially instilled into rats, but as these experiments are less easy than those of Miss Sladden to describe within a limited compass I have confined myself to the latter.

Evolution is thus seen to be the inevitable result of changes of habit and food forced on expanding races by the necessities of environment, and organic structure is merely crystallised habit. Moreover, the fact that new habit is acquired only in the adult stage, while in the immature stages the ancestral habit is retained, accounts for the biogenetic law that the individual life-history recapitulates that of the race.

REVIEWS

PHYSICS

The Evolution of Physics. By ALBERT EINSTEIN and LEOPOLD INFELD. [Pp. x + 319, with 3 plates and 75 figures.] (Cambridge: at the University Press, 1938. 8*s*. 6*d*. net.)

THE purpose of this book is "to sketch in broad outline the attempts of the human mind to find a connection between the world of ideas and the world of phenomena." It is due in no small measure to Prof. Einstein that the evolution of physics can now be defined in these purely subjective terms. In pre-relativity days the corresponding phrase would have been something like "to sketch in broad outline the attempts of the human mind to discover the contents and laws of behaviour of the external world." No simple-minded reader of these pages, we believe, can fail to realise that the newer statement represents a far better outlook, making possible a far deeper understanding, on the progress of physics. The order of development of the ideas is partly logical and partly chronological, and where a choice has had to be made the former sequence has been preferred. In such a brief description no other course was desirable, or even possible, and the authors have also wisely chosen to concentrate attention on the main lines of development rather than to aim at comprehensiveness. Some important branches of physics thus receive only brief mention, and others (*e.g.* the theory of sound, surface tension, osmotic pressure, etc.), however important in themselves and in their relation to extra-physical considerations, are completely ignored. This, of course, is no defect; it is a deliberate contrivance, and serves to emphasise the fact that the development of general ideas, and not the extent of their application, is the main theme.

There are four chapters, each representing an epoch in the history of physical thought. The first deals with the rise of the mechanical view from the ideas of Galileo and Newton—a movement which inspired the hope that all phenomena could be "explained by the action of forces representing either attraction or repulsion, depending only upon distance and acting between unchangeable particles." The second chapter describes the decline of this view as difficulties accumulated in the study of electromagnetic phenomena and light. This represents an interim period, during which the mechanical view was still firmly enthroned but the influences which were ultimately to overthrow it were gradually gathering strength. In the third chapter, the displacement of the mechanical view by field theory is described. Here attention is concentrated, not on particles and the forces they exert on one another, but on the "structure of the field" between them. The two great expressions of the field idea are Maxwell's electro-magnetic theory and the theory of relativity, the chief points of which are explained. This is naturally the most difficult section of the book, and, though the presentation

is clear and well-conceived, it justifies the authors' reminder that careful reading is necessary and that "a scientific book, even though popular, must not be read in the same way as a novel." The authors' summing-up of this chapter seems to imply that they consider the present conceptual scheme of macroscopic physics less satisfying than that contemplated (but not realised) in the days of the mechanical view, in that one concept (matter) was then sufficient, whereas now we need two (matter and field). They consider the possibility of a pure field theory, but decide that, at present at least, matter also is indispensable. There are two considerations, however, which prompt us to question this conclusion. In the first place, the mechanical view required not only the concept of matter, but the concepts of space and time also—three independent concepts—whereas the general theory of relativity merges all these together, so that the characteristics of field and matter alike can all be expressed in terms of a single unit, say the unit of length. And secondly, since we are dealing only with general ideas and not with practicability of application, is it not true to say that, when the structure of the system is completely described in terms of the $g_{\mu\nu}$, the density of matter at *every* set of values of x, y, z, t is determined by the energy-momentum tensor, whether that density is zero or not? This tensor has, of course, frequently been used to describe continuous distributions of matter, where matter and field are identical, but does it need more than the correct mathematical expression in order to describe *any* field of space-time containing isolated pieces of matter? If not, then field becomes the only "reality," and the line-element which defines a system defines comprehensively the bodies and the spaces between them at all times. The phenomena dealt with by the quantum theory, of course, stand outside, and these are outlined in the final chapter, with a brief account of the difficulties they create and the attempts made to solve those difficulties.

It will be realised that the book is concerned with the most fundamental problems of physics, and deals not so much with the explanations of phenomena as with the kinds of explanation that have been sought. As such it is of great value. One can scarcely recommend a book by Professor Einstein on the ideas which have led to the theory of relativity without appearing slightly ridiculous. Even Homer may nod, however, and perhaps it is not presumptuous to express the opinion that on this occasion he has not done so.

H. D.

Physik: für Studierende an Technischen Hochschulen und Universitäten. Von Ingenieur Dr. Paul Wessel. Herausgegeben von Dr. V. Riederer von Paar. [Pp. xii + 550, with 277 figures.] (Munich: Ernst Reinhardt, 1938. RM. 4.90.)

This German text-book is advertised as suitable for students in technical colleges and universities. The first thing which strikes the English reader is the very wide range of topics with which it deals. The descriptive portion of the text covers quite a number of subjects of a second-year honours course in physics as well as those of previous courses. The treatment is, however, as elementary as possible and is very clear. There is, perhaps, a slight but natural bias towards the point of view of an electrical engineer. Among many good sections those on electrical machines, X-rays and polarised light are excellent. The most striking single feature is the special

chart of the periodic system devised by the author, in which he uses the lengths of arcs of concentric circles as a means of exhibiting the distribution of electrons in the various sub-atomic shells. Assuming that the book was intended as an introduction to the study of physics, it may be said that the purpose has been successfully achieved, though naturally the mathematics has been cut down to matriculation standard. Depth has been sacrificed to width, *e.g.* expressions for the axial and equatorial fields of a bar magnet, and the capacity of a parallel plate condenser are neither stated nor derived. On the other hand short but clear accounts, with good diagrams, of such instruments as the cathode ray oscillograph, the mercury vapour rectifier and the ultramicroscope are given. Even relativity, artificial disintegration and wave mechanics receive a fair amount of attention. Numerous descriptive examples are included. The book may also be recommended to the second-year student of scientific German on account of its extensive vocabulary, easy type of sentence and comparative cheapness.

N. D.

Photoelements and Their Application. By Dr. Bruno Lange. Translated by Ancel St. John, Ph.D. [Pp. 297, with 67 figures.] (New York: Reinhold Publishing Corporation; London: Chapman & Hall, Ltd., 1938. 27*s.* 6*d.* net.)

It should be pointed out at once that this book is confined to a very limited field. By "photoelements" Dr. Lange means "rectifier-type" or "barrier-layer" photocells only, and most of the work described was carried out by himself or his fellow countrymen. No attempt has been made to bring the American edition up to date, so the book is really three years old. This is felt mainly in the first half, dealing with the physical properties of these cells and the theory of their action. Nevertheless, the historical summary and some clear expositions of elementary theory (*e.g.* pp. 77–87) will serve as useful introductions to more detailed and later work. A proper perspective of the subject cannot be obtained from this book alone.

The second half of the book is perhaps of more general interest, for here many applications of the cells are described, especially in photometers and photographic exposure meters. Incidentally, it should be emphasised more strongly that it is difficult to use valve amplification with this type of cell. Many of the instruments described have been designed by Dr. Lange himself. Bibliographies are appended to each half of the book.

The translation is not always very happy, many sentences still showing signs of the original German construction, and we read of "Wilson *fog* chambers," "*ultra*red radiation," etc. The book is profusely illustrated and excellently produced, but the price is high.

It is a great convenience to have a summary in English of scattered German papers on photoelements, particularly when the author of the book is himself a pioneer in the subject.

F. A. V.

Negative Ions. By H. S. W. Massey. Cambridge Physical Tracts, No. 1. [Pp. xiv + 105, with 19 figures.] (Cambridge: at the University Press, 1938. 6*s.* net.)

In this volume, the first of the new series of Cambridge Physical Tracts, Prof. Massey gives a clear and concise account of the present extent of our

knowledge about the formation and dissociation of negative ions. In Chapter I the author discusses the theoretical basis for the existence of negative atomic ions and compares theoretical values of electron affinities with those obtained by various experimental methods. Chapter II contains a theoretical discussion of negative molecular ions, whilst Chapters III and IV deal chiefly with experimental results and discuss fully the various modes of formation of negative ions and the detachment of electrons from negative ions. In the last chapter there is a discussion of the probable part played by negative ions in such phenomena as glow discharges and the problems of the upper atmosphere. There is relatively little mathematics in the book; the more detailed developments of *wave mechanical theory* are printed in small type and may be omitted without any loss to the general understanding of the theory. This is a very readable book and the author has succeeded in blending the theoretical and practical aspects of the subject into a consistent whole.

S. R. T.

A Text-book on Crystal Physics. By W. A. Wooster, M.A., Ph.D. [Pp. xxii + 295, with 108 figures.] (Cambridge: at the University Press, 1938. 15*s*. net.)

Dr. Wooster has succeeded in compressing a very considerable amount of information into the 300 pages of this useful volume, which deals with the mechanical, thermal, electrical, optical and magnetic properties of crystals, and their relations to the structure. This has been achieved partly by the employment of tensor notation and partly by condensing much of the description of experimental methods to the bare essentials. In a work that is to serve as a text-book this condensation has drawbacks, but Dr. Wooster's descriptions are lucid, and sufficient references to original works are given to enable the student to find details elsewhere. It is also probable that few Courses in the newer universities will have offered the student sufficient formal crystallography for him to grasp easily the significance of the tensor notation. But once he has accustomed himself to its use he cannot fail to appreciate its elegance and convenience, and Dr. Wooster is to be congratulated on an effort to increase its employment. Again the use of the indicatrix is not usually familiar to students other than those who read Mineralogy, and Chapter V on crystal optics may offer difficulties to the majority. But this chapter contains much valuable material, and corrects several inaccuracies in existing text-books. For instance, the Reviewer was quite unaware of the incorrectness of the normal explanations of conical refraction.

In general this text-book may prove difficult reading to many undergraduate students, but will certainly be of great value to postgraduates who wish to study and teach crystallography.

S. H. P.

Ferromagnetism: The Development of a General Equation to Magnetism. By J. R. Ashworth, D.Sc. [Pp. xiv + 97, with 17 figures.] (London: Taylor & Francis, Ltd., 1938. 7*s*. 6*d*. net, not 12*s*. 6*d*. as stated in the Books Received List of the January Number).

This book contains an account of experiments on ferromagnetism carried out by the author and published in the *Philosophical Magazine* and elsewhere

between 1897 and 1937. From his experiments the author draws a number of conclusions about the nature of the intramolecular field of Weiss and about other aspects of the theory. He does not, however, mention the developments which have followed the application of quantum mechanics to the problem of ferromagnetism, or the theory of "domains" which has been so useful in reconciling the high susceptibility with the large value of the Weiss field. Some of his experimental material, also, is hardly up to date; his discussion of the change of resistance in a magnetic field does not mention any of the work carried out in the last few years.

N. F. M.

On Understanding Physics. By W. H. WATSON, M.A., Ph.D., F.R.S.C. [Pp. xii + 146.] (Cambridge: at the University Press, 1938. 7*s*. 6*d*. net.)

THIS book is an attempt to interest physicists and scientists in general in the recent developments in philosophy known as logical positivism, and in particular, the work of Wittgenstein.

The author first stresses the necessity for discipline in philosophy and concludes that its primary concern is with the logic of ordinary language. In a chapter on Methods of Representation, Prof. Watson points out the danger of using spatial representations of time, resulting in the error of arguing about "time's arrow," and thus neglecting the difference between linguistic possibility and physical (or logical) possibility (needless to say, this is the confusion underlying J. W. Dunne's "Serialism"). Similarly the absurdity of calling an electron a "wave" is also dealt with, and there are some salutary remarks on the growing arrogance of mathematicians. It is difficult, however, to accept the view that the Uniformity of Nature is not a hypothesis about the world, but a statement concerning our method of representation. Here the logical positivists would appear to be pressing their views too far and betraying the fact that they are not active scientific researchers themselves.

There are three important chapters on the Nature of Mechanism, the Logic of Substance and Motion, and Some Aspects of the Symbolism of Mechanics and Electricity, but they are difficult reading. The importance of scientists understanding the symbolic processes which they are continually employing cannot be too strongly urged, but one is left with the impression that this book could have been much more clearly written.

G. B. B.

Direct and Alternating Current Potentiometer Measurements. By D. C. GALL, F.Inst.P. With a Foreword by S. PARKER SMITH, D.Sc., M.I.E.E., A.M.Inst.C.E. Vol. IV of a Series of Monographs on Electrical Engineering under the Editorship of H. P. YOUNG. [Pp. xiv + 231, with 106 figures, including 12 plates.] (London: Chapman and Hall, Ltd., 1938. 15*s*. net.)

HERE is a timely book written by a man having long experience with potentiometers and their design in connection with a firm of instrument makers. About one quarter of the book is devoted to D.C. potentiometers. This part especially should be read by all physics students as well as by engineers, for it contains many useful hints regarding standard cells, galvanometers, and the accuracy attained with various types of potentiometer. There are

also some very practical pages on methods of constructing and adjusting resistance coils, and the general care of potentiometers, at the end of Chapter XII.

Both co-ordinate and polar forms of A.C. potentiometer are described in detail, followed by their applications, including measurement of current, voltage and power, incremental permeability and other properties of magnetic materials, as well as various miscellaneous uses. Actual numerical results of such measurements are given in many cases. References are appended to each chapter.

It is a good idea to include a chapter on the representation of alternating currents by complex quantities, especially in connection with the co-ordinate potentiometer, but whether the chapter is in its best form is not so clear. Returning to an earlier chapter, there is evidently something wrong with Fig. 17, the control grid of the valve being left disconnected, and the accompanying description wants rewriting. The few other slips scattered about the book are easily corrected, and this is not the place to enumerate them.

This authoritative work should be accessible to all having occasion to use potentiometers.

F. A. V.

CHEMISTRY

Gmelins Handbuch der anorganischen Chemie. 8. Auflage. Herausgegeben von der Deutschen Chemischen Gesellschaft. (Berlin: Verlag Chemie, G.m.b.H., 1937/38.) System-Nummer 22: Kalium, Lieferung 4 [pp. 128] RM. 15.–, Lieferung 5 [pp. 142] RM. 19.50; System-Nummer 24: Rubidium [pp. 250] RM. 31.50; System-Nummer 25: Cæsium, Lieferung 1 [pp. 104] RM. 12.–, Lieferung 2 [pp. 164] RM. 21.75; System-Nummer 39: Seltene Erden, Lieferung 1 [pp. 122] RM. 14.25; System-Nummer 63: Ruthenium [pp. 124] RM. 16.50; System-Nummer 64: Rhodium [pp. 153] RM. 20.25.

LAST autumn German chemists celebrated the 150th birthday of Leopold Gmelin, Bunsen's predecessor in the chair of chemistry at Heidelberg University, who was born at Göttingen in 1788. Correspondence, portraits, and documents relating to him were exhibited in Berlin, and his biography published by E. Pietsch in a small attractively illustrated pamphlet. By far the greatest tribute, however, was paid to the memory of this worthy chemist when the German Chemical Society undertook the publication of a new edition of his famous treatise on Inorganic Chemistry.

Leopold Gmelin himself saw his book through five editions; two more had been published as the result of private enterprise, when, in 1922, the German Chemical Society decided to become responsible for an eighth edition, and appointed Prof. R. I. Meyer editor-in-chief, with power to select competent co-workers. To-day, under the leadership of his successor E. Pietsch, more than forty chemists, all specialists in the various branches of inorganic and physical chemistry, are permanent members of the "Gmelin Staff."

The obvious question as to whether the result justifies this huge amount of labour and expense is easily answered if the Gmelin is compared with any other of the modern comprehensive works on Inorganic Chemistry. On previous occasions reviews published in SCIENCE PROGRESS[1] attempted to do justice to the unique character of this monumental Gmelin edition. No need

[1] See, for example, 32, 168 (1937).

to refer again to the wealth of material, the reliability of the information given, and the clarity of arrangement. Let it suffice here to state that the later volumes, recently published, fully uphold the standard.

It is a matter of great satisfaction to see that the originator of the scheme, Prof. R. I. Meyer, although forced by the age limit to retire from his post as editor, has nevertheless continued to devote his time and effort to the work. He has now contributed the first part of the volume on Rare Earths, thus returning to a field in which as a young man he won high distinction and is still considered one of the greatest living authorities. The complicated history of the discovery of the Rare Earths and the occurrence of their deposits—arranged in mineralogical as well as in geographical order—are described with the mastery which only a life-long interest in the subject can give.

The group of alkali metals is nearing completion. After the issue of these new volumes on potassium, rubidium and cæsium, all that is missing is the description of a few compounds of potassium. At the end of the cæsium volume eight pages are devoted to an account of the vain attempts to discover the highest homologue of the group, eka-cæsium, and to a discussion of its presumable properties.

The two volumes on ruthenium and rhodium, respectively, are the first dealing with metals of the platinum group; their appearance will be greatly welcomed by industrial as well as research chemists. The history of the discovery, occurrence, and preparation of the six platinum metals has so much in common that, in order to avoid repetition, these topics will be dealt with conjointly in the volume devoted to platinum; but the physical and chemical properties of ruthenium and rhodium, and their compounds with other elements—including the great number of complex salts—are exhaustively dealt with in the two volumes under review.

F. A. P.

The Fine Structure of Matter. Vol. II of a Comprehensive Treatise of Atomic and Molecular Structure. Part III: The Quantum Theory and Line Spectra. By C. H. Douglas Clark, D.Sc., A.R.C.S., A.I.C., D.I.C. [Pp. lxxii + 185, with 29 figures.] (London: Chapman & Hall, Ltd., 1938. 15*s*. net.)

This is but one part of a very comprehensive series of books by the same author upon atomic and molecular structure. It is essentially an elementary introduction to the study of line spectra and the treatment is such, that the book may be expected to appeal to the chemist rather than the physicist. The orbital vector method is employed throughout, wave mechanical aspects being more or less neglected. This course has a definite practical value in a book of this nature which aims at an elementary survey. Mathematics is almost avoided, a few essential proofs being relegated to a lengthy appendix.

The book is clearly written and well illustrated. Frequent and effective use is made of data about multiplicities, intensities, etc., in brief tabular form. The rather complex subject of multiplicity in atomic spectra is dealt with in a compact and concise manner.

It is clear that the author has had the Periodic Table of the elements strongly in mind, and in fact the whole book may be regarded as leading up to a general spectroscopic treatment of the Periodic Table. In this connection the detailed treatment of the application of the Pauli exclusion principle is well done.

Much more attention than usual is devoted, in a concluding chapter, to

the problem of valency, and to the chemist and the general reader this is probably the most valuable section. It is an excellent feature.

One or two criticisms can be raised. Surprisingly enough, no mention is made, in the text, of hyperfine structure investigations in line spectra, although some papers upon this are quoted in the extensive lists of references that are given. In view of the elementary nature of the book, the number of these references is decidedly excessive. Although printed in small type, they occupy about one-eighth of the total space devoted to the rest of the text. Since the hyperfine structure quantum number is now denoted by F, the use of this letter in other connections (see page 547) is not a happy one.

This book will undoubtedly be attractive to chemists who possess but a limited acquaintance with spectroscopy.

S. TOLANSKY.

Solvents. By T. H. DURRANS, D.Sc., F.I.C. Vol. IV of a Series of Monographs on Applied Chemistry under the Editorship of E. HOWARD TRIPP, Ph.D. Fourth edition. [Pp. xviii + 238, with 4 figures.] (London: Chapman & Hall, Ltd., 1938. 15*s*. net.)

A BOOK which has gone through four editions since 1930 requires very little comment. In the new edition 33 extra pages have been added and the price increased from 10*s*. 6*d*. The importance of organic solvents in the cellulose lacquer industry grows apace, an indication of which is contained in the numerous new references to specifications of the British Standards Institution and the American Society for Testing Materials.

A welcome addition is the extension of the chapter on toxicity. Since the publication of the third edition a valuable book on the toxicity of industrial organic solvents, giving summaries of published work, has been compiled by Dr. Ethel Browning under the auspices of the Medical Research Board. Some of its main conclusions have been incorporated in this chapter, but no direct reference to their source has been made. In view of the fact that the previous list of references to toxicity has been omitted it rather looks as if this is due to an oversight.

M. B. DONALD.

Crystal Chemistry. By C. W. STILLWELL, Ph.D. International Chemical Series. [Pp. x + 431, with 72 figures and 1 folding plate.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1938. 25*s*. net.)

SINCE the discovery of X-ray interference by von Laue there has accumulated an enormous amount of material on the structures of crystals which has led, not only to results of immediate importance but also to the formulation of fundamental principles of a general nature. This new science of Crystal Chemistry is an essential complement to the work of the chemist, and it is imperative that he should be acquainted with its achievements and its possibilities. It is obvious that a simple and accurate account of work in this field would fill a very real need and make a wide appeal. One turns therefore with high hopes to this volume on crystal chemistry, especially when the author states in his preface that his main purpose is to present the broader aspects of this relatively new branch of chemistry in as simple a fashion as is consistent with accuracy. There is no doubt that this book does contain a

great deal of useful and accurate information, but unfortunately it lacks balance. The pages allotted to the various subjects show this. Fibres are given 68 pages, the structure of organic crystals 14 pages; 98 pages are allotted to metals and alloys and but 15 pages to silicates, in spite of the author's statement (p. 286) that "the greatest single accomplishment of crystal chemistry has been the systematisation of silicate minerals." In the chapter on organic crystals very little attempt has been made to discuss the subject in a comprehensive way from the stereochemical point of view and in the case of Wermer's complexes (to which 16 undistinguished pages are given) the stereochemical considerations have been ignored. The book could have been more critical with advantage, for one finds the phrase "it is said" placed indiscriminately before statements which command universal agreement and those which have only the most slender support. Again there are frequent signs of careless writing, *e.g.* "The glass and zeolite structures are very similar. They differ fundamentally . . ." (p. 306). The treatment in detail, as in the general plan, is uneven. Work done in 1937 on one aspect of a subject receives full discussion, whereas on an equally important aspect nothing since 1930 will be mentioned. The organic chemist does not write the structural formula of cellulose as shown on p. 363 and it is misleading to convey the impression on p. 368 that inter-atomic distances in cellulose can be found to 0·01 A. However, whilst it is very difficult to write the ideal book on crystal chemistry it is easy to criticise the attempt of others. The reviewer, however, would venture the constructive criticism that if the book were reduced to half its size and provided with more and better diagrams of crystal structures it would better achieve the main purpose which the author has in mind. Nevertheless, it does reveal to chemists the power and the versatility of X-rays methods and thereby fulfils an important mission.

W. W.

Organic Chemistry. Vols. I and II. HENRY GILMAN, Editor-in-Chief. [Vol. I: pp. vi + 857 + lvi; Vol. II: pp. vi + lvi as in Vol. I, and 858–1890; with numerous figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. 37*s*. 6*d*. net each volume.)

IT is a well-known fact that those who are best able to write text-books on organic chemistry seldom do so. It is not a subject which lends itself to adequate treatment by an armchair chemist, and those most competent to write prefer to work in their laboratories. There comes a time, however, when for mere convenience it is worth while to write up a subject and present the student with the result. This is evidently what Gilman has decided, and in order to escape the boredom which cannot be dissociated from writing a book, he has looked round for colleagues. As a consequence, the new "treatise" is a series of monographs written mostly by well-known American chemists, and English-speaking people will welcome it wholeheartedly. It has the usual perfect set-up of a Wiley production and is a pleasure to handle quite apart from its contents. These rather take the breath away, so vast is organic chemistry to-day, and the only way to talk about the book is to take it chapter by chapter.

The account of alicyclic compounds is restrained and sufficient. That on aromatic-character is written by Fieser. He is an authority on the subject, and we find in this chapter what we should expect. Perhaps the directive

effect of groups in aromatic compounds could have been given more attention. The "Ingold-Robinson" theory gains faint praise and so perhaps misses salvation; but essential references are given which would enable a reader to find out more about "an elaborate electronic theory of organic reactions which has gained wide acceptance in England." There is an interesting and balanced account of group-migration. Many facts are mentioned which show that the subject is far from being "finished." I was surprised to find that what we call "current electronic theories" are not accepted in the United States, and hope the author will pick a few more of the locks for which he thinks there is no master-key. If this means an "open door" policy in chemical thought it will be all to the good. It is interesting to find Dr. Fieser indifferent to rather than critical towards our own views and to find that he regards the Pfeiffer-Wizinger (1928) theory of aromatic substitution as the most promising.

The account of stereochemistry, written as it partly is by Roger Adams, must necessarily be good. There is little time wasted on historical introductions: the authors get down to their difficult task, evidently taking the view that it is more important to say what is now known to be right than to say what is now known to be wrong. The general scheme is excellent and only minor points need to receive comment. The rate constant given for racemisation is really that for complete inversion. The statement that "biphenyls can be much more easily resolved than ordinary substances containing asymmetric carbon atoms" is a curious one, and some of us might have put "less" instead of "more." What is called the "equilibrium method of resolution" is really asymmetric transformation. This subject might have been given more attention.

Organo-metallic compounds are dealt with by Gilman himself. No one knows more than he does about this subject and he surveys it admirably. In the chapter on free radicals more might have been said in warning the novice against free radicals as an escape from precision of thought in explaining mechanisms. Under "Unsaturation and Conjugation," adequate references are given to the chief workers in different countries. The authors have made an interesting chapter out of what can be rather tiring material. "Open Chain Nitrogen Compounds" are well classified and described.

In "Molecular Rearrangements," an attempt has been made to present the opposing theories fairly. More examples of different types might have been mentioned—for instance, Chapman's work on imino-ethers is omitted. Adkin's chapter on "Comparison of Chemical Reactivity" has evidently involved much clear thinking and will be appreciated by anyone who is interested in the kinetics of organic chemical reactions.

Volume II begins with a good general account of the "natural amino acids" and after that Dr. Treat B. Johnson deals fully with pyrimidines, purines and nucleic acids. Here comes aneurin.

The fact that in less than 100 pages the writer of the chapter on alkaloids mentions almost all the important bases shows that no detail can be given. The account is, however, up-to-date and includes biogenetic syntheses. Now that the chemistry of anthocyanins has been so perfected, the chapter on this subject is shorter than it might have been ten years ago. It can be read with interest and understanding.

Carotenoids receive 80 pages sterols, bile acids, cardiac agluconos and sex hormones, 178 pages. Carbohydrates are dealt with in three chapters. The

first (77 pages) is a sound treatment of mono- and di-saccharides. The second (58 pages) is a useful account of derivatives of sugars; ascorbic acid and fermentations find their place here. The third section (60 pages) is mainly about cellulose, and this is a sound piece of work.

Electronic Theory provides a very well-planned chapter. The work of Ingold, Robinson and Sidgwick is fully reported. Both the static and the kinetic sides of the subject are gone into. The author devotes only 6 pages to tautomerism. It certainly cannot be said of him that he does not permit his readers to see the wood because of his ingenuity at afforestation. I should like to have this chapter as a separate volume and keep it with my two Sidgwicks.

Constitution and Physical Properties are treated under the two headings: physical properties which do and those which do not lead to determination of structure. No attempt is made to elaborate detail, but nothing fundamental appears to have been left out.

It is perhaps surprising to find 70 pages devoted to "rotatory dispersion," but the chapter contains much that would in most books have gone in under general stereochemistry. It deals excellently with the origin of optical activity and with the correlation of constitution with optical activity. The Walden Inversion is dismissed in 3 pages. This is almost prophetic.

The last chapter is by Pauling, on resonance, the nature of the chemical bond and the structure of molecules. This is a fitting end to an excellent advanced treatise, and I have no doubt that many people will, like children, read the last chapter first.

E. E. TURNER.

A Brief Introduction to the Use of Beilstein's Handbuch der Organischen Chemie. By E. H. HUNTRESS, Ph.D. Second edition. [Pp. x + 44.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. 5*s*. net.)

THIS volume is essentially the same as the first edition published in 1930, but contains extra matter dealing with all the 27 volumes, and the corresponding supplementary volumes, of Beilstein's *Handbuch*. A brief explanation is thus given of the system adopted for classifying aromatic and heterocyclic compounds. Careful study of Professor Huntress's book will materially aid beginners in the use of Beilstein, and more experienced workers will probably find it useful at times. It is always worth while to have something one can read in one's own language, and Dr. Huntress has made a good job of his attempt to make Beilstein quicker at yielding up its information. I think all libraries that have the *Handbuch* should have the Brief Introduction, which I feel would be more likely to be used if it had the same page size as the German work so that it could stand on the same shelf as the latter. It needs a stronger cover.

E. E. TURNER.

Agricultural Analysis: A Handbook of Methods, excluding those for Soils. By C. H. WRIGHT, M.A., F.I.C. [Pp. x + 343, with 8 figures.] (London: Thomas Murby & Co., 1938. 16*s*. net.)

THIS is a companion volume to the author's work on soil analysis. It deals with the analysis of fertilisers and feeding stuffs, including milk and milk products, as well as with insecticides and fungicides. The book is an excellent

reference volume for use in laboratories, where occasional samples of different types of these materials have to be dealt with as well as in routine laboratories. The author has adopted the principle of first discussing briefly in each section the possible methods, including those coming under Official Regulations in this country, those agreed upon by various committees set up for the purpose by such bodies as the Society of Public Analysts, methods adopted in America by the A.O.A.C., as well as other appropriate methods, and of then setting out in full detail certain of these methods. To some extent this involves what may appear to be useless repetition, but the procedure serves a useful purpose in enabling a worker to select the method most suited to his own purpose. The constituents, for the determination of which methods are given, are by no means confined to those which come under Official Regulations but cover a much wider field. The value of the book is enhanced by the references given to the original publications. A brief account is given, at the end, of the more commonly required solutions for volumetric analysis and their preparation together with notes on a few indicators. The book is well printed in clear type, appears to be remarkably free from errors, and contains good author and subject indexes. The book should be of value to those workers, particularly overseas, who do not have ready access to a well-stocked library.

W. Godden.

British Chemical Industry : Its Rise and Development. By Sir Gilbert T. Morgan, O.B.E., D.Sc., Sc.D., LL.D., F.R.S., and David Doig Pratt, M.A., Ph.D. [Pp. xii + 387, with 32 plates and 79 figures.] (London : Edward Arnold & Co., 1938. 21*s*. net.)

This book incorporates the substance of a series of Public Lectures delivered some three years ago. The subject matter is arranged in sixteen chapters and, as far as possible, in such a manner that each chemical manufacture is grouped with the naturally occurring raw material on which the trade depends. The wide scope of the book will be seen from the titles of these chapters, *viz.* Salt ; Sulphur ; Sand, Clay and Limestone ; Industrial Gases ; Selected Metallurgical Processes ; Borax and Phosphate ; Paints and Pigments ; Oils, Fats and Waxes ; Cellulose ; Coal ; Oil, Shale and Petroleum ; Explosives ; Dyestuffs and Intermediates ; Plastics and Rubber ; Industrial Solvents ; Fine Chemicals. Each section has been compiled in consultation with experts in the branches of technology concerned, and a glance at the names of these collaborators is sufficient to indicate at once the authority which lies behind this fascinating volume. This is no mere catalogue of manufacturing processes, but a skilful blend in which scientific and technical details, historical development, and economics are each given their share of attention. Without such a combination it is not possible to view British chemical industry in the right perspective, and the fact that this essential has been so well appreciated by the authors endows the book with much of its value. Thus, to those readers, who may have tended to regard the industry as a series of more or less water-tight compartments, the inter-relation and inter-dependence of its various branches is made clearly apparent.

It is perhaps not surprising that one should be tempted to criticise here and there the particular emphasis laid on the different aspects of the subjects treated. For instance, in the section on explosives, the well-merited tribute to individual members of the Staff of the Research Department, Woolwich,

might with advantage have included some reference to the work of those who were responsible for the actual production of explosives on such an unprecedented scale in the Great War, the work of whom contributed in so large a measure to the development of our chemical industry on its present scale. The book provides, however, an excellent picture, not only of the ramifications of this industry, but also of what it means to the welfare of the nation as a whole.

H. W. CREMER.

Asphalts and Allied Substances. By HERBERT ABRAHAM. Fourth edition. [Pp. xxiv + 1491, with 333 figures.] (New York: D. van Nostrand Co., Inc.; London: Chapman & Hall, Ltd., 1938. 60*s*. net.)

As compared with the third edition, which was published in 1929 and reprinted in 1932, the fourth edition of this "asphalters' bible" shows an increase of 331 pages of text, and references to patents and to general literature are greatly enlarged; and as there is an acknowledgment of assistance in the first edition, and there is none in the third and fourth, the personal activity of the author has been prodigious.

There are no new sections, but there is a great expansion of existing ones and a small amount of rearrangement and "promotion" consequent on this increase of collected matter. We find, too, fully developed, the typically American desire to present the subject in its complete aspect from start to finish, so that much space is taken with descriptions and diagrams (many from the 1929 edition) of the distillation of petroleum and of coal and shale, and the refining of products. This seems to duplicate much information found in other books with no advantage except that a limited amount of such information is included within one pair of boards. This is, of course, a matter of opinion, but it is worth referring to because so much space might have been used for more immediate "asphaltic" advantage, such as the graphs of properties which have appeared only in an earlier edition, and a greater expansion of the account of emulsions to be mentioned again later.

We note in passing the compliment paid to Spielmann and Hughes' *Asphalt Roads* (1936) in the reproduction of the two illustrations associated with Boeton asphalt. And we gravely doubt whether Sir Walter Raleigh was really describing the "so-called 'Pitch Lake'" in his famous lines about the Tierra de Brea.

Somewhat disappointing is the treatment of Emulsions, which have grown in importance to a greater degree than is shown by the increase of only four partly rewritten pages. There is no scientific discussion of the crucial matter of their behaviour with mineral matter. Only two American stability tests are mentioned, which are strongly opposed in this country, and there is no mention of the English Lability Test; in fact, one would gather that only America and Germany have done any work on the subject.

There is serious criticism to be made in connection with the organisation of references and indexes. In this matter there are two schools of thought. The one is, that all references should be in one continuous numbered series, so that any one sought for may be found without the delay of having first to find the correct group of references (in the present case there are 37 of them). On the same grounds, name and subject indexes should be combined, so there is only one Index to be searched. And the names should be linked with the

page in the book on which the associated work appears and with the detailed number in the bibliography. The only drawback in this scheme is an absence of references collected by chapter; but even this is not insurmountable.

This volume is an example of the second school of thought, with added disadvantages of its own. The Bibliography is good and full, but it is divided into sections, (A)–(J), which have no relation to the divisions of the book. "References" are collated according to chapters and collected together at the end of the book, which has its obvious advantage, but greatly prolongs a search. The great defect is that there is no Name Index, so that it is only with great labour that it is possible to ascertain whether So-and-so has written or been quoted in connection with any particular subject.

Now that these matters for citicism have been considered, we can express our warm praise and admiration of this tremendous accumulation of facts, with apparently complete freedom from error. This edition has entrenched the book still more firmly in its unrivalled position in the asphalters' library.

PERCY E. SPIELMANN.

The Principles of Motor Fuel Preparation and Application. Vol. I. By A. W. NASH, M.Sc., M.I.Mech.E., M.I.Chem.E., F.C.S., F.Inst.Fuel, and D. A. HOWES, D.Sc., M.I.P.T. Second edition. [Pp. xiv + 628, with 173 figures, including 15 plates.] (London: Chapman & Hall, Ltd., 1938. 36s. net.)

IT is now four years since Volume I of this treatise was published. Its immediate and great success was shown firstly by the unanimous approbation of the reviews it received, secondly by its appearance on the reference book-shelves of practically every organisation which was in any way connected with the petroleum or motor transport industries, and now thirdly by the call there has been for a second edition. Such a success must have been very gratifying to the authors and to their publishers, and it will be equally pleasing to the wide circle of readers who have had cause to refer to the book for some item from the enormous store of information it contains.

It is hardly necessary to recapitulate in detail the main contents of this work. Briefly, it deals with every aspect of the production of fuel for the spark-ignition motor (testing and analysis are dealt with in Volume II) from the distillation of the crude oil and the stripping of gas and cracking of heavy oil which together give us our raw fuel material, through the stages of refining treatment to the final storage and distribution. The production, properties, uses, advantages and disadvantages of alternative volatile fuels such as benzole, alcohols, shale spirit, hydrogenated coal spirit and other synthetic materials are each dealt with with equal thoroughness.

Even in the short time of three years, however, great advances have taken place in the petroleum industry, and it has been necessary to incorporate these in the new edition by extending and modifying some chapters and adding new ones. The development, for example, of the catalytic polymerisation processes which convert gaseous olefines (hitherto waste by-products of the cracking operation) into motor fuels of high anti-knock quality and even into super-high-octane aviation fuel has necessitated an entire new and lengthy chapter. The alternative "pyrolysis" processes which by non-catalytic methods have also successfully utilised the waste gases of the petroleum industry for the manufacture of high-grade petrol also receive their due share of description, and this new chapter ends appro-

priately with a general comparison of the alternative methods now available for the production of synthetic gasoline from petroleum gases.

While this section is the most prominent addition to the first edition, almost every chapter has been brought up to date by the addition of new material. Only one section in which much new knowledge has been acquired in recent years seems to have been overlooked, for the latest reference in the bibliography of natural gasoline is 1930, and the extensive work of recent years on the methods whereby greater quantities of stable natural gasoline can be incorporated in motor spirit without increased risk of vapour lock and with great improvement as regards starting and warming up, has not received the attention it deserves.

No book, however, is perfect, and with such a high general standard it is ungracious to cavil at details. We can congratulate both the authors and their public on the advent of this new edition, and hope that the successor to Volume II will appear in due course.

J. B. Thole.

GEOLOGY

An Introduction to Geology. By A. E. Trueman, D.Sc., F.G.S. [Pp. x + 258, with 133 figures.] (London: Thomas Murby & Co., 1938. 4*s*. net.)

Prof. Trueman's enthusiastic advocacy of the teaching of geology in schools has prompted him to write this book for their use. He has aimed at introducing the reader to an outline of the whole subject, and accordingly we find that the book embraces the principles of physical geology, of structural geology and of stratigraphy. The last of these is a most welcome feature and is admirably done. But the necessity of introducing a large amount of descriptive matter makes the whole treatment condensed, and one fears that the very brevity in such subjects as mineralogy, petrology and palæontology will almost vitiate the value of these chapters.

The treatment of physiographic processes and their effects on and relations to the landscape makes a useful introduction to the subject for geography students; but the strictly geological significance of the present-day processes as a key to the interpretation of the past is often missed; and the reviewer feels that the hypogene processes and igneous petrology receive but scant attention. Perhaps this is because these aspects of geology are not so easily illustrated in this country and also require a type of study that is beyond the scope of a school curriculum. Certainly Prof. Trueman has endeavoured throughout to show ways in which the schoolboy can acquire first-hand contact with the geological phenomena under discussion.

"The arrangement of the sedimentary rocks" is dealt with fully, but both the relation of this to earth movements and the geological interpretation of mountains are hardly touched upon.

Chapter XV on "Some Applications of Geology" might well have laid more stress on the present unsatisfied demand for professional geologists.

The book is written partly for the schoolboy or girl and partly for the teacher, for one of its aims is to indicate ways in which geology can be taught in schools. This makes the treatment a little uneven: nevertheless, the book should stimulate interest in this science by its comprehensive treatment of the subject, and particularly by the stress it lays on the geological interpretation of scenery and on stratigraphy, that branch of

geology without a knowledge of which no true conception of geological time can be acquired.

L. J. W.

BOTANY

Sixty Years of Botany in Britain (1875–1935): Impressions of an Eye-witness. By F. O. Bower, Sc.D., LL.D., F.R.S. [Pp. xii + 112, with 14 plates.] (London: Macmillan & Co., Ltd., 1938. 10*s*. 6*d*. net.)

Some sixty years ago the "New Botany" came into being in this country. The pioneer work of Hales, Grew, Brown, Griffith, and other British botanists together with the work of the great Continental botanists was, if known, unheeded and had no effect on the teaching in the universities: taxonomy was supreme and its leaders had no sympathy with the wider outlook. Henfrey by his translations and writings tried to effect a reformation and failed.

The Education Act of 1870 introduced the teaching of science in the National Schools, but the provision for a time was hardly effectual since there were too few properly qualified teachers. This dearth led to the founding of the Normal School of Science (now the Royal College of Science) with Huxley as Dean: his hands were free and he organized the best general training in Science yet devised.

Hitherto, Botany had its place mainly in the preclinical training of medical students; its teaching was by lectures at best illustrated by diagrams, detached plates from such works as Curtis's *Flora Londinensis*, by preserved plants and sometimes living material. The only practical work was the dissection of flowers during the lectures, and sometimes the students were taken into the field.

Huxley at the Normal School of Science changed all this; great emphasis was laid on laboratory work and the student became an active investigator rather than a passive listener. The work of organising the botanical side of the biological course was entrusted by Huxley to Thiselton Dyer. Thus British Botany was inoculated and the leaven worked: students were enthusiastic and the spirit of investigation became so engrained that (some forty years ago) a student would deny having knowledge unless he had actually worked at the problem.

Professor Bower, the doyen of British botanists, was Dyer's demonstrator in the occasional Summer Courses at South Kensington; for two years he was assistant to Daniel Oliver at University College, London, and was entirely responsible for the laboratory work; and in 1882 he was appointed lecturer at the Normal School of Science. He thus played a happy part in introducing the "New Botany" to Britain and in the autobiographical part of the volume under review he gives a most interesting account of his impressions during the period. The picture is completed by a number of biographical sketches, illustrated by excellent portraits, of Huxley, Thiselton Dyer and Vines, who took the leading part in the renaissance, and of Marshall Ward—the first fruit of the New Botany—Bayley Balfour, Scott, Francis Darwin, and others who by their teaching and research stimulated the development of the subject. Of these sketches, that on Vines is the best.

The book ends with a chapter on the Morphological Kaleidoscope and a summary entitled Past—Present—Future.

Prof. Bower has written an engaging book which will appeal to all who are interested in the subject. For my own part, here and there I wished for more, but there is an art in omission, as Sam Weller said "she'll vish there wos more, and that's the great art o' letter writin'."

T. G. H.

Plant Ecology. By J. E. WEAVER and F. E. CLEMENTS. Second edition. [Pp. xx + 601, with frontispiece and 271 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1938. 30s. net.)

THE first edition of this work was published in 1929, since when the extent of our knowledge in this field has considerably increased. This is reflected in the addition of some 83 pages to the text and over 400 references to the bibliography. The latter now comprises 1,035 citations. The most important increment is that concerned with conservation of soil, of water and of wild life. Despite these additions the present work is actually slightly thinner than the earlier volume and but very little heavier. It is satisfactory that the call for a new edition has afforded the authors an opportunity for incorporating recent work and bringing the text up to date. Those who require a general and not too detailed account of the subject will find this a useful text, which contains also practical advice as to methods of study, whilst the extensive bibliography will serve as a guide to further reading.

E. J. S.

Herbals: Their Origin and Evolution. By AGNES ARBER, M.A., D.Sc., F.L.S. Second edition. [Pp. xxiv + 326, with 27 plates and 131 figures.] (Cambridge: at the University Press, 1938. 21s. net.)

THE first edition of this work published in 1912 has been out of print for some years and had become a scarce item in the second-hand book market. It is therefore with great pleasure that we welcome a new edition of this classical work on a subject which touches so many fields of interest.

Whether we are attracted by herbals, because of the part they played in the history of medicine; because they represent some of the earliest efforts towards the scientific study of plants and embody the beginnings of botanical classification; or by reason of the record they afford of the evolution of the delineation of plants, we shall find in these pages a scholarly and pleasing presentation of the relevant facts.

The period mainly dealt with is that of the printed herbals from the incunabula, such as those of Bartholomæus Anglicus (1472) and the well-known Ortus Sanitatis (1491), to the English herbals of the later half of the seventeenth century of Cole (1656) and Turner (1664).

The author devotes a chapter to the "Doctrine of Signatures and Astrological Botany," which is introduced with the remark that "it may be well to call to mind the existence of more than one backwater, connected indeed with the main channel, but leading nowhere." To the latter statement we would, however, demur, for the doctrine of signs illustrates peculiarly the value that may accrue even from a false hypothesis when honestly held. That many did honestly believe in the doctrine of signs there can be no doubt and it is probably difficult to overestimate the importance for the study of plants of the meticulous observation which the search for these "signs" produced.

Not only by her text, but also by the numerous and well-chosen illustra-

tions, Mrs. Arber enables her readers to apprehend alike the interest and artistic achievement of these early writers and to possess something of the essence of the herbals even if we cannot afford their substance.

E. J. S.

Pathologie der Pflanzenzelle. Teil II: Pathologie der Plastiden. By ERNST KÜSTER. Protoplasma-Monographien, Band XIII. [Pp. xii + 152, with 91 figures.] (Berlin: Gebrüder Borntraeger, 1937. RM. 16.– bound.)

PROF. KÜSTER is well known as the leading authority on the structure of plants exhibiting pathological conditions. The present volume is the second part of a comprehensive review by him of the pathology of the plant cell, the first part of which, dealing with the pathology of the protoplasm, appeared ten years ago. In the volume now under review, the pathology of plastids is dealt with in two chapters, the first being concerned with changes in the shape, the second with changes in structure of different kinds of plastids under a variety of conditions producing pathological effects. While the chromatophores of algæ figure largely in these considerations information is also presented with regard to the plastids of mosses and higher plants. Changes in colour of plastids are dealt with in a short appendix.

This little book, written by a master of the subject, will be of interest both to anatomists and physiologists interested in the plant cell. The subject matter is clearly arranged and is illustrated by a wealth of exceptionally good illustrations, many of them original. The book is provided with a useful bibliography of about 250 references and with adequate subject and author indexes, while it is produced in the attractive style now familiar to readers of Protoplasma monographs.

W. S.

Forest Pathology. By JOHN SHAW BOYCE, M.A., M.F., Ph.D. American Forestry Series. [Pp. x + 600, with 216 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1938. 30*s*. net.)

THE publication in the U.S.A. of two important text-books on forest pathology within eight years is evidence of the increasing interest which is being taken in the welfare of forest trees. America has indeed suffered severely in recent years as a result of tree diseases, particularly from those caused by fungal parasites of foreign origin, which have wrought great havoc in the native trees—blister rust has threatened the existence of the valuable white pine, chestnut blight has practically wiped out the American chestnut, and now a very expensive campaign is being waged against the recently introduced Dutch elm disease to which the American elm is highly susceptible. Prof. Boyce's book deals comprehensively with the various diseases to which forest trees, both coniferous and broadleaved, are liable. He classifies these diseases according to the part of the tree which is affected, *e.g.* root, foliage and stem diseases, and there are separate chapters on seedling diseases and on diseases caused by flowering plant parasites such as mistletoe. Estimates are given of the amount of damage caused by the principal diseases, and measures for their control are indicated where these are feasible. While the book deals primarily with American conditions, the author makes many references to European problems and has surveyed very fully all the available literature. His book will be an indispensable guide to students and a most useful reference book to research workers in this field. The concluding

chapters deal with the deterioration of felled timber in storage and of wood used for constructional purposes, and with the general principles of forest disease control; while in one of the appendices is given a very useful list of the common names of American trees with their equivalents. The numerous illustrations are well reproduced and the general get-up of the book is very pleasing.

W. P. K. F.

ZOOLOGY

Genmutation. I: Allgemeiner Teil. By HANS STUBBE. Handbuch der Vererbungswissenschaft, herausgegeben von E. Baur und M. Hartmann, Vol. II F. [Pp. iv + 429, with 90 figures.] (Berlin: Verlag von Gebrüder Borntraeger, 1938. RM. 60.– Subscription price: RM. 48.–)

THE preface explains that this volume, entitled "General Part" is to be followed by a "Special Part" which will contain among other matter a systematic list of mutations. Genetics is a science evolving so rapidly that the frontiers of its various parts are still very ill defined. A discussion of the subject matter included under "Gene Mutation" in its wide sense would encroach on almost every branch of genetics. The author has therefore had to limit his scope in a manner often quite arbitrary, and as a result the syllabus of his work might appear strange to some readers. It includes, for example, such subjects as the evolution of dominance, incomplete realisation of factors, and the theoretical segregation of autopolyploids, whereas chromosome aberrations are only mentioned in connection with position effect.

The book starts with a survey, in historical order, of reported cases of mutation, beginning with the year 1590. Following this is a discussion of the gene concept and of the properties of mendelian factors, winding up with unstable genes and multiple allelomorphs. The remainder, almost one-half of the book, is devoted to a quantitative consideration of mutation, and by far the greater portion of this section concerns itself with mutations produced artificially. The review of this only ten-year-old, but already very extensive subject is particularly excellent. The last pages discuss the physics of the action of high frequency radiation, the magnitude of the target region of individual mutational steps, and, based on these, the various attempts to estimate total gene number and gene size.

The author states that he has reviewed the literature up to the middle of 1937; he has done more than this, for the most recent developments are most thoroughly assimilated, and the whole work is really up to date. But a reader not already familiar with the subject will not find it easy to discriminate among the wealth of information presented: the author's attitude to indifferent work is philosophical rather than critical.

This excellent work has 90 illustrations, and contains a bibliography of well over 1,000 titles, arranged under chapter headings. There is no index—a deficiency partly remedied by a classified list of contents at the beginning.

A. C. F.

The Measurement of Linkage in Heredity. By K. MATHER, Ph.D. Methuen's Monographs on Biological Subjects. [Pp. x + 132.] (London: Methuen & Co., Ltd., 1938. 4*s.* 6*d.* net.)

BEFORE the rediscovery of Mendel in 1900, and indeed since that date in some quarters, heredity was spoken of as a "force." According to Galton even,

it was a force whose determinate action, operating equally on all members of a family, could be calculated according to a simple arithmetical law. Now we know that all differences inherited by sexual reproduction are particulate or atomic. This principle carries with it the consequence that the character of an individual offspring of a parent which is hybrid or heterozygous for a heritable difference cannot be predicted with certainty. Every such parent produces two kinds of gametes, and it is a matter of *chance* which of these kinds shall give rise to a particular offspring. Furthermore, any collection of such offspring must be a *sample* of a population arising by a series of such chances.

To many biologists the admission of probability instead of certainty was no less distasteful because its mathematical consequences were unforeseen. But, willy nilly, we now have to admit that all analyses of populations which take into consideration (as they usually need to) the inborn differences of individuals depend for their rigour on statistical treatment. This is true whether the analysis is carried out on the progenies of mendelian breeding or of darwinian selection ; while for the study of uncontrolled human heredity it is the key of the whole problem.

Dr. Mather's book is an account of how to deal with all the possible combinations of mendelian and post-mendelian behaviour that are implied by the chromosome theory of heredity. The title of the book gives an inadequate notion of its scope, for reasons we have just seen. The theory of sampling, the measurement of heterogeneity and the method of maximum likelihood which Dr. Mather explains for his special purpose and with special examples are the basis of all statistical treatment in biology.

It may be said that it is unnecessary for everyone who is to solve important biological problems to be a master of this technique. That is quite true. What is necessary is that he should know the scope of the technique and in what circumstances he needs to turn to the specialist who knows the job ; and furthermore that there should be a specialist to whom he can turn with confidence. As a guide for the first purpose and as an introduction for the second this book is indispensable.

C. D. DARLINGTON.

Bird Flocks and the Breeding Cycle : A Contribution to the Study of Avian Sociality. By F. FRASER DARLING, Ph.D., F.R.S.E. [Pp. x + 124, with frontispiece and 1 figure.] (Cambridge : at the University Press, 1938. 6*s*. net.)

THE publication in 1937 of his book *A Herd of Red Deer* placed Dr. Fraser Darling in the front rank of writers on animal behaviour whose work is based on field observation. That intimate study of mammalian society he has now followed up with a short treatise on flock behaviour as it affects the breeding cycle of certain species of British birds. Many writers have unduly spun out such an essay ; but Dr. Darling writes with admirable simplicity and terseness. His main thesis is that " the social group and its magnitude in birds, gregarious at the breeding-season, are themselves exteroceptive factors in the development and synchronisation of reproductive condition in the members of individual pairs and throughout the flock." This theory, based on some of Marshall's recent conclusions supplemented by his own patient field-studies, he works out with convincing lucidity.

Dr. Darling's researches (mainly devoted to two species of gulls) were

made during visits of some months' duration in two successive years to the small uninhabited Priest Island, one of the Summer Isles of the North-West Highlands, lying six or eight miles off the mainland. Here his intensive study in 1936 of four separate Herring Gull colonies, ranging from 4 to 90 birds, and two Lesser Blackbacked Gull colonies with 18 and 80 birds each, and, again, in 1937, of two colonies of the former species with 6 and 150 birds each, and two of the latter with 30 and 120 birds each, clearly show that the larger colonial groups were more successful in their breeding results than the small ones. He concludes that any given colonial species "by an extended nesting range, though with a no less close sociality, is more successful than another; and that when colonies are very small, they may be wholly unsuccessful."

Throughout his essay the author rightly insists on the enormous potency of environment, a factor which some modern works on animal behaviour have tended unduly to neglect. This most stimulating book should be read and enjoyed by the field ornithologist and the student of animal behaviour alike.

BERTRAM LLOYD.

Proceedings of the Fourth International Locust Conference, Cairo, April 22, 1936. [Pp. ix + 96 + 390.] (Cairo: Government Press, 1937.)

It is, on a first reading, difficult to summarise the contents of this large volume, for besides a report of the formal activities of the conference, there are fifty-one separate papers dealing with every aspect of current locust investigations. Much of the material, however, is of value only as propaganda and tends perhaps to obscure the real need for further action.

It cannot, indeed, be said that the locust problem is solved—the need for additional observations on the ecological and physiological conditions determining phase-transformation is pointed out in several places (notably in Appendix 7 by Dr. B. P. Uvarov)—but for practical purposes so much is known of the genesis of Locust outbreaks that lack of scientific knowledge is probably not now the main factor limiting their control. The problem, like many others that require international co-operation, is difficult because it involves *immediate* expenditure of money (not in reality of large sums in relation to the prospective gains), and also, what is even more troublesome, the financing by one or a few countries of measures which may benefit many. The difficulty arises in this form because locusts have certain permanent breeding sites from which they invade the whole surrounding country at irregular intervals. After an invasion, breeding may persist for a time outside the permanent sites and control measures hitherto have been purely palliative, being aimed at destroying the invading swarms. Such destruction is not only rather ineffective but also does nothing to prevent the indefinite continuation of the trouble. The only rational measures for control consist in patrolling the limited areas of permanent breeding and attempting to prevent outbreaks from starting. It is probable that this could already be done in many places and, with better ecological data and a more complete mapping of the breeding areas, locusts might cease to be major pests. Meanwhile, the breeding areas are mostly in rather inaccessible places where a scientific staff would not normally be maintained; it might well be regarded as reasonable that all countries likely to benefit from the stopping of invasions should contribute to the cost of patrolling the places where they start. An

attempt to concert steps in this direction formed an important part of the activities of the conference. In the published proceedings themselves, matter of general interest is considerably diluted by reports of the routine activities of government departments. The volume, in fact, might be described as a human as well as a scientific document.

O. W. R.

Lac Cultivation in India. By P. M. GLOVER, B.Sc. [Pp. viii + 147, with 25 plates, including 2 coloured.] (Namkum, Ranchi, Bihar, India: The Indian Lac Research Institute, 1937. Rs. 2/–.)

THIS volume is described as a "Second and Revised" edition of the author's *Practical Manual of Lac Cultivation* which was published in 1931. It is, in fact, practically a new work containing 147 pages, whereas the previous work contained only 81. This increase is due in part to a more detailed examination of the financial and economical aspects of the subject, but the experience accumulated at the Lac Institute during the last six years has been fully utilised, with the result that the present book is a much more critical study of the subject than the earlier one, which was primarily concerned with matters of immediate practical importance to the cultivator.

The chief lac-producing area in India is Chota Nagpur in Bihar, which is responsible for more than half the total of Indian lac production and naturally conditions obtaining in this area occupy a proportionally prominent place in this book, but special chapters (XV, XVI, XVII, and XVIII) are devoted to other areas, so that a very accurate picture of the whole industry is given.

With regard to the practical aspects of lac cultivation the book has been considerably amplified, and the reasons for introducing new methods lucidly explained. The distribution of the illustrations through the work instead of grouping them at the beginning is an improvement, and the figures in the text illustrating practical methods advocated by the Research Institute are particularly good.

An interesting feature of the book is the use made of "odds and ends" of information as to local methods and experience accumulated in the course of the work at the Lac Research Institute. The whole treatment of the subject is such that the reader is conscious of a close and sympathetic understanding between the cultivator and the Research Institute which is much to the credit of both parties and argues well for the future of the industry.

Voelcker, on his second visit to India, expressed astonishment at the way in which the Indian agriculturist, usually considered a slave to tradition and stereotyped methods, had availed himself of the results of scientific investigation. There are signs that the lac cultivator is equally open to advice provided it is given tactfully and in the right form. There are, of course, special difficulties in the case of the lac industry, one of the chief being that the actual production of stick lac (the raw material of the industry) is for the most part subsidiary to normal agricultural activities with the result that intelligent and systematic cultivation is difficult to establish.

This book in conjunction with the equally important work being carried out in Europe and America on the use of lac should do much to place the lac industry on a firm basis by showing the cultivator how best to produce a raw material of consistently high quality, which will be amenable to the more scientific methods of utilisation now being developed.

T. H. B.

Dairy Bacteriology. By BERNARD W. HAMMER, Ph.D. Second edition. [Pp. xiv + 482, with 45 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. 25*s*. net.)

PROF. HAMMER has made a careful selection of the more recent advances in dairy bacteriology and has incorporated them successfully in the second edition of his book. This has been no easy task in view of the large volume of research which is being conducted in the subject. He has been wise in adhering to his original plan of omitting controversial points and detailed descriptions of organisms. The chapter on the bacteriology of butter cultures has been revised and brought more into line with modern concepts. The production of diacetyl and its relation to the flavour of butter has been clearly explained. In the chapter on the bacteriology of cheese many interesting and important points regarding the function of *Penicillium roqueforti* have been included. In some sections of the book, however, too many examples are given to illustrate a point, for example, two whole pages are devoted to the effect of clarification on the bacterial content of milk, most of which consists of citations. This might have been condensed with advantage to the reader. In the chapter on the spread of disease through milk many examples are cited in detail of outbreaks of disease. Valuable as these details are they might have been limited without the example losing its significance.

Prof. Hammer's book is a valuable contribution to dairy bacteriology and can be recommended with confidence to students and others as a standard work.

C. H. CHALMERS.

PHILOSOPHY

Free Will or Determinism. By M. DAVIDSON, D.Sc., F.R.A.S. With Foreword by SIR RICHARD GREGORY, Bart., F.R.S. [Pp. xvi + 203.] (London: C. A. Watts & Co., Ltd., 1937. 10*s*. 6*d*. net.)

THE title of this book, which is written by a clergyman of the Anglican Church who is also a distinguished amateur astronomer, is hardly a fair index of its contents. "Readers", as the author truly says (p. 80), "will expect something to be said on the question of the freedom of the will, irrespective of any views regarding indeterminacy or otherwise in the physical world. I do not propose dealing with this problem except in a very brief fashion." It may be added that the discussion even of the physical and biological world is conducted more in relation to the question of teleological or predeterministic explanations than to that of human free will. The plan of the book is briefly as follows. The philosophical aspects of modern physics are briefly described in the form familiar to many readers through the works of Sir Arthur Eddington, and criticisms of Eddington's position by Cohen, Levy, Dingle and Joad are successively summarised and discussed. Some aspects of biology are then presented, with special reference to the mechanist-vitalist controversy, and the book concludes with a consideration of the existence or otherwise of absolute standards of goodness, beauty and truth, and some comments on religious experience and mysticism.

The treatment throughout gives evidence of wide reading, openness of mind, and freedom from *a priori* dogma (or conviction, as the case may be) which is certainly unusual in the writings of Anglican clergymen and will be regarded as laudable or reprehensible according to one's theological standpoint. The author has been very chary of expressing his own opinions,

preferring for the most part to present the views of others and to invite the reader to draw his own conclusions. His views may be summed up in the statements that the recent progress of physics has contributed nothing to the problem of free will (pp. 13, 187), that "some day the final word on vital phenomena may be spoken by the physicist" (p. 141), and that there are no absolute standards of goodness and beauty, though he has "an almost intuitive knowledge that truth is an absolute standard" (pp. 16, 150). With regard to the last point, however, he shows no tendency to take up the extreme logical positivist position that moral and æsthetic judgments are nonsense, and it may be assumed that he would regard relative standards of goodness and beauty as having meaning, and probably a very important meaning. Neither does he mention the conventionalist theory of truth held by some modern logicians. The value of the book, however, lies not so much in its conclusions as in its potentiality as an impetus to thought. The numerous citations from scientists, theologians and philosophers, and the pointed comments thereon, can scarcely fail to provoke an intelligent reader into agreement or dissent, and start a productive train of meditation in his mind. The book may be strongly recommended to those whose power of original thought is stimulated by the thoughts of others.

H. D.

MISCELLANEOUS

Science for the Citizen. By LANCELOT HOGBEN. [Pp. 1120, with 480 figures.] (London: George Allen & Unwin, Ltd., 1938. 12*s*. 6*d*. net.)

TO-DAY there is great danger of science degenerating into the naïve mysticism of a Jeans or an Eddington, or the wish-fulfilment "science" of National Socialism, which has prompted M. Planck to state "the importance of a scientific idea is often due not so much to its truthfulness as to its inner value" (*Weltanschauung* = world outlook). This attitude has, for political ends, actually produced a new racial "science" advancing such theories as: "No person of Jewish blood can think German. Therefore, when a Jew writes German he lies," and that Einstein's Theory of Relativity must be denounced because a Jew can only produce non-Aryan and therefore false scientific theories!

If man is to enjoy the fruits of science this nonsense must be exploded, and Hogben's book could leave no reader in danger of deception, for it shows science as a whole, as one unit in the complete structure of human culture, not as a number of isolated cells. It shows science and technology developing side by side, as determined by the social structure of society and the technical level of production appropriate to that structure, thus teaching us how we can ensure that science is used to enlarge and not destroy life.

Even if the reader skips the "harder" passages, the clear exposition, helped by Horrabin's excellent diagrams, will equip him well to understand contemporary thought. All teachers of science who wish to bring reality into their instructions should read the book. On particular points I can only criticise in my own field; and here I think it a pity to have used the word *poundal* in mechanics, and that the author has not given sufficient credit to the early engineers of Sumer, Egypt, India, Crete, who laid the foundations upon which the Greeks could build.

JOHN CASE.

Science and Mechanisation in Land Warfare. By DONALD PORTWAY. [Pp. x + 158, with 8 figures.] (Cambridge: W. Heffer & Sons, Ltd., 1938. 6s. net.)

THIS book is a collection of essays written to show the influence of modern scientific development on the conduct of war. It is intended, primarily, as a text-book for those students who are preparing for the "Military Special" Examination of Cambridge University, but it will also be of value, not only to those who are taking courses of military study at the Universities, but also to the officers and cadets of the senior and junior divisions of the O.T.C., as well as to the officer-producing groups of Territorial Regiments.

The author, a Territorial and O.T.C. officer of long experience both during the war and subsequently, is fully alive to the danger of a military training dependent solely on the past, the sort of training which relies implicitly on the creeds and canons of the past war and which offers blind obedience to the training manuals of the day without thought or imagination.

The essays deal with such subjects as the use of railways in war, mechanisation, chemical warfare, meteorology and aircraft problems. There is also an interesting and detailed account, such as would be expected from the University Lecturer in Engineering, of the varied duties of the Corps of Royal Engineers, as well as an introduction into modern artillery methods including survey sound ranging and flash spotting.

The book is always interesting and encourages the reader to a further study of the subjects presented by a useful bibliography at the end of each chapter. The first essay on Fundamental Scientific Principles is perhaps the least successful in the book, but it is doubtful if so vast a subject could ever be dealt with satisfactorily in the limited space available. The final chapter on "Some problems of Personnel" should be particularly interesting to O.T.C. officers, and Lieut.-Col. Portway's criticisms and suggestions are always worthy of careful reading and consideration. Altogether the book is one which can be recommended to anyone who is interested in the military problems of to-day; it should find a place in every military library.

P. C. B.

Science and Psychical Phenomena. By G. N. M. TYRRELL. [Pp. xvi + 379.] (London: Methuen & Co., Ltd., 1938. 12*s*. 6*d*. net.)

FOR anyone who desires an up-to-date account of the work that has been carried out upon the scientific investigation of the "borderland" phenomena of the human mind, Mr. Tyrrell's *Science and Psychical Phenomena* will be of very great interest. There is no doubt that much that orthodox psychology has not studied is worth close observation as well as experimentally planned research. Mr. Tyrrell's work presents his readers with the results of both. He has drawn upon the *Proceedings of the Society for Psychical Research*, as well as upon laboratory experiments such as those of Professor Rhine of Duke University and his colleagues in "card guessing" (using Zener cards) for his data. The experiments in question have been repeated in this country by Dr. Thouless without success; and, indeed, Rhine himself found that the power of "extra-sensory perception" fluctuated from time to time in his own subjects, and usually ended by disappearing. Such fluctuation leads one to question the conclusions that are based upon the statistics of samples extracted from large numbers of trials. More interesting than these are Mr. Tyrrell's own experiments with the device he calls "the

pointer apparatus". He used a set of five boxes, open on the side facing himself, closed on the other. They were placed in a row, with a screen separating him from his subject, so that he could indicate any one of the boxes at random by means of a pointer, in the absence of any ordinary perception of what he was doing on the part of the subject. The latter had to find the correct box into which the pointer had been inserted. According to his results the successes, with one subject at least, were statistically significant. As several disturbing factors, such as that of telepathy, might have obscured any evidence for true "extra-sensory perception", the technique of the experiment was altered so that this should be eliminated. In the end, an electrical apparatus was devised by the use of which the experimenter did not himself know which box of the five would be indicated when he pressed one of the keys. This set a circuit for a light that would glow only after the subject had opened the lid of her chosen box. Even then, according to the author, the results showed a high and statistically significant success. Mr. Tyrrell's method of experimentation was very ingenious and is to be commended as a thoroughly scientific procedure; but proof that one, or even a few, "sensitives" yield results that are not generally recognised is hardly more than an indication that any such mental power as "extra-sensory perception" may be universally possessed. It is well known that individual differences in mental capacity obtain, and their measurement is one of the chief preoccupations of present-day psychologists; but, so far as science is concerned, sporadic, occasional and unpredictable events must be referred to already known causes, or await a further scientific probing in which every possible influence must be taken into account. This seems really to be the position of present-day psychical research. Where fraudulence has been positively eliminated, such explanatory concepts as those of thought transference or telepathy, communications from discarnate human spirits, diabolical intervention, and the like, may be introduced to account for the phenomena observed. These may be reasonable hypotheses; but in fact many other possible and less "occult" influences to account for the significance of the statistics must be considered and eliminated in order to be certain that any one of such hypotheses is acceptable. The correct interpretation of statistical results of *any* kind is as vitally important as the obtaining of them.

F. A.

South Latitude. By F. D. Ommanney. [Pp. xii + 308, with 16 plates.] (London, New York, Toronto: Longmans, Green & Co., 1938. 9*s*. 6*d*. net.)

During the last ten years the Royal Research Ship *Discovery II* has made five voyages to the Southern Ocean to investigate the life history of the whale, and associated matters. This book is a description by one of her zoologists of some of his experiences during that work. It is not a scientific report; but a very vivid account of the life of some of the workers. The difficulties and discomforts of life and work in a humid atmosphere at temperatures constantly oscillating about the freezing point are well described. Those who have experienced them know that these are worse than the cold of Polar Regions or the heat of tropical deserts. But the author's view that the general public is impressed only by startlingly low (or high) temperatures is probably correct.

We can follow the work on the slippery "plan" on a raw morning of

the Antarctic summer while the huge carcasses pass through the factory which extracts the oil and other useful material, and the swarming birds screech and flap round as the waste is thrown or washed back to the sea. There are equally live accounts of the way of a ship at sea, beating against the gales of the Horn, taking a "station" where all the skill of the navigators is strained to keep her steady while the nets and instruments go out and are wound back on the miles of wire. This is a real travel-adventure book, a saga of the polar seas.

C. B. F.

The American Indian. By CLARK WISSLER. Third edition. [Pp. xviii + 466, with frontispiece and 82 figures, including 8 plates and 1 folding plate.] (New York and London: Oxford University Press, 1938. 18*s*. net.)

THE first edition of *The American Indian* appeared twenty years ago. It covered the whole field of American anthropology, as far as was possible in one volume. It was both extremely learned and also easy reading, and it has in course of years become an authoritative text-book. A great deal of work has been done since then, and nowhere more than in the field of archaeology. In the present edition this side has been thoroughly revised and rewritten and forms an admirable and indeed entertaining summary of the present position. We cannot but envy for example the opportunities that some of our colleagues across the Atlantic have for doing research. "On the Campus of the University of Alaska is a site which yields the types of chipped implements characteristic of certain sites in the Gobi, Asia." (The discovery was reported in *American Antiquity* in 1937, which indicates that revision has really been carried into line with modern research.) The chapters on racial characters are so suggestive and full of ideas that it is a pity that Dr. Wissler, though it is admittedly outside his regular line, has not thought fit to remove such obvious anomalies as, for example, the statement on page 362 that the Australian aborigines have frizzy or woolly hair and should be classed with Negroes because of this. Boas' statements about bodily change in immigrants are apparently accepted with less reserve than is general. It is natural for the specialist to criticize his own line of country, and no doubt workers in fields which are beyond the author's speciality would have criticisms to make. Despite this, however, we have here a book which has stood the test of time and will be of interest not only to students but to all who find from their boyhood's days the remains of an enthusiasm for the "Redskins," which this book will surely kindle into a very real interest.

L. H. D. B.

Season of Birth. By ELLSWORTH HUNTINGTON. [Pp. viii + 473, with 104 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. 17*s*. 6*d*. net.)

THIS most interesting book is a statistical study and analysis of a vast amount of data regarding the influence of season on the number of births and its relation to the death rate, and longevity, and also to the vigour or eminence attained by persons born at that season.

There is evidence that reproduction is especially stimulated at certain seasons. Like all animals, man has a definite seasonal rhythm of reproduction—an inheritance from ancient time when relatively few children survived unless born at the best season.

There are two climatic optima or sets of most favourable conditions. A physical optimum—an average temperature of 60° F. to 65° for day and night together, and a mental optimum estimated at 39° to 54°. In both, humidity, variability of temperature, and sunlight play a part, but temperature is the dominant factor. But there are cultural and climatic factors which influence these seasonal changes—weather, the favourite season for marriages, in some countries religious fasts and festivals, especially also the demands of certain occupations—the greatest effects being seasonal migration whereby men tend to be away from home during part of the year.

A primitive child born in March has advantages over a child born at other seasons, its mother's ability to feed it will steadily increase for seven months in harmony with the growing needs of the child ; by the time the March child began to need much milk its mother was beginning to be well fed.

Like advantages make it best for lambs to be born in April, and robins to be hatched in May. During the eight months after their calves are born, the cows whose calves arrive in February give more milk than do those whose calves are born at any other season. The February calf, like the February child, is unusually well fed. Further, the child born in February or March has greater chance of surviving through the hot weather and so avoiding the epidemics of that season. Advances in hygiene and medical science have altered the season so that the warm climate of August is healthy and the children survive.

The text is accompanied by numbers of curves which the author regards as more appealing than tables of figures. This may be, but the reader will find the interpretation of many of these curves is no easy task.

The application of the facts described to practical economics is obviously possible. Parents will adjust the dates of conception and births of their children to fit the most favourable months, and so provide their family with the advantages associated with the best seasons.

P. J.

The Origin of Life. By A. I. OPARIN. Translated by SERGIUS MORGULIS. [Pp. xii + 270, with 8 figures.] (New York and London: Macmillan & Co., Ltd., 1938. 8*s*. 6*d*. net.)

How life originated has been the universal enquiry since the remotest time. The first possessors of life were, however, unable to recognise it, and it was only when biology had travelled over incalculable eras, to the complication of life with the power of conception—rightly or wrongly associated by us as synchronous with the appearance of man—that life was first realised as an entity, and the simplest and easiest explanation of it, as of all unknown things and phenomena, was divine—"God created." Even to-day this explanation survives and spontaneous generation was a principle accepted by scientific thought almost into the early years of the present century.

The chief difficulty has always been the inability to conceive "gradualness." Life did not, and does not, arrive suddenly ; it is a slow evolving "process," and as far as man can see life is always derived from life—the intermediate stages of the evolution from non-living inorganic forms have disappeared or become masked.

In this most interesting book Prof. Oparin has attempted to trace the origin of life to material furnished by the gaseous mass which had once separated from the Sun owing to a cosmic catastrophe, whereby carbon and other elements of the Solar atmosphere passed into this gaseous mass which

ultimately was destined to form our Earth. Carbon has exceptional ability to form atomic associations and is found in all living things. In the process of formation of our planet from the incandescent mass of gas, clouds of carbon quickly condensed and entered the primitive nucleus of the Earth, coming in contact with the elements of heavy metals—primarily with iron, an essential component of the central core. As the Earth cooled the carbon reacted with the heavy metals, producing "Carbides."

The Earth's atmosphere at that period contained neither oxygen nor nitrogen but was filled with superheated aqueous vapour. The thin layer of igneous rocks, unable to resist the tides of the inner molten mass caused by the forces of the Sun and Moon, would rupture, and through the crevices the molten liquid from the interior spread over the Earth's surface. The superheated aqueous vapour of the atmosphere in contact with the carbides gave rise to the simplest organic matter, the hydrocarbons, which again originated a great variety of derivatives—through oxidation by the oxygen component of Water.

The advent at this period of ammonia gave rise to amides, amines and other nitrogenous derivatives. So that when our planet cooled sufficiently to allow condensation of aqueous vapour and the formation of the first envelope of hot water round the Earth, this water contained in solution organic substances, the molecules of which were made up of carbon, hydrogen, oxygen, and nitrogen ; substances endowed with great chemical potentialities —by the complex reactions of which the proteins must have originated. These substances at first occurred in seas and oceans as colloidal solutions, and as these solutions of various substances were mixed, new formations "Coazervates" or semi-liquid gels resulted. In these gels the colloidal particles assume a definite position towards each other—the beginning of some elementary structure which confers on each coazervate a degree of individuality, determining its ability to absorb and to incorporate into itself organic substances dissolved in the surrounding water—thus constituting a power to grow and resulting in the increased complexity of the molecules and the development of new properties.

But there is still a long way to travel before we arrive at Life. Yet the author holds that though the road is hard and long and the synthesis of living things very remote, it is "not an unattainable goal along this road."

P. J.

Das Aufbauprinzip der Technik. Von Ingenieur Dr. techn. P. Wessel. [Pp. 39, with 14 figures.] (Munich: Ernst Reinhardt, 1937.)

Suppose that some article is to be manufactured. The author of this short book investigates the principles involved in the chain of processes, beginning with the raw material and ending with the finished article. Seven subsidiary principles are recognised as forming collectively one grand principle of technical production. These are called, respectively, the principles of organisation, selection, dimensioning, invention, grading in "form hardness," "self and mutual" refinement, and reaction. The discussion of each subsidiary principle is illustrated by various practical examples. Such a novel treatment is calculated to excite considerable interest in engineers, applied mathematicians and physicists.

N. D.

Patents, Designs and Trade Marks. By CHARLES S. PARSONS, B.Sc. [Pp. viii + 184.] (London: The Technical Press, Ltd., 1938. 10*s*. 6*d*. net.)

THE book is one of the useful kind which explains clearly the broad principles of the law so far as patents, designs and trade marks are concerned, without being too specialised for the average business man. While a little knowledge is sometimes dangerous, it can equally help the layman to avoid dangers by telling him when to get professional advice. The author has done more than present a clear picture of the facts. He discusses the possible methods by which a business man may legitimately secure both temporary and permanent protection of his ideas, and how he may similarly attempt to expand into a field of manufacture which is apparently protected and gives some illuminating examples of both types.

The chapter on design is perhaps a little disappointing in failing to clear up the position regarding the protection of decorative design as opposed to design of form and shape. There are many industries, of which pottery is an example, where precise information on this point would be valuable.

The author is naturally reluctant to express opinions as to the extent of the protection afforded under our present Patent Laws and on their unsatisfactory nature in some respects, but there is a concise and very useful chapter on the question of foreign patents which is a veritable mine of information to the inexperienced and unwary.

The book should commend itself to business men in a wide variety of industries as being a most useful volume to keep at hand.

HARRY W. WEBB.

The Scientific Journal of the Royal College of Science. Vol. VIII. [Pp. 93, with 21 figures.] (London: Edward Arnold & Co., 1938. 7*s*. 6*d*.)

THE three scientific societies of the Royal College of Science publish annually a volume containing a selection of the papers which have been read before their members during the session. This issue contains those read during the session 1937–38. The Imperial College Chemical Society contributes four papers. Dr. C. F. Goodeve outlines some of the fundamental principles of photochemistry, and discusses the prominent part which they play in the phenomena of vision. Prof. D. M. Newitt describes investigations on the equilibrium of the water-gas reaction at high pressures, and shows how the pressure coefficient of the equilibrium constant may be calculated. Mr. D. F. Cheesman provides a sympathetic study of the motives in which many alchemists found inspiration, whilst Dr. H. D. K. Drew gives an account of the phenomena of chemiluminescence in organic reactions. The section occupied by the Royal College of Science Natural History Society contains a geochemical paper by Prof. A. Brammall and Mr. J. G. C. Leech dealing with base-exchange and its problems, an account by Dr. H. C. Brown of research on the electric charge of micro-organisms, and an explanation by Mr. J. Marshall of the methods of dendrochronology, or the correlation of the ring-growth sequence of trees or timber with the passage of time. The three papers contributed by the Royal College of Science Mathematical and Physical Society are an account by Dr. W. Cochrane of the study of surface form by the use of electron diffraction methods, a paper by Mr. R. C. Pankhurst on the application of physical principles in organ building, and a lecture

on the spark discharge by Dr. T. E. Allibone. These, and the fifteen other contributions which are published in title only, afford ample evidence of the progressive outlook of the students responsible for organising this important phase of academic activity.

A. A. E.

A Musical Slide-Rule. By Ll. S. Lloyd, C.B., M.A. [Pp. 25, with slide-rule in pocket.] (London: Oxford University Press, 1938. 2*s.* net. Slide-rule issued separately, 1*s.* net.)

The slide-rule consists of two pieces of card on each of which is inscribed a logarithmic scale of musical intervals in just intonation. On the reverse the same scale is expressed in vibration numbers. That is to say, on this side the scales look like the scales of an ordinary arithmetical slide-rule, save that the cards have the intervals in mean tone and equal temperament given, one on each card. The slide-rule should be invaluable for showing music students—for whom it is intended—the difference between ideal and compromise in musical temperament and the relations between these matters and the theory of harmony and counterpoint.

E. G. R.

Catalogue of Lewis's Medical and Scientific Lending Library. Part I: Authors and Titles. New edition, revised to the end of 1937. [Pp. 8 + 550.] (London: H. K. Lewis & Co., Ltd., 1938. 16*s.* net. To Subscribers 8*s.*)

All subscribers to Lewis's Library will welcome this new catalogue of medical and scientific books. Its arrangement is similar to previous issues, giving the author's name, the title of the book, format, price and—most important—the year of publication of the last edition. It is unfortunate that it is complete only up to the end of 1937, but this defect can be remedied by reference to Lewis's bi-monthly lists.

Apart from its obvious value to subscribers to the Library, this catalogue will be found useful by librarians, compilers of bibliographies, and all who have to refer to British and American medical and technical books.

Part II, containing a classified index of books under subjects, will be issued in 1939 and thus will complete this handy reference catalogue.

T. C.

BOOKS RECEIVED

(Publishers are requested to notify prices.)

The Geometry of Determinantal Loci. By T. G. Room, M.A., Professor of Mathematics in the University of Sydney. Cambridge: at the University Press, 1938. (Pp. xxviii + 483.) 42*s*. net.

Elementary Mathematical Statistics. By William Dowell Baten, Ph.D., Associate Professor of Mathematics and Research Associate in Statistics of the Michigan Agricultural Experiment Station, Michigan State College. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. (Pp. x + 338, with 49 figures.) 15*s*. net.

Theoretical Mechanics. A Vectorial Treatment. By Carl Jenness Coe, Ph.D., Assistant Professor of Mathematics, The University of Michigan. New York: The Macmillan Company, 1938. (Pp. xiv + 555, with 102 figures.) $5.00.

An Introduction to Vector Analysis for Physicists and Engineers. By B. Hague, D.Sc., Ph.D., F.C.G.I., Lecturer in Electrical Engineering, University of Glasgow. Methuen's Monographs on Physical Subjects. London: Methuen & Co., Ltd., 1939. (Pp. x + 118, with 39 figures.) 3*s*. net.

Cosmology. A Text for Colleges. By J. A. McWilliams, S.J., Ph.D., Professor of Cosmology, St. Louis University. Second revised edition. New York and London: Macmillan & Co., Ltd., 1938. (Pp. x + 243.) 9*s*. net.

Grimsehls Lehrbuch der Physik. Band I: Mechanik—Wärmelehre—Akustik. Neubearbeitet von Professor Dr. R. Tomaschek, Direktor des physikalischen Instituts der technischen Hochschule, Dresden. Tenth edition. Leipzig and Berlin: B. G. Teubner, 1938. (Pp. viii + 681, with 740 figures.) Price abroad, RM.14.85.

Principles of Electricity and Electromagnetism. By Gaylord P. Harnwell, Professor of Physics, University of Pennsylvania. International Series in Physics. New York and London: McGraw-Hill Publishing Co., Ltd., 1938. (Pp. xiv + 619, with 382 figures.) 30*s*. net.

Electron Optics. Theoretical and Practical. By L. M. Myers, Research Department, Marconi's Wireless Telegraphic Co., Ltd. London: Chapman & Hall, Ltd., 1939. (Pp. xviii + 618, with frontispiece and 379 figures, including 68 plates.) 42*s*. net.

The Mobility of Positive Ions in Gases. By A. M. Tyndall, D.Sc., F.R.S., Henry Overton Wills Professor of Physics, University of Bristol. Cambridge Physical Tracts. Cambridge: at the University Press, 1938. (Pp. x + 93, with 35 figures.) 6*s*. net.

The Radio Manual. By George E. Sterling, Assistant Chief, Field Section, Engineering Department, Federal Communications Commission. Third edition. New York: D. van Nostrand Co., Inc.; London: Chapman

& Hall, Ltd., 1938. (Pp. vi + 1120, with frontispiece and 443 figures.) 25*s.* net.

The Basic Mechanics of Human Vision. By R. Brooks Simpkins. London: Chapman & Hall, Ltd., 1939. (Pp. viii + 228, with 8 plates and 79 figures.) 12*s.* 6*d.* net.

Physics for Technical Students. Sound, Electricity and Magnetism, Light. By William Ballantyne Anderson, Ph.D., Professor of Physics, Oregon State College. Third edition. New York and London: McGraw-Hill Publishing Co., Ltd., 1937. (Pp. x + 796, with 534 figures, including 1 plate.) 15*s.* net.

Textbook of Heat. By R. Wallace Stewart, D.Sc., and John Satterly, D.Sc., M.A., F.R.S.C. Second edition, revised by C. T. Archer, M.Sc., A.R.C.S., F.Inst.P., Senior Lecturer in Physics at the Royal College of Science. London: University Tutorial Press, Ltd., 1939. (Pp. viii + 410, with 223 figures.) 7*s.* 6*d.*

Heat. By W. J. Sparrow, M.A., B.Sc., Senior Physics Master at Saltley Secondary School, Birmingham. London: John Murray, 1938. (Pp. viii + 382, with 145 figures.) 8*s.* 6*d.*

Concise School Physics. Complete edition. By R. G. Shackel, M.A., Head of the Physics Department, St. Olave's Grammar School, London. London, New York, Toronto: Longmans, Green & Co., 1938. (Pp. viii + 669, with 491 figures.) 6*s.* 6*d.*

Concise School Physics. Electricity and Magnetism. By R. G. Shackel, M.A., Head of the Physics Department, St. Olave's Grammar School, London. London, New York, Toronto: Longmans, Green & Co., 1938. (Pp. viii + 195, with 155 figures.) 2*s.* 9*d.*

The World of Engineering. By J. L. Dixon, B.Sc., A.M.Inst.C.E., Chief of Civil Engineering Department, British Institute of Engineering Technology. London: The Scientific Book Club, 1939. (Pp. viii + 205, with 4 plates and 86 figures.) 2*s.* 6*d.* net.

Stream and Channel Flow (Hydraulic Graphs and Tables). By E. E. Morgan, M.C., M.I.Struct.E., Assoc.M.Inst.C.E., A.M.Inst.M. & Cy.E., Senior Engineering Assistant to the Surrey County Council. With a Foreword by C. L. Howard Humphreys, T.D., M.Inst.C.E., M.I.Mech.E., M.Inst.W.E., M.Cons.E. London: Chapman & Hall, Ltd., 1938. (Pp. xxii + 240, with 5 figures and 12 graphs.) 25*s.* net.

An Introduction to Electrical Engineering. By E. W. Marchant, D.Sc., F.C.G.I., David Jardine Professor of Electrical Engineering in the University of Liverpool. London: Methuen & Co., Ltd., 1939. (Pp. xiv + 297, with 222 figures.) 12*s.* 6*d.* net.

Gmelins Handbuch der anorganischen Chemie. 8.Auflage. Herausgegeben von der Deutschen Chemischen Gesellschaft. System-Nummer 22: Kalium. Berlin: Verlag Chemie, G.m.b.H., 1938. Lieferung 6 (pp. 156, with 47 figures), price abroad RM.21.75; Lieferung 7 (pp. 108, with 2 figures), price abroad RM.16.50.

Gmelins Handbuch der anorganischen Chemie. 8.Auflage. Herausgegeben von der Deutschen Chemischen Gesellschaft. System-Nummer 64: Rhodium. Berlin: Verlag Chemie, G.m.b.H., 1938. (Pp. viii + 153, with 5 figures.) Price abroad, RM.20.25.

Gmelins Handbuch der anorganischen Chemie. 8.Auflage. Herausgegeben von der Deutschen Chemischen Gesellschaft. System-Nummer 66: Osmium, mit einem Anhang über Ekaosmium. Berlin: Verlag Chemie, G.m.b.H., 1939. (Pp. xxiv + 100.) Price abroad RM.14.25.

Gmelins Handbuch der anorganischen Chemie. 8. Auflage. Herausgegeben von der Deutschen Chemischen Gesellschaft. System-Nummer 68: Platin, Teil A, Lieferung 1. Berlin: Verlag Chemie, G.m.b.H., 1938. (Pp. 145, with 2 figures.) Price abroad RM.16.50.

The Phase Rule and its Applications. By Alexander Findlay, Professor of Chemistry, University of Aberdeen. Revised with the Assistance of A. N. Campbell, Associate Professor of Chemistry, University of Manitoba. Eighth edition. London, New York Toronto: Longmans, Green & Co., 1938. (Pp. xvi + 327, with 163 figures, including 4 plates and 1 folding plate.) 12*s*. 6*d*. net.

A Textbook of Inorganic Chemistry for Colleges. By James F. Norris, Professor of Organic Chemistry, and Ralph C. Young, Assistant Professor of Chemistry, Massachusetts Institute of Technology. Second edition. New York and London: McGraw-Hill Publishing Co., Ltd., 1938. (Pp. x + 803, with 33 figures, including 1 plate.) 21*s*. net.

An Inorganic Chemistry. By H. G. Denham, M.A., D.Sc., Ph.D., Professor of Chemistry, Canterbury College, University of New Zealand. Third edition. London: Edward Arnold & Co., 1939. (Pp. viii + 694, with 149 figures.) 12*s*. 6*d*. net.

Second Year College Chemistry. By William H. Chapin, Emeritus Professor of Chemistry in Oberlin College. Fourth edition, revised by William H. Chapin and Luke E. Steiner, Associate Professor of Chemistry in Oberlin College. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. (Pp. xvi + 407, with 43 figures.) 9*s*. 6*d*. net.

Revision Notes in Inorganic Chemistry to Higher School Certificate. By E. P. Wilson, M.Sc., A.I.C., Senior Science Master, and F. W. Ambler, M.Sc., Chemistry Master, Castleford Grammar School. London: William Heinemann, Ltd., 1938. (Pp. viii + 240, with 29 figures.) 4*s*.

Theoretical Qualitative Analysis. By J. H. Reedy, Associate Professor of Analytical Chemistry, University of Illinois. International Chemical Series. New York and London: McGraw-Hill Publishing Co., Ltd., 1938. (Pp. x + 451, with 34 figures.) 18*s*. net.

The Essentials of Volumetric Analysis. By John Lambert, M.Sc., Assistant Master at King Edward's School, Birmingham, in conjunction with A. Holderness, M.Sc., Senior Chemistry Master at Archbishop Holgate's Grammar School, York, and F. Sherwood Taylor, Ph.D., M.A., B.Sc. London: William Heinemann, Ltd., 1938. (Pp. viii + 92, with 10 figures.) 2*s*. 6*d*.

Laboratory Manual of Organic Chemistry. By Harry L. Fisher, Ph.D., Research Chemist, U.S. Industrial Alcohol Co., Stamford, Conn. Fourth edition. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. (Pp. xxiv + 412, with 21 figures.) 13*s*. 6*d*. net.

Practical Organic Chemistry. By Frederick George Mann, Sc.D., D.Sc., F.I.C., Fellow and Lecturer, Trinity College, Cambridge, University Lecturer in Chemistry, and Bernard Charles Saunders, M.A., Ph.D., B.Sc., Fellow and Charles Kingsley Lecturer, Magdalene College, Cambridge, University Demonstrator in Chemistry. With a Foreword by Sir William J. Pope, K.B.E., F.R.S. Second edition. London: Longmans, Green & Co., Ltd., 1938. (Pp. xiv + 418, with 66 figures.) 8*s*. 6*d*. net.

Systematic Qualitative Organic Analysis. By H. Middleton, M.Sc., A.I.C., Lecturer in Organic Chemistry, Bradford Technical College. London: Edward Arnold & Co., 1939. (Pp. viii + 279, with 12 figures.) 8*s*. 6*d*. net.

Steel and its Heat Treatment. Vol. I: Principles—Processes—Control. By D. K. Bullens and the Metallurgical Staff of the Battelle Memorial Institute. Fourth edition. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. (Pp. xvi + 445, with 208 figures.) 22*s*. 6*d*. net.

Contributions to the Knowledge of the Chemical Composition of the Earth's Crust in the East Indian Archipelago. I: The Spectrographic Determination of the Elements According to Arc Methods in the Range 3600–5000 A. II: On the Occurrence of Rarer Elements in the Netherlands East Indies. By Dr. W. van Tongeren, Assistant in the Department of Mineralogy, State University of Utrecht. Amsterdam: D. B. Centen's Uitgevers-Maatschappij N.V., 1938. English Agents: H. K. Lewis & Co., Ltd., London. (Pp. xii + 181, with 14 figures.) 11*s*. net. Part I can be obtained separately at 7*s*.

The Properties of Glass. By George W. Morey, Member of the Staff of the Geophysical Laboratory, Carnegie Institute of Washington. American Chemical Society Monograph Series. New York: Reinhold Publishing Corporation; London: Chapman & Hall, Ltd., 1938. (Pp. 561, with 152 figures.) 62*s*. 6*d*. net.

Handbook of Chemical Microscopy. Vol. I. By Émile Monnin Chamot, B.S., Ph.D., Professor of Chemistry, Emeritus, and Clyde Walter Mason, A.B., Ph.D., Professor, Chemical Microscopy and Metallography, Cornell University. Second edition. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. (Pp. xvi + 478, with 165 figures.) 22*s*. 6*d*. net.

German Grammar for Chemists and Other Science Students. By John T. Fotos, Professor of Modern Languages, and John L. Bray, Head of the School of Chemical and Metallurgical Engineering, Purdue University. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. (Pp. xxii + 323.) 11*s*. net.

Understanding the Atom. By John Rowland, B.Sc. The New People's Library, Vol. XVII. London: Victor Gollancz, Ltd., 1938. (Pp. 95.) 1*s*. 6*d*. cloth; 1*s*. paper.

Manual of Sedimentary Petrography. By W. C. Krumbein and F. J. Pettijohn, Department of Geology, University of Chicago. The Century Earth Science Series. New York and London: D. Appleton-Century Co., Inc., 1938. (Pp. xiv + 549, with 265 figures.) 30*s*. net.

Field Determination of Rocks. By E. H. Davison, B.Sc., F.G.S., Head of the Department of Geology and Mineralogy, School of Mines, Camborne. London: Chapman & Hall, Ltd., 1938. (Pp. viii + 87, with 10 plates and 4 figures.) 7*s*. 6*d*. net.

Geology of India. By D. N. Wadia, M.A., B.Sc., F.G.S., F.R.G.S., F.R.A.S.B., Geological Survey of India. Second edition. London: Macmillan & Co., Ltd., 1939. (Pp. xx + 460, with 20 plates and 45 figures.) 24*s*. net.

Miocene Stratigraphy of California. By Robert M. Kleinpell. Tulsa, Oklahoma: The American Association of Petroleum Geologists; London: Thomas Murby & Co., 1938. (Pp. x + 450, with 22 plates and 14 figures.) 22*s*. 6*d*. net.

Practical Oil Geology. By Dorsey Hager, Consulting Geologist and Petroleum Engineer. Fifth edition. New York and London: McGraw-Hill Publishing Co., Ltd., 1938. (Pp. xx + 466, with 204 figures.) 24*s*. net.

An Introduction to Botany. With Special Reference to the Structure of the Flowering Plant. By J. H. Priestley, D.S.O., B.Sc., F.L.S., Professor

of Botany, and Lorna I. Scott, M.Sc., Lecturer in Botany, University of Leeds. Illustrated by Marjorie E. Malins and Lorna I. Scott. London, New York, Toronto: Longmans, Green & Co., 1938. (Pp. x + 615, with 170 figures.) 17*s*. 6*d*. net.

An Introduction to Botany. By Arthur W. Haupt, Associate Professor of Botany, University of California at Los Angeles. McGraw-Hill Publications in the Botanical Sciences. New York and London: McGraw-Hill Publishing Co., Ltd., 1938. (Pp. xii + 396, with frontispiece and 278 figures.) 18*s*. net.

Vergleichende Morphologie der höheren Pflanzen. I. Band: Vegetationsorgane, 2.Teil, 1.Lieferung. Von Dr. Wilhelm Troll, o.Professor der Botanik an der Martin-Luther-Universität Halle-Wittenberg. Berlin: Gebrüder Borntraeger, 1938. (Pp. 192, with 168 figures.) RM.16.—(Subscription price RM.12.80).

Plant Physiology. With reference to the green plant. By Edwin C. Miller, Ph.D., Professor of Plant Physiology, Kansas State College of Agriculture and Applied Science, and Plant Physiologist, Kansas Agricultural Experiment Station. Second edition. McGraw-Hill Publications in the Botanical Sciences. (Pp. xxxii + 1201, with 39 figures.) 45*s*. net.

The Physiology of Plants. By William Seifriz, Ph.D., Professor of Botany, University of Pennsylvania, Philadelphia. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. (Pp. viii + 315, with frontispiece and 95 figures, including 1 coloured plate.) 17*s*. 6*d*. net.

The Evolution of Genetic Systems. By C. D. Darlington, Head of the Cytology Department, John Innes Horticultural Institution. Cambridge: at the University Press, 1939. (Pp. xii + 149, with 26 figures.) 10*s*. 6*d*. net.

The Genetics of Garden Plants. By M. B. Crane, Head of the Department of Pomology, and W. J. C. Lawrence, Curator, The John Innes Horticultural Institution, Merton. With a Foreword by Sir Daniel Hall, K.C.B., LL.D., D.Sc., F.R.S. London: Macmillan & Co., Ltd., 1938. (Pp. xxii + 287, with 62 figures.) 12*s*. 6*d*. net.

Commercial Fruit and Vegetable Products. A Textbook for Student, Investigator and Manufacturer. By W. V. Cruess, Professor of Fruit Technology, University of California. Second edition. McGraw-Hill Publications in the Agricultural Sciences. New York and London: McGraw-Hill Publishing Co., Ltd., 1938. (Pp. x + 798, with 109 figures.) 36*s*. net.

Introduction to the Botany of Field Crops. Vol. I: Cereals; Vol. II: Non-Cereals. By J. M. Hector, Professor of Agricultural Botany, University of Pretoria. Johannesburg: Central News Agency, Ltd., 1938. London agents: Gordon & Gotch, Ltd. (Vol. I: pp. xii + 500; Vol. II: pp. viii + 675.) £3 10*s*. net per set.

An Introduction to American Forestry. By Shirley Walter Allen, Professor of Forestry, School of Forestry and Conservation, University of Michigan. American Forestry Series. New York and London: McGraw-Hill Publishing Co., Ltd., 1938. (Pp. viii + 402, with 122 figures.) 21*s*. net.

Weeds, Weeds, Weeds. By Sir Charles Vernon Boys, LL.D., F.R.S. Second edition. London: Wightman & Co., Ltd., 1938. (Pp. 113.) 2*s*. net.

Textbook of General Zoology. By Winterton C. Curtis and Mary J. Guthrie, Professors of Zoology, University of Missouri. Third edition. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. (Pp. xviii + 682, with 486 figures.) 18*s*. 6*d*. net.

An Introduction to Vertebrate Anatomy. By Harold Madison Messer, Long Island University. New York and London: Macmillan & Co., Ltd., 1938. (Pp. xviii + 406, with 374 figures.) 16*s*. net.

Outlines of Evolutionary Biology. By the late Arthur Dendy, D.Sc., F.R.S., Professor of Zoology in the University of London (King's College). Fourth edition, re-issued with a foreword by Maurice Burton, D.Sc. London: Constable & Co., Ltd., 1938. (Pp. xlii + 481, with 190 figures.) 16*s*. net.

An Introduction to the Vertebrates. By Leverett Allen Adams, Associate Professor of Zoology, University of Illinois. Second edition. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. (Pp. viii + 479, with 327 figures.) 17*s*. 6*d*. net.

Paläobiologie. Bewegung, Umwelt und Gestalt fossiler Tiere. Von Dr. Carl Christoph Beringer. Stuttgart: Ferdinand Enke Verlag, 1939. (Pp. viii + 61, with 60 figures.) RM.4.40, paper covers.

Scoliodon (The Shark of the Indian Seas). By. E. M. Thillayampalam, M.Sc., M.A., Ph.D., Vice-Principal, Chundikuli Girls' College, Jaffna, Ceylon. The Indian Zoological Memoirs on Indian Animal Types, No. II. Second edition. Lucknow: Lucknow Publishing House, 1938. (Pp. xiv + 126, with 94 figures.) Rs.2/8.

Bacillus Salmonicida Bacteriophage: with particular reference to its occurrence in water and the question of its application in controlling B. Salmonicida Infection. By M. H. Christison, I. Mackenzie and T. J. Mackie (from the Bacteriology Department, Edinburgh University). Fishery Board for Scotland, Salmon Fisheries, 1938, No. V. Edinburgh: H.M. Stationery Office, 1938. (Pp. 18.) 1*s*. net.

The Absorption of Bones in the Skull of Salmon during their Migration to Rivers. By Dr. V. Tchernavin, Department of Zoology of Edinburgh University. Fishery Board for Scotland, Salmon Fisheries, 1938, No. VI. Edinburgh: H.M. Stationery Office, 1938. (Pp. 4, with 1 plate.) 6*d*. net.

The Movements of Salmon marked in the Sea. III: The Island of Soay and Ardnamurchan in 1938. By W. J. M. Menzies, F.R.S.E., Inspector of Salmon Fisheries of Scotland. Fishery Board for Scotland, Salmon Fisheries, 1938, No. VII. Edinburgh: H.M. Stationery Office, 1938. (Pp. 8, with 4 folding plates.) 1*s*. 3*d*. net.

The California Woodpecker and I. By William Emerson Ritter. U.S.A.: University of California Press; Great Britain and Ireland: Cambridge University Press, 1938. (Pp. xvi + 340, with coloured frontispiece and 29 figures, including 1 plate.) 16*s*. net.

Special Pathology and Therapeutics of the Diseases of Domestic Animals. Vols. I, II and III. By Dr. Franz Hutyra, late Professor of Infectious Diseases at the Ancient Royal Veterinary College, Budapest, Dr. Joseph Marek, late Professor of Special Pathology and Therapy at the Royal Hungarian Palatin-Joseph University, Budapest, and Dr. Rudolph Manninger, Professor of Infectious Diseases at the Royal Hungarian Palatin-Joseph University, Budapest. Fourth English edition, edited by J. Russell Greig, Ph.D., M.R.C.V.S., F.R.S.E., Director, Moredun Research Institute, Edinburgh, with the collaboration of J. R. Mohler, V.M.D., D.Sc., and Adolph Eichhorn, D.V.S. London: Baillière, Tindall & Cox, 1938. (Vol. I: pp. xvi + 962, with 16 coloured plates and 317 figures; Vol. II: pp. xii + 704, with 186 figures; Vol. III: pp. xii + 763, with 7 coloured plates and 221 figures.) £6 6*s*. net.

Vitamins and Vitamin Deficiencies. Vol. I: Introductory and Historical; Vitamin B_1 and Beri-Beri. By Leslie J. Harris, Ph.D., Sc.D., D.Sc., F.I.C., Nutritional Laboratory, Medical Research Council and University of Cambridge. With a Foreword by Sir Frederick Gowland Hopkins, O.M., F.R.C.P., F.R.S. London: J. & A. Churchill, Ltd, 1938. (Pp. xiv + 204, with 50 figures.) *8s. 6d.* net.

Nutrition and Diet Therapy. A Textbook of Dietetics. By Fairfax T. Proudfit, Instructor in Nutrition and Diet Therapy, University of Tennessee College of Medicine and School of Nursing. Seventh edition. New York and London: Macmillan & Co., Ltd., 1938. (Pp. viii + 923, with 64 figures, including 3 coloured plates.) *14s.* net.

The Health of the Nation and Deficiency Diseases. By John Maberly, M.R.C.S., L.R.C.P. London: Baillière, Tindall & Cox, 1938. (Pp. xii + 118, with frontispiece.) *5s.* net.

Laboratory Outlines in Comparative Physiology. By Charles Gardner Rogers, Ph.D., Sc.D., Professor of Comparative Physiology in Oberlin College. Second edition. McGraw-Hill Publications in the Zoological Sciences. New York and London: McGraw-Hill Publishing Co., Ltd., 1938. (Pp. x + 133.) *8s. 6d.* net.

A Textbook of Biochemistry. By Roger J. Williams, Ph.D., D.Sc., Professor of Chemistry, Oregon State College. New York: D. van Nostrand Co., Inc.; London: Chapman & Hall, Ltd., 1938. (Pp. x + 525, with 19 figures.) *21s.* net.

Laboratory Methods of Biochemistry. By A. Bertho and W. Grassmann. Translated by W. McCartney, Ph.D., F.I.C., Imperial Bureau of Animal Nutrition, Rowett Institute, Aberdeen. London: Macmillan & Co., Ltd., 1938. (Pp. xiv + 281, with 33 figures and 1 folding plate.) *18s.* net.

Bacterial Metabolism. By Marjory Stephenson, Sc.D., Associate of Newnham College, Cambridge, Member of the Scientific Staff of the Medical Research Council. Second edition. London, New York, Toronto: Longmans, Green & Co., 1939. (Pp. xiv + 391, with 53 figures.) *21s.* net.

A Text-Book of Pharmacognosy. By George Edward Trease, B.Pharm., Ph.C., A.I.C., F.L.S., Lecturer on Pharmacognosy in the University College of Nottingham. Third edition. London: Baillière, Tindall & Cox, 1938. (Pp. x + 739, with 233 figures and 9 maps.) *21s.* net.

The Microscopical Study of Drugs. By Lilian A. Kay, B.Pharm., Ph.C., Lecturer in Pharmacognosy, Leicester College of Technology. London: Baillière, Tindall & Cox, 1938. (Pp. viii + 228, with 47 plates and 10 figures.) *10s. 6d.* net.

Karl Pearson. An Appreciation of Some Aspects of His Life and Work. By E. S. Pearson. Cambridge: at the University Press, 1938. (Pp. viii + 170, with 9 plates.) *10s. 6d.* net.

A Short History of the Steam Engine. By H. W. Dickinson. Cambridge: at the University Press, 1939. (Pp. xviii + 255, with 11 plates and 78 figures.) *15s.* net.

General Cartography. By Erwin Raisz, Instructor in Cartography, Institute of Geographical Exploration, Harvard University. McGraw-Hill Series in Geography. New York and London: McGraw-Hill Publishing Co., Ltd., 1938. (Pp. x + 370, with 200 figures.) *24s.* net.

Scientific Results of Cambridge Expeditions to Iceland, 1932–38. Iceland Papers, Vol. I. London: Oxford University Press, 1939. (Pp. viii + 236, with numerous illustrations.) *21s.* net.

Wild Country. A Highland Naturalist's Notes and Pictures. By F. Fraser Darling. Cambridge: at the University Press, 1938. (Pp. viii + 104, with 82 illustrations.) 10*s*. 6*d*. net.

The Australian Aborignes. How to Understand Them. By A. P. Elkin, Professor of Anthropology, University of Sydney. Sydney and London: Angus & Robertson, Ltd., 1938. (Pp. xvi + 262, with 21 plates and 1 folding plate.) 8*s*. 6*d*.

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